
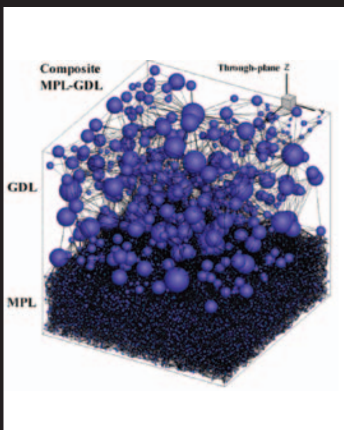

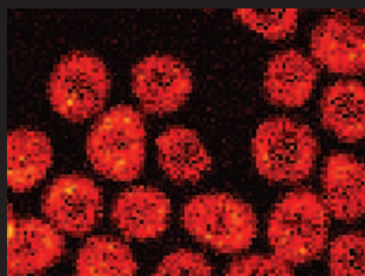
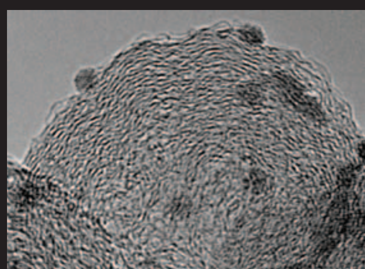
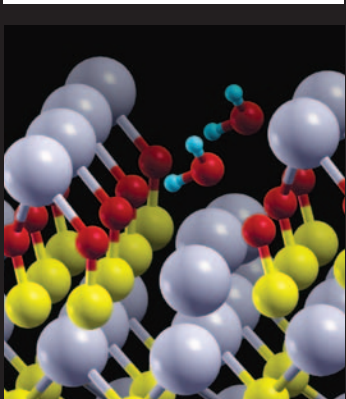
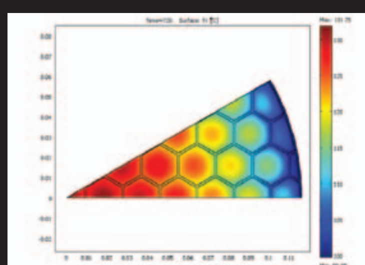

U.S. DEPARTMENT OF
ENERGY

June 7-11, 2010
Washington, DC

Hydrogen Program

2010 Annual Merit Review and Peer Evaluation Report

DOE/GO-102010-3144
December 2010

About the Cover

Photo collage (from top to bottom, left to right):

Portable fuel-cell-powered charger. Photo courtesy of MTI Micro Fuel Cells (NREL PIX 17982.)

Webline manufacturing of fuel cell membranes. Photo courtesy of BASF Fuel Cell, Inc. (NREL PIX 17983.)

Pore network model of microporous and gas diffusion layers (MPL/GDL) of a fuel cell anode/cathode. Image courtesy of Sandia National Laboratories.

DF-STEM image of platinum-gold fuel cell catalysts. Image courtesy of Oak Ridge National Laboratory.

STEM image of platinum on carbon supports. Image courtesy of Oak Ridge National Laboratory.

New ab-initio modeling of the semiconductor/liquid interface for photoelectrochemical solar water-splitting. *Image courtesy of Lawrence Livermore National Laboratories.*

Thermal model during charging of a section of a hydrogen storage bed using a metallic honeycomb structure. Image courtesy of Savannah River National Laboratory.

High-pressure hydrogen refueler tank truck.   Photo courtesy of Energetics, Inc. (NREL PIX 17985).

Back-up power from fuel cells.   Photo courtesy of Reli0n (NREL PIX 17984).

Photo on right:

Washington Monument, Washington, DC. Photo by Ned Stetson, U.S. Department of Energy (NREL PIX 17980).

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PROLOGUE

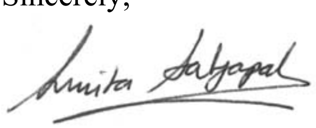
Dear Colleague:

This document summarizes the comments provided by peer reviewers on hydrogen and fuel cell projects presented at the FY 2010 U.S. Department of Energy (DOE) Hydrogen Program and Vehicle Technologies Program Annual Merit Review and Peer Evaluation Meeting (AMR), held June 7–11 in Washington, D.C. In response to direction from various stakeholders, including the National Academies, this review process provides evaluations of the Program's projects in applied research, development, demonstration, and analysis of hydrogen, fuel cells, and infrastructure technologies. The plenary session included overview presentations from the Office of Energy Efficiency & Renewable Energy (EERE), the Hydrogen Program, the Vehicle Technologies Program, and the Office of Basic Energy Sciences. In addition, the plenary session included information about American Recovery and Reinvestment Act projects and commentary from project principal investigators (PIs).

The recommendations of the reviewers are taken into consideration by DOE technology development managers in generating future work plans. The table below lists the projects presented at the review, evaluation scores, and the major actions to be taken during the upcoming fiscal year (October 1, 2010, to September 30, 2011). The projects have been grouped according to sub-program (Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Technology Validation; Safety, Codes & Standards; Education; and Systems Analysis) and reviewed according to the five evaluation criteria. For the first time, a session was dedicated to projects initiated under the American Recovery and Reinvestment Act (ARRA), with projects reviewed according to an appropriate set of criteria. The weighted scores for all projects are based on a four-point scale. To furnish PIs with direct feedback, all evaluations and comments are provided to each presenter; however, the authors of the individual comments remain anonymous. The PI of each project is instructed by DOE to fully consider these summary evaluation comments, as appropriate, in their fiscal year (FY) 2011 plans.

In addition to thanking all participants of the AMR, I would like to express my sincere appreciation to the reviewers. You make this report possible, and we rely on your comments, along with other management processes, to help make project decisions for the new fiscal year. We look forward to your participation in the FY 2011 Annual Merit Review, which is presently scheduled for May 9–13, 2011, at the Crystal Gateway Marriott and the Crystal City Marriott in Arlington, Virginia. Thank you for participating in the FY 2010 AMR.

Sincerely,



Sunita Satyapal
Program Manager
Hydrogen Program
Fuel Cell Technologies Program
Office of Energy Efficiency and Renewable Energy
U.S. Department of Energy

Hydrogen Production and Delivery

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-02	Biomass-derived Liquids Distributed (Aqueous Phase) Reforming *Yong Wang; Pacific Northwest National Laboratory (PNNL)*	2.8		X		The reviewers noted that much progress has been made in characterization of catalysts, addressing conversion and selectivity barriers and in understanding of reaction pathways. Selectivity improvements should be emphasized in the work scope. Reviewers also recommended that the project address screening of other catalyst variables, catalyst synthesis and long term testing of catalysts. Recommendations include identifying future R&D plans and focus (e.g., either syngas for SOFCs or hydrogen production) and strengthening collaborations with other researchers and potential end-users.
PD-03	Hydrogen from Glycerol: A Feasibility Study *Shabbir Ahmed; Argonne National Laboratory (ANL)*	3.1			X	Reviewers noted that good progress was made in establishing the range of associated costs for glycerol reforming needed to achieve DOE goals for production. Although the project was completed in FY 2010, the reviewers identified additional areas for further investigation of this topic if work continues at a later date. These include incorporation of new and emerging reactions (e.g., a water gas shift membrane reactor) and separation technologies into the flow chart to examine their economic validity, and investigation of glycerol as a fuel additive for fuel-flexible reformers.
PD-04	Distributed Bio-Oil Reforming *Stefan Czernik; National Renewable Energy Laboratory (NREL)*	3.0	X			The reviewers pointed out that there are still a number of issues and topics that remain to be addressed at bench scale, including the investigation of bio-oils of different qualities and types and an alternative means for bio-oil stabilization other than alcohols. Also, long term bench scale testing is needed to observe and characterize poisoning effects.
PD-05	High-Performance, Durable Palladium Alloy Membrane for Hydrogen Separation and Purification *Ashok Damle; Pall Corporation*	3.0	X			According to reviewers, good progress has been made in membrane development and fabrication, in long-term evaluation of the hydrogen flux in WGS environments, in determining effects of impurities, and in characterizing the effect of hydrogen recovery on costs. Reviewers recommended that the project should consider the impact of pressure loss and include a comparison with PSA technology in the techno-economic analysis. A test protocol for achieving simultaneous target performance levels should be established and documented, and extensive testing of the membranes in the presence of hydrogen sulfide is needed.
PD-06	A Novel Slurry Based Biomass Reforming Process *Thomas Vanderspurt; United Technologies Research Center (UTRC)*	3.3			X	According to reviewers, future funding in this area should depend on results of economic evaluation. A cost comparison with PSA technology was recommended. Next steps should include investigation of effects of variations in feedstock composition and impurities on catalyst performance, use of non-wood flower feedstock, and scale-up to system pilot testing.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-07	Composite Pd and Alloy Porous Stainless Steel Membranes for Hydrogen Production and Process Intensification (Office of Fossil Energy) *Yi Hua (Ed) Ma; Worcester Polytechnic Institute*	2.9	X			Reviewers commented that the technical work plan for the project is well-designed, comprehensive, and considers all of the relevant issues to test the feasibility of the concept at the laboratory scale. Reviewers expressed some concerns with the membranes' lack of tolerance to sulfur and carbon monoxide impurities and suggested that future work focus on economics and partnerships with industrial developers and/or a coal gasification facility to perform tests at larger scale and under real-world conditions.
PD-08	Development of Robust Hydrogen Separation Membranes (Office of Fossil Energy) *Bryan D. Morreale; National Energy Technology Laboratory (NETL)-Office of Research and Development*	2.9	X			According to reviewers, the project team has a good understanding of engineering principles, membrane technology, and conversion processes. The capability to conduct unbiased verification testing of membranes was considered a strength. Additionally, the project team has developed a good understanding of sulfur poisoning of palladium and palladium-copper alloys, which should provide important clues to increasing sulfur tolerance. Reviewers commented that the project may benefit from a "user industry" team to obtain direct commercial guidance and to gain consensus on process and process economic considerations, and they recommend the addition of a partner from the coal gasification industry.
PD-09	Scale-Up of Hydrogen Transport Membranes for IGCC and FutureGen Plants (Office of Fossil Energy) *Carl Evenson; Eltron Research Inc.*	3.1	X			Reviewers commented that some parts of this project were difficult to evaluate due to intellectual property concerns. Otherwise, reviewers viewed the teaming with an industrial partner (Eastman Chemicals) as a strength, and considered the proposed future work plan systematic and logical. It was suggested that additional laboratory work be performed to better understand performance losses prior to the testing with Eastman. Additionally, a better economic assessment of membrane module costs was recommended, since the module cost may be significantly more than the materials cost.
PD-10	Amorphous Alloy Membranes for High Temperature Hydrogen Separations (Office of Fossil Energy) *Kent Coulter; Southwest Research Institute®*	2.8	X			Reviewers thought the project was innovative and exploratory, and the systematic approach using combinational screening techniques and computational modeling could lead to some very useful results. It was recommended that the project first demonstrate the feasibility of developing a stable glassy metal alloy, and then offer a compelling plan to achieve success with respect to applying a catalytically active protective coating. It was recognized by the reviewers that the project will eventually perform tests at the Western Research Institute gasification facility but an industrial partner should also be considered. An additional task to perform a preliminary economic analysis was suggested.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-11	Experimental Demonstration of Advanced Palladium Membrane Separators for Central High-Purity Hydrogen Production (Office of Fossil Energy) *Sean Emerson; UTRC*	2.6			X	Overall, most reviewers thought the approach was reasonable and that the ternary alloys in the body-centered cubic (BCC) phase warranted further investigation. However, the reviewers felt that significant additional work was needed at the laboratory scale in fabricating defect-free membranes to overcome deficiencies in achieving flux, durability, and stability. Reviewers recommended that the manufacturability issue be resolved and that the project team select a ternary metal that avoids oxide segregation.
PD-12	Supported Molten-Metal Membrane (SMMM) for Hydrogen Separation (Office of Fossil Energy) *Ravindra Datta; Worcester Polytechnic Institute*	2.3		X		In general, the concept and approach were considered innovative by reviewers. However, it was felt that significant additional efforts were needed in the prioritization of key challenges and in establishing focus and direction. Reviewers recommended that the project team seek collaborators such as a metals or materials expert and others that could assist with modularization, economics, and commercialization aspects of SMMMs. Due to the low score, project scope will be modified.
PD-13	R&D Status for the Cu-Cl Thermochemical Cycle-2010 *Michele Lewis; ANL*	2.9	X			According to reviewers, good progress has been made and the team has established excellent collaborations with domestic and international partners. Reviewers commented that the primary focus should be on solving critical path issues before scale-up is attempted. Key challenges identified include materials compatibility, corrosion, copper cross-over, and electrolyzer development for improved efficiency. They also identified the membrane as a critical challenge and suggested that the project team consider including membrane development in their research plan.
PD-14	Hydrogen Delivery Infrastructure Analysis *Marianne Mintz; ANL*	3.5	X			The reviewers commented that this project successfully takes into account most of the important variables involved in determining delivery cost and is therefore a good assessment for focusing research. It was suggested that more short-term projection and industry input would be beneficial.
PD-15	H2A Delivery Analysis and H2A Delivery Components Model *Olga Sozinova; NREL*	3.4	X			The reviewers noted that the project is very inclusive and thorough, and they commended the team on their competency and ability to analyze diverse delivery pathways. It was suggested that the model needs more accurate and representative data for calibration and for more reliable predictions. The project team should also clarify the current hydrogen delivery baseline cost.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-16	Oil-Free Centrifugal Hydrogen Compression Technology Demonstration *Hooshang Heshmat; Mohawk Innovative Technologies*	3.0	X			The reviewers praised the team for their approach and noted that the project is novel, yet solid in its design and in covering all variables. The reviewers recommended that further work is needed to provide single-stage testing data and noted that performance projections based on the current design may be overly optimistic. Furthermore, it was suggested that the project should currently be at a more advanced stage. For example, single-stage compressors should already be in the manufacturing stage.
PD-17	Development of a Centrifugal Hydrogen Pipeline Gas Compressor *Frank Di Bella; Concepts NREC*	3.3	X			The reviewers noted that results appear to be reliable and that an effective design for large-scale hydrogen production has been achieved for meeting most of the DOE targets. According to reviewers, although an excellent mix of industry, academic, and national laboratory partners has been established, the project is missing some important industry input and relies too heavily on assumptions from project collaborators.
PD-18	Advanced Hydrogen Liquefaction Process *Joe Schwartz; Praxair*	2.4		X		The reviewers noted that Praxair has extensive experience in hydrogen liquefaction, currently the highest-density delivery option. However, reviewers felt that important details were lacking throughout the project, especially in the verification of process efficiency numbers. Furthermore, it was observed that the project does not appear to be able to meet the objectives of the DOE EERE mission due to cost considerations.
PD-19	Active Magnetic Regenerative Liquefier *John Barclay; Prometheus Energy*	2.7		X		The reviewers noted that the project is based on a sound plan and has good potential to dramatically reduce energy use for liquefaction. The sound theoretical background used to support the development of the active magnetic regenerative liquefier (AMRL) technology was commended. However, it was strongly recommended that the project provide test data for a complete system.
PD-20	Inexpensive Delivery of Cold Hydrogen in Glass Fiber Composite Pressure Vessels *Andrew Weisberg; Lawrence Livermore National Laboratory (LLNL)*	3.2	X			The project objective is to demonstrate a novel application of materials and composites to pressure vessels for the delivery of cold hydrogen. The reviewers called the project plans and processes sound and praised the team's strong technical knowledge and coordination with industrial partners. It was felt that the project team has demonstrated continual improvements of the technology and evolution from concept to real devices. It was observed, however, that more work is needed in assuring the safety of the technology and that the expected life cycle of the full-scale vessels needs to be addressed further.

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PD-21	Development of High Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery *Don Baldwin; Lincoln Composites*	2.9	X			The reviewers noted the team's competency in designing, manufacturing, and qualifying hydrogen vessel technology, and recognized the near-term application of the tank-trucks. However, it was recommended that cost-performance tradeoff studies should be done prior to a demonstration, and it was suggested that the project would benefit from the establishment of collaborations in key areas. It was specifically recommended that failure modes and effects analysis and other robustness tools be included in the project scope.
PD-22	Fiber Reinforced Composite Pipelines *Thad Adams; Savannah River National Laboratory (SRNL)*	2.9	X			Reviewers noted that the project has developed strong collaborations with manufacturers and with ASME. It was noted that the strong practical approach for evaluating FRPs for hydrogen piping should yield new insight into this potential substitute for hydrogen service. However, it was felt that many questions remain unanswered at this late stage of the four-year project period. It was noted that the project should be addressing the cost of FRP, cyclic pressure responses, joints, and effects of trenchless installation.
PD-23	A Combined Materials Science/Mechanics Approach to the Study of Hydrogen Embrittlement of Pipeline Steels *Petros Sofronis; University of Illinois*	3.2	X			The reviewers felt that the project successfully provides the basic knowledge required for understanding the effect of embrittlement on hydrogen infrastructure. It was specifically noted that the project's approach for subcritical crack growth experiments using sound engineering practices is important in the development of a useful thermodynamic model of de-cohesion. However, it was noted that more work is needed to define real-world problems, and that slow progress has been made on the actual determination of failure modes, failure rates, and the dependence on various operating conditions.
PD-24	Composite Technology for Hydrogen Pipelines *Barton Smith; Oak Ridge National Laboratory (ORNL)*	2.9	X			The reviewers noted that the project is developing useful data for evaluating the status and potential of composite pipelines for hydrogen, and that relevant materials and information for cost modeling are being provided by project collaborators (it was specifically highlighted that the team uses a good and balanced approach in utilizing existing oil and gas industry experience in composite pipelines). It was felt, however, that additional work is needed in expanding the scope of the project. The project may benefit from studies of long-term pressure and temperature cycling of the composite pipe, as well as investigations on connectors and well-health system monitoring tools.

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PD-25	Hydrogen Embrittlement of Structural Steels *Brian Somerday; Sandia National Laboratories (SNL)*	3.3	X			The reviewers commended the collaborations with NIST, component manufacturers, and ASME as strengths in this project. It was specifically pointed out that the project has developed a sound understanding for material effects of hydrogen toward the long-term safety and integrity of steels. As a recommendation, reviewers noted that further examination of pressure cycling effects on steel pipelines in hydrogen operation is needed.
PD-26	Innovative Hydrogen Liquefaction Cycle *Martin Shimko; Gas Equipment Engineering Corporation*	3.3	X			The reviewers commended the project for successfully demonstrating the potential of using modifications to existing technology (such as helium expanders) to significantly reduce the energy and cost for hydrogen liquefaction, while improving reliability. Continuous catalytic heat exchangers, however, were considered a possible project weakness. It was recommended that the team include evaluations for both the capital cost of the system and the resulting liquid hydrogen cost (per kg).
PD-27	Solar High-Temperature Water Splitting Cycle with Quantum Boost *Robin Taylor; SAIC/FSEC*	3.1	X			According to reviewers, the project is well-planned, well-organized, and focused on the key deliverables. Good progress has been made in lowering the electrolytic cell potential. The complexity of the cycle, potentially high capital and maintenance costs, and the need for design for 24/7 operation remain issues. Reviewers suggested that future work include durability testing and thermal cycling, and that attention should be given to the sulfur trioxide decomposition step, particularly on discerning the need for its integration into the overall process.
PD-28	Solar-Thermal ALD Ferrite-Based Water Splitting Cycles *Al Weimer; University of Colorado*	2.9	X			As noted by reviewers, the project team has demonstrated rapid-cycle hydrogen generation and good material durability. The project team has done a good job in addressing AMR 2009 reviewer concerns on earlier project weaknesses. Reviewers suggested extending the economic analyses of ferrite-based water splitting cycles to include materials fabrication costs, the use of solar flux to heat the carrier materials and more realistic operating and maintenance costs for the cycle. A longer-term cycling test added to the R&D plan would be valuable to demonstrate long cycle life of the materials.
PD-29	High-Capacity, High Pressure Electrolysis System with Renewable Power Sources *Martin Shimko; Avalence LLC*	2.9	X			According to reviewers, progress is being made, including a successful demonstration of 6500-psi operation in single-cell testing. Reviewers were not convinced that this approach would reduce system cost. A cost study is needed when the multi-cell high pressure system is designed. Additionally, there are concerns with the gas purity and potential resulting safety risks on a larger system. Remaining issues with the membrane also need to be resolved.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-30	PEM Electrolyzer Incorporating an Advanced Low Cost Membrane *Monjid Hamdan; Giner, Inc.*	3.4	X			According to reviewers, the project has demonstrated good progress toward the goals and has a defined a clear path forward for scale-up. The highly effective and coordinated team approach was specifically highlighted. Also commended were the titanium separator work and the life test studies and accelerated testing experiments. It was recommended by reviewers that longer-term testing be included to determine lifetime/failure modes, and suggested that high-pressure operations could be explored.
PD-31	Renewable Electrolysis Integrated System Development and Testing *Kevin Harrison; NREL*	3.2	X			Reviewers noted that the real-world demonstration of the closely coupled system is informing the technology by generating invaluable data including long-duration, unattended system operations. The analysis work was viewed as appropriate and informative and the collaborations are considered strong. It was felt, though, that project focus could be improved. It was also recommended that the project continue independent testing of electrolyzer systems and further expand its analysis scenario.
PD-32	Photoelectrochemical Hydrogen Production: DOE PEC Working Group Overview *Eric L. Miller; University of Hawaii at Manoa*	3.2	X			Reviewers note that PEC technology is highly relevant to DOE production targets, but is at a relatively low technology readiness level. The extensive Working Group collaborations among project teams are a notable strength, and the projects have demonstrated significant recent progress in a range of materials systems under investigation. Reviewers noted that more work is needed in evaluating a broader class of materials systems, and more effort is needed in advancing technology readiness and in demonstrating technology viability and scalability to large-scale systems.
PD-33	Nanostructured MoS2 and WS2 for the Solar Production of Hydrogen *Thomas Jaramillo; Stanford University*	3.4	X			Reviewers see this project as an excellent example of nanoscience applied to the problem of PEC hydrogen production. Specific research strengths include the focus of approach, as well as the good scientific methods developed in evaluating nanostructured materials properties and viability. Reviewers noted that additional work is needed in the broader evaluation of technology viability and scalability to manufacturing systems.
PD-34	Development and Optimization of Cost Effective Material Systems for Photoelectrochemical Hydrogen Production *Eric McFarland; University of California, Santa Barbara*	3.0	X			Reviewers noted that this project did well in its out-of-the-box technical approaches to the material stability issue. Strengths include characterization and synthesis abilities in formulating and creating targeted compositions, high-throughput screening capabilities enabling rapid screening of compositional variations, and collaborative efforts with the DOE PEC Working Group. It was suggested, though, that further work is needed in evaluating specific passivation schemes, formulating cost projections, and addressing technology viability and scalability.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-35	Semiconductor Materials for Photoelectrolysis *John Turner; NREL*	3.1	X			Reviewers commented favorably on the project's success in bringing leaders in the PEC community into the DOE program. Specific project strengths cited include the efforts in establishing efficiency standards, the work in addressing important stabilization strategies, and the focus on evaluating the potential viability of given materials systems. According to reviewers, further work is needed in addressing technology viability and scalability issues. More emphasis is also needed to accelerate the anticipated synergism between PEC Working Group theorists and experimentalists for facilitating the development of viable PEC materials and systems.
PD-36	Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures *Tasios Melis; University of California, Berkeley*	3.4	X			Reviewers highlighted the systematic manner in which mutants have been developed, characterized and shown to behave in predictable ways as a particular strength of this impressive work. It was commented that in addition to its relevance in photobiological hydrogen production, the results of this study will have very clear relevance in considerations of algal systems for other forms of bioenergy production. Establishing partnerships with groups more focused on photobioreactor design and evaluation was a specific reviewer recommendation.
PD-37	Biological Systems for Hydrogen Photoproduction *Maria Ghiradi; NREL*	3.2	X			Reviewers noted that the project team has demonstrated a strong record of accomplishments, publications, invitations, and scholarship, and is doing a good job in exploring the fundamental understanding of hydrogenase structure and function, particularly with regard to oxygen tolerance. Concern was expressed, however, that the project's tasks and subtasks, although individually interesting and appropriate, in many cases seem scattered, with a wide range of goals and objectives. Project focus and prioritization efforts are recommended by reviewers.
PD-38	Fermentation and Electrohydrogenic Approaches to Hydrogen Production *Pin-Ching Maness; NREL*	3.6	X			The good combination of novel fermentation with microbial electrolysis cell (MEC) technology was considered a project strength, and the project results in terms of molar yields were considered quite impressive. It was also felt that good progress has been made towards improving hydrogen per hexose ratios. Reviewers expressed some concern that this approach requires a relatively expensive feedstock and that maintaining the feed mixtures could be a significant challenge. Additional work is needed in the economic assessment of the proposed process.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-39	Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacterial System *Qing Xu; J. Craig Venter Institute*	3.4	X			Reviewers observed that the project team has strong expertise in metabolic engineering of this type, and that the team's ability to effectively leverage metagenomic libraries is a significant advantage. It was noted that the project has made consistent progress in transferring oxygen-tolerant hydrogenases from several sources into cyanobacteria and demonstrating activity. Reviewers expressed some concerns that the parallel nature of the collaborations seems to be a weakness. A clearer demonstration of the integrative effort was recommended. It was also suggested that localization of introduced hydrogenases within the cell could prove very informative.
PD-42	Catalytic Solubilization and Conversion of Lignocellulosic Feedstocks *Troy Semelsberger;Los Alamos National Laboratory (LANL)*	2.8		X		Reviewers noted that the proposed low-temperature catalytic aqueous phase reforming (APR) system in this study is extremely challenging but, if successful, would be a viable alternative to the traditional pyrolysis or high-temperature gasification methods. The reviewers expressed concerns that limited results have been achieved to date despite the team's solid vision. Revisiting some of the original tests and checking for an insoluble phase in the product was specifically suggested. Expanding collaborative efforts was a further recommendation.
PD-45	Distributed Reforming of Renewable Liquids using Oxygen Transport Membranes *Balu Balachandran; ANL*	2.7		X		Reviewers noted the strong focus on improving the ethanol reforming process in the work. Additional strengths include the project team's expertise and experience in synthesizing and testing oxygen transport membrane materials. It was felt by reviewers, however, that substantial additional work is needed in establishing clear project objectives, targets, and milestones; in clearly demonstrating the advantage of oxidative reforming over steam reforming; and in performing engineering analyses to verify the viability of an industrial process.
PD-46	Reversible Liquid Carriers for an Integrated Production, Storage and Delivery of Hydrogen *Alan Cooper; Air Products*	2.8		X		The reviewers noted that the concept is sound and incorporates good safety aspects. The delivery simplification of hydrogen-to-vehicle from a production facility was also commended. It was felt, however, that more work is needed in defining the infrastructure details required to implement the project. Reviewers suggested that the project would benefit from more OEM involvement.
PD-47	Materials Solutions for Hydrogen Delivery in Pipelines *Doug Stalheim; SECAT*	3.5	X			The reviewers commended the project for its clear, direct applied-science approach, and they emphasized the invaluable insight into the effect of hydrogen on pipeline steels provided by the work. Additional work is recommended to further consider the effects of hydrogen at weld points, as these are often the weakest points of a pipeline. It was also recommended that the project should continue with fracture toughness testing and fatigue crack growth testing.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-48	Development of Highly Efficient Solid State Electrochemical Hydrogen Compressor (EHC) *Ludwig Lipp; FuelCell Energy, Inc.*	3.2	X			Reviewers observed that the project is novel, promising and technically elegant. This compressor has no moving parts to degrade performance and durability, and still promises to be able to separate non-reactive compounds like nitrogen, argon, and helium. Additional work is needed to increase capacity beyond two pounds per day. Reviewers also commented that the clarity of the milestone comparison table could be improved, that future cost estimates are needed, and that collaboration could be improved with the addition of an end-user partner.
PD-51	Characterization of Materials for Photoelectrochemical Hydrogen Production (PEC) *Clemens Heske; University of Nevada, Las Vegas*	3.4	X			As highlighted by reviewers, the project provides important data to guide the design of the new PEC materials, in both synthetic and theoretical approaches. Specific project strengths cited include the technical approaches, the excellent materials characterization capabilities, strong collaborations with other research labs in the DOE PEC Working Group, and a focus on materials stability. It was noted that further work is needed in producing larger active area cells and in evaluating the viability and feasibility of supported PEC technologies.
PD-52	PEC Materials: Theory and Modeling *Yanfa Yan; NREL*	3.2	X			According to reviewers, the strong theory and calculation competency of this work is important in guiding promising areas of PEC research. A particular project strength cited was the consideration of important physical properties in material dopants, such as the formation of different phases and incorporation at different possible sites. Further work is needed in developing the theory tools to give predictions regarding the final phase of the materials and dopants. Reviewers also felt the project needs to take a stronger direction-setting role on the synthesis of materials from other DOE PEC Working Group teams and in evaluating viability and scalability of supported PEC technologies.
PD-53	Photoelectrochemical Hydrogen Production: Progress in the Study of Amorphous Silicon Carbide as a Photoelectrode in Photoelectrochemical Cells *Arun Madan; MVSystems*	3.2	X			According to reviewers, this work successfully leverages the existing knowledge base in photovoltaics to supplement the PEC knowledge base. Reviewers stated that strengths include the project team's focus on a single material class, their elegant solution to the problem of band mismatch for water splitting, and the good materials characterization capability of project partners. It was recommended, though, that the cost of this approach compared to other options be further addressed, along with technology viability, scalability, and manufacturing in large scale.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
PD-54	Photoelectrochemical Hydrogen Production: Progress in the Study of Tungsten Oxide Compounds as Photoelectrodes in Photoelectrochemical Cells *Nicolas Gaillard; Hawaii Natural Energy Institute*	2.8		X		Reviewers noted the focus on a single material class as a good approach. Cited strengths include the project team's good scientific methods in materials synthesis and characterization, their collaboration across many groups in the DOE PEC Working Group, and their work with economic viability studies. Reviewers stated that further work is needed in considering the cost of this approach compared to other options, in optimization of counter electrode designs, in improvement in efficiency, and in establishing technology viability and scalability.
PD-55	Photoelectrochemical Hydrogen Production: Progress in the Study of Copper Chalcopyrite as Photoelectrodes in Photoelectrochemical Cells *Jess Kaneshiro; Hawaii Natural Energy Inst.*	3.0	X			Reviewers commended the project for its focus on a single material class and leveraging of the high photocurrents observed in this material class toward developing highly efficient water splitting devices. A project strength is the novel approach to system challenges based on theory and correlation to known photovoltaic systems. Reviewers commented that further work is needed in considering the cost of this approach compared to other options, in development of surface treatments and optimized back contacts, and in establishing technology viability and scalability. It was recommended that the team consider separating silverization efforts into a separate project if segregation cannot be easily overcome.
PD-56	Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen *Liwei Xu; Midwest Optoelectronics, LLC*	2.8		X		Reviewers noted the project's focus on identifying additional PEC materials systems and evaluating their potentials and viability as a strength. According to reviewer recommendations, however, more work is needed in leveraging substantial industry knowledge in alkaline electrolysis in relation to the different substrate and immersion system schemes under investigation. Further work is also needed in the reevaluation of technology viability, scalability, and overall manufacturing potential.
PD-58	Characterization and Optimization of Photoelectrode Surfaces for Solar-to-Chemical Fuel Conversion *Tadashi Ogitsu; LLNL*	3.2	X			Reviewers note that the project is developing a potentially powerful method for understanding catalysis of the water-splitting/hydrogen-evolving reaction, and can be important in guiding promising areas of PEC research. The focus on modeling surface corrosion characteristics is seen as particularly relevant, and is filling a hole in the overall DOE PEC Working Group research portfolio. According to reviewer recommendations, more work is needed in developing models to extend below the surface, in applying models to alternative less-expensive model material systems, in modeling possible surface modification, and in developing models to evaluate photoanode in addition to photocathode systems.

Hydrogen Storage

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-01	System Level Analysis of Hydrogen Storage Options *Rajesh Ahluwalia; ANL*	3.5	X			Reviewers commented that this project should continue to perform system analysis in support of hydrogen storage technology development. The team should document project results and increase coordination of analysis work with the Hydrogen Storage Engineering CoE.
ST-02	Analyses of Hydrogen Storage Materials and On-Board Systems *Stephen Lasher; TIAX, LLC*	3.4			X	Funding for this project ends as the project concluded in FY 2010 with extension of schedule to complete reports into second quarter of FY 2011. TIAX will provide cost analysis report as well as a final project report.
ST-03	Compact (L)H_2 Storage with Extended Dormancy in Cryogenic Pressure Vessels *Gene Berry; LLNL*	3.3	X			The project team should focus on developing a thorough understanding of para-ortho hydrogen conversion and the factors that affect it, implications of using composite pressure vessels in cryogenic applications including degassing into the vacuum insulation space, and the robustness of vacuum insulation systems in automotive applications.
ST-04	Hydrogen Storage Engineering Center of Excellence *Don Anton; SRNL*	3.2	X			While it is still early in the overall Engineering CoE effort, the reviewers considered the coordination and progress good; however, they expressed concern over the potential impact that the ending of the three materials CoEs may have. SRNL should continue to ensure efficient coordination of the Engineering CoE partners, prepare a current state-of-the-art assessment of on-board storage systems for the three material classes for a go/no-go decision point in mid-FY 2011, and ensure system engineering gaps have been identified and appropriate engineering R&D is being carried out within the Engineering CoE.
ST-05	Systems Engineering of Chemical Hydride, Pressure Vessel, and Balance of Plant for On-Board Hydrogen Storage *Darrell Herling; PNNL*	2.9	X			Reviewers found that PNNL has made good progress; however, they expressed concerns regarding the number of areas within the Engineering CoE that PNNL is involved in and regarding their extensive involvement with solid AB, which is considered a high-risk approach for the use of chemical hydrogen materials. In coordination with Engineering CoE partners, PNNL should prepare a current state-of-the-art assessment of the use of solid AB in an on-board hydrogen storage system for a mid-FY 2011 go/no-go decision and should continue to support the Engineering CoE in its other center roles.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-06	Advancement of Systems Designs and Key Engineering Technologies for Materials Based Hydrogen Storage *Dan Mosher; UTRC*	3.2	X			The UTRC effort was found to be well-organized and well-planned and to play a vital role within the Engineering CoE. Reviewers raised concerns that there needs to be good coordination and collaboration with other center partners (e.g., LANL on purification strategies) to avoid duplication of efforts. UTRC should complete model integration and focus on material packing (e.g., metal hydride and sorbents), balance of plant optimization, and qualitative risk assessments in coordination with their Engineering CoE partners.
ST-07	Chemical Hydride Rate Modeling, Validation, and System Demonstration *Troy Semelsberger; LANL*	3.5	X			The LANL team was considered to be well organized and their efforts critical to the Engineering CoE. The development of the acoustic fuel gauge was lauded by the reviewers. LANL should continue their Engineering CoE efforts including development of a fuel gauge sensor and reactor development for hydrogen release from chemical hydrogen storage materials. They should prepare a current state-of-the-art assessment for the go/no-go decision in mid-FY 2011.
ST-08	System Design, Analysis, Modeling, and Media Engineering Properties for Hydrogen Energy Storage *Matthew Thornton; NREL*	2.9	X			NREL's modeling efforts are considered an important function of the Engineering CoE, and the reviewers considered the team to be well qualified. However, they expressed concern regarding whether sufficient auto OEM input is included in the vehicle modeling. There were also questions concerning the "viability index" modeling effort, which they said needs better clarity. NREL should continue to provide guidance to the Engineering CoE on system design trade-offs through their vehicle-level modeling efforts, including prioritization of targets, and they should continue to assist in the selection and characterization of adsorbent materials.
ST-09	System Design and Media Structuring for On-Board Hydrogen Storage Technologies *Darsh Kumar; General Motors*	3.0	X			The reviewers consider the GM team to be strong and their collaborations to be important within the Engineering CoE; however, concern was raised over potential redundancy. GM should continue to collaborate with and support the Engineering CoE on the modeling and design of metal hydride and sorbent-based storage systems.
ST-10	Ford/BASF-SE/UM Activities in Support of the Hydrogen Storage Engineering Center of Excellence *Andrea Sudik; Ford Motor Company*	3.1	X			The reviewers consider the Ford team to be strong and their collaborations to be important within the Engineering CoE, especially their effort investigating the impact of compaction of sorbent materials on their performance. Ford should continue to support the Engineering CoE on adsorbent material engineering and vehicle, cost, and manufacturing modeling.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-11	Fundamental Reactivity Testing and Analysis of Hydrogen Storage Materials *Don Anton; SRNL*	3.1			X	This project is scheduled to end at the end of FY 2010. A final report is being prepared.
ST-12	Quantifying & Addressing the DOE Material Reactivity Requirements with Analysis & Testing of Hydrogen Storage Materials and Systems *John Khalil; UTRC*	2.8			X	This project is scheduled to end during FY 2011. During the remaining time, UTRC will complete its risk assessment activities and carry out additional dust cloud and blow-down characterizations.
ST-13	The Reactivity Properties of Hydrogen Storage Materials in the Context of Systems *Daniel Dedrick; SNL*	3.1			X	This project is schedule to end at the end of FY 2010. A final report is being prepared.
ST-18	A Biomimetic Approach to Metal-Organic Frameworks with High H_2 Uptake *Hong-Cai Zhou; Texas A&M University*	3.0	X			This project, started in FY 2007, was part of the Hydrogen Sorption CoE that is ending in FY 2010. The project will continue its phase 2 work as an independent project. Reviewers recommend emphasizing experiments demonstrating the impact of the open metal sites on hydrogen adsorption kinetics and thermodynamics. Materials should emphasize high volumetric capacity (high surface area per volume), not just exclusively high surface area. External validation of results is also recommended.
ST-19	Multiply Surface-Functionalized Nanoporous Carbon for Vehicular Hydrogen Storage *Peter Pfeifer; University of Missouri-Columbia*	2.9	X			This project, started in FY 2008, was part of the Hydrogen Sorption CoE that is ending in FY 2010. The project will continue its phase 1 work as an independent project. Reviewers recommend that further validation of experimental results be shown through external collaborations, particularly on the measurements of excess capacity hydrogen isotherms and isosteric heats of adsorption.
ST-21	NREL Research as Part of the Hydrogen Sorption Center of Excellence *Lin Simpson; NREL*	3.1			X	This project is part of the Hydrogen Sorption CoE that is ending in FY 2010. A final report will be issued summarizing the overall work accomplished during the past five years of the project. DOE will also maintain a storage material database that will include data from this project.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-22	A Joint Theory and Experimental Project in the Synthesis and Testing of Porous COFs for On-Board Vehicular Hydrogen Storage *Omar Yaghi; UCLA*	2.8	X			Closer internal coordination is needed between the synthesis and theory pieces of the project for theory validation. It is recommended that the group attempt to synthesize at least one composition that was predicted by theory to have high capacity at room temperature. The team should consider using infrared and/or NMR tools to determine the sorption locations and thermodynamics relative to the covalent organic framework structure for the most promising samples, for model validation.
ST-23	New Carbon-Based Porous Materials with Increased Heats of Adsorption for Hydrogen Storage *Randall Snurr; Northwestern University*	3.1	X			Reviewers recommend that further validation of experimental results be shown through external collaborations, particularly on the measurements of excess capacity hydrogen isotherms and isosteric heats of adsorption. Closer internal coordination is needed between the synthesis and theory pieces of the project for theory validation. The project team should increase material characterization to validate sorption locations (for model validation) and performance at both low (<10 bar) and higher pressure (~70-100 bar).
ST-24	Hydrogen Trapping through Designer Hydrogen Spillover Molecules with Reversible Temperature and Pressure-Induced Switching *Angela Lueking; Penn State University*	2.6	X			Reviewers recommend that further validation of experimental results be shown through external collaborations, particularly on the measurements of excess capacity isotherms and kinetics. Efforts should stress a systematic study (e.g., statistical design of experiments) in determining the controlling factors for weak chemisorption (spillover) and additional work on identifying the barriers to improving the uptake and desorption kinetics. The hydrogen trapping work should be of lower priority.
ST-25	Polymer-Based Activated Carbon Nanostructures for H_2 Storage *Israel Cabasso; State University of New York-ESF at Syracuse*	2.3		X		A go/no-go decision point process is underway. Significant improvement is needed on the project plan. A more systematic study is needed to (1) use modeling to identify promising materials, (2) develop those materials, and (3) test the hydrogen storage properties. Improved characterization of the doping material is necessary. Reviewers recommend that further validation of experimental results be shown through external collaborations, particularly on the measurements of excess capacity isotherms and isosteric heat of adsorption.
ST-26	Capacitive Hydrogen Storage Systems: Molecular Design of Structured Dielectrics *Robert Currier; LANL*	1.8		X		This project has been discontinued, and a final report is being prepared.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-27	Tunable Thermodynamics and Kinetics for Hydrogen Storage: Nanoparticle Synthesis Using Ordered Polymer Templates *Mark Allendorf; SNL*	3.2	X			The reviewers found this project to be very relevant and to address critical areas related to hydrogen storage materials—e.g., thermodynamics and kinetics. While there was a general feeling that considerable progress has been made, current results do not clearly demonstrate if thermodynamics can be "tuned" through control of particle size. To address this, the project team should carry out further experimental work to characterize materials and validate the computational predictions so that a definitive conclusion can be drawn.
ST-28	Design of Novel Multi-Component Metal Hydride-Based Mixtures for Hydrogen Storage *Christopher Wolverton; Northwestern University*	3.3	X			The approach of using computational modeling to direct experimental efforts was highly praised by the reviewers. The project should continue according to its research plan, with a greater emphasis on obtaining experimental validation of the computational predictions. However, it was recommended that kinetics be included, and significant emphasis be given to this area.
ST-31	Advanced, High-Capacity Reversible Metal Hydrides *Craig Jensen; University of Hawaii*	3.6	X			This project is scheduled to be completed in FY 2011. During the remaining time, the project should finish its investigations on the extent of reversibility of metal borohydrides under moderate temperature and pressure and the regeneration of $LiAlH_4$ in non-conventional solvents.
ST-32	Lightweight Metal Hydrides for Hydrogen Storage *J.-C. Zhao; Ohio State University*	3.3	X			The project should continue to follow its research plan on the investigation of the reversibility and release kinetics of novel aluminoborane compounds and ammoniated boron-hydrogen compounds.
ST-38	Hydrogen Storage by Novel CBN Heterocycle Materials *Shih-Yuan Liu; University of Oregon*	3.0	X			Reviewers suggested that the project continue and complete demonstrating low energy requirements for the overall dehydrogenation and hydrogenation reactions. They observed that progress has been made on hydrogen release experiments emphasizing hydrogen release kinetics and impact of impurities. Reviewers recommend investigating release mechanisms to understand hydrogen storage capacity that can be realized at given T/P window.
ST-40	Chemical Hydrogen Storage R&D at Los Alamos National Laboratory *Anthony Burrell; LANL*	3.9			X	This project is part of the Chemical Hydrogen Storage CoE that is ending in FY 2010. A final report will be issued summarizing the overall work accomplished during the past five years of the project. DOE will also maintain a storage material database that will include data from this project.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-41	PNNL Progress as Part of the Chemical Hydrogen Storage Center of Excellence *Jamie Holladay; PNNL*	3.2			X	This project is part of the Chemical Hydrogen Storage CoE that is ending in FY 2010. A final report will be issued summarizing the overall work accomplished during the past five years of the project. DOE will also maintain a storage material database that will include data from this project.
ST-44	SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Metal Hydride and Adsorbent Systems *Ted Motyka; SRNL*	2.9	X			The SRNL technical effort was found by the reviewers to be well coordinated with strong collaborations; however, some reviewers would like to see an increased effort in experimental validation of the modeling. The project should continue to support the Engineering CoE through completion of the "Acceptability Envelope" development and material assessment, completion of system scoping models and materials and system design and characterization efforts. Current state-of-the-art assessments will be completed for a go/no-go decision in mid-FY 2011.
ST-45	Key Technologies, Thermal Management, and Prototype Testing for Advanced Solid-State Hydrogen Storage Systems *Joseph W. Reiter; NASA-JPL*	2.6	X			The reviewers expect that the internal knowledge at JPL, from its space programs and previous hydrogen system experience, will be important to the Engineering CoE; however, they found less progress to date than had been expected. The project should continue to support the Engineering CoE through completion of development of an insulation database and leading the adsorbent system design efforts. Current state-of-the-art assessments will be completed for a go/no-go decision in mid-FY 2011. Efforts should be made to achieve greater accomplishments for the next year than have been demonstrated over the past year.
ST-46	Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage *Kevin Drost; Oregon State University*	2.9	X			The Microproducts Breakthrough Institute at OSU was thought by the reviewers to have innovative technology to bring to the Engineering CoE; however, the reviewers did not clearly understand how the microchannel heat exchanger inserts could benefit the storage systems. Also, the extent of OSU's collaborations was not clear. The project should continue to support the Engineering CoE through development of microchannel heat exchangers and combustors. Efforts should be made to identify key applications for the technology and focus on those specific areas. Collaborations should be strengthened.
ST-47	Development of Improved Composite Pressure Vessels for Hydrogen Storage *Norman Newhouse; Lincoln Composites*	2.5	X			While the reviewers know the background of Lincoln Composites and feel their experience can benefit the Engineering CoE, the project approach and progress to date were not as strong as the reviewers expected. The project should continue to support the Engineering CoE through development, testing and validation of lower-cost and improved pressure vessels for hydrogen storage systems. Lincoln Composites should strengthen their collaborations and be a better contributing partner in the Engineering CoE.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-48	Hydrogen Storage Materials for Fuel Cell Powered Vehicles *Andrew Goudy; Delaware State University*	2.7			X	This is a Congressionally Directed Project. The project team should focus on more promising material systems, ensure appropriate baseline model systems are selected, and minimize replication/duplication of work that has been previously carried out or is being carried out by other groups.
ST-49	Hydrogen Storage in Metal-Organic Frameworks *Omar Yaghi; UCLA*	3.3			X	This project has been completed. A final project report will be submitted.
ST-50	Hydrogen Storage through Nanostructured Porous Organic Polymers (POPs) *D.J. Liu; ANL*	3.2			X	A go/no-go decision process is underway. Reviewers recommend that further validation of experimental results be shown through external collaborations, particularly on the measurements of excess capacity hydrogen isotherms and isosteric heats of adsorption. The project team should increase material characterization to validate sorption locations (for model validation) and also performance at low (<10 bar) and higher pressure (~70-100 bar).
ST-51	Electron-Charged Hydrogen Storage Materials *Chinbay Fan; Gas Technology Inst.*	2.1		X		This project will be subjected to a go/no-go decision before the end of calendar year 2010 based on demonstration of a 25% increase in sorption capacity with an applied electric field versus without a field.
ST-53	Life-cycle Verification of Polymeric Storage Liners *Barton Smith; ORNL*	3.1	X			Reviewers found the project approach to be adequate but thought it could be strengthened by inclusion of model validation. Reviewers also questioned if the results will be made publicly available if proprietary materials are being tested. This project should continue according to its research plan on the permeation and lifecycle verification testing of liner materials, and results should be made publicly available.
ST-54	Standardized Testing Program for Solid-State Hydrogen Storage Technologies *Michael Miller; Southwest Research Inst.®*	3.2	X			This project should continue according to its research plan and test and provide results of hydrogen storage materials. Coordination should be strengthened with other DOE-led efforts on material storage characterization and method standardization.
ST-55	NaSi and Na-SG Powder Hydrogen Fuel Cells *Michael Lefenfeld; SiGNa*	3.1	X			This is a Congressionally Directed Project. Reviewers recommend that the project continue to improve hydrogen yield while reducing water requirements and explore waste/canister disposition options with cost and environmental considerations.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ST-92	SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence Design and Testing of Metal Hydride and Adsorbent Systems *Ted Motyka; SRNL*	3.1	X			The SRNL technical effort was found by the reviewers to be well coordinated with strong collaborations; however, some reviewers would like to see an increased effort in experimental validation to the modeling. The project should continue to support the Engineering CoE through completion of the "Acceptability Envelope" development and material assessment, completion of system scoping models and materials and system design and characterization efforts. Current state-of-the-art assessments should be completed for a go/no-go decision in mid-FY 2011.
ST-93	High Strength Carbon Fibers *Felix Paulauskas; ORNL*	3.6	X			The reviewers found this project to be highly relevant, with a strong approach and good results to date. Stronger industrial collaborations were recommended. This project should continue, according to its research plan, to work on developing lower-cost precursor material for the production of high-strength carbon fibers.

Fuel Cells

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-01	Advanced Cathode Catalysts and Supports for PEM Fuel Cells *Mark Debe; 3M*	3.6	X			Reviewers regarded this project as one of the most relevant projects in the Program's portfolio. Work remains concerning water management solutions. It was recommended that the next phase of the project involve large scale customer and developer testing.
FC-02	Highly Dispersed Alloy Catalyst for Durability *Vivek Murthi; UTC Power*	3.1	X			Reviewers suggest that the PI test the most stable catalyst with the most stable support before the project ends in April 2011 and demonstrate durability of the MEAs.
FC-03	Development of Alternative and Durable High Performance Cathode Supports for PEM Fuel Cells *Yong Wang; PNNL*	2.8	X			Reviewers suggest that this project continue with enhanced collaborations with industry partners. Durability measurements should rely on MEA testing rather than rotating disk electrode testing.
FC-04	Non-Platinum Bimetallic Cathode Electrocatalysts *Deborah Myers; ANL*	2.9	X			Reviewers commented that the PI should demonstrate how surface structure might affect oxygen reduction reaction and address the stability of Pd-transition metal alloys in acid media.
FC-05	Advanced Cathode Catalysts *Piotr Zelenay; LANL*	3.3	X			Reviewers thought that there were very good performance results for the non-PGM catalysts. Future work should focus on the *in situ* durability of the catalysts (both PGM and non-PGM).
FC-06	Durable Catalysts for Fuel Cell Protection during Transient Conditions *Radoslav Atanasoski; 3M*	3.3	X			Reviewers commented that this project team uses novel concepts of oxygen evolution reaction at the cathode and oxygen reduction reaction at the anode to address durability under start/stop conditions. Future work should include proof that the mitigation strategy works under these conditions with dispersed carbon catalysts.
FC-07	Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes *Brian Pivovar; NREL*	2.9	X			Reviewers noted that this project is less than a year old, but that the effort to understand substrate materials and novel material production method is evident. Down-selection over time is encouraged to narrow the focus of the project and demonstrate a significant breakthrough.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-08	Nanosegregated Cathode Catalysts with Ultra-Low Platinum Loading *Nenad Markovic; ANL*	3.5	X			Reviewers thought that this new project was very relevant to DOE objectives, addressing high activity electrocatalysts and ultra-low PGM loading. Accelerated MEA testing is recommended to validate results shown in *ex situ* testing.
FC-09	Contiguous Platinum Monolayer Oxygen Reduction Electrocatalysts on High-Stability-Low-Cost Supports *Radoslav Adzic; BNL*	2.9	X			This project was rated highly by reviewers regarding relevance to DOE objectives, and it was noted for having a strong team, good collaborations, and solid fundamental concepts. It was suggested that the team analyze materials to identify the most promising materials for future R&D emphasis.
FC-10	The Science and Engineering of Durable Ultralow PGM Catalysts *Fernando Garzon; LANL*	2.7	X			Reviewers encouraged the team to partner with a fuel cell company capable of making state-of-the-art electrodes. Reviewers are interested in how flooding will be addressed in future plans.
FC-11	Molecular-scale, Three-dimensional Non-Platinum Group Metal Electrodes for Catalysis of Fuel Cell Reactions *John Kerr; LBNL*	1.9		X		LBNL's approach is new and very high risk, but, considering the talent of the team, reviewers think that the potential exists for a breakthrough in catalysis. Early go/no-go decisions should be made, and work should only continue on materials that demonstrate feasibility.
FC-12	Polymer Electrolyte Fuel Cell Lifetime Limitations: The Role of Electrocatalyst Degradation *Deborah Myers; ANL*	3.3	X			Reviewers think that this new project may answer some key fundamental questions about fuel cell degradation mechanisms. The PI should study the effects of sulfate contamination on low-loaded novel electrode materials.
FC-13	Durability Improvements through Degradation Mechanism Studies *Rod Borup; LANL*	3.3	X			The reviewers noted that this team has world class scientists with complementary specialties and has already identified the impact of electrode ink solvent and durability. Reviewers suggested streamlining and consolidating some of the tasks.
FC-14	Durability of Low Pt Fuel Cells Operating at High Power Density *Scott Blanchet; Nuvera Fuel Cells*	3.0	X			Reviewers noted that the work of this project is balanced between modeling and experimentation. The PI is encouraged to investigate materials that can enable operation at higher temperatures.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-15	Improved Accelerated Stress Tests (ASTs) Based on Real World FCV Data *Tom Madden; UTC Power*	2.7	X			Reviewers commended the focus and approach of the team and stated the importance of connecting laboratory findings to real-world data. The PI is encouraged to reconsider including performance recovery cycles, since they can help keep low-Pt materials from being unnecessarily eliminated from consideration.
FC-16	Accelerated Testing Validation *Rangachary Mukundan; LANL*	3.4	X			Reviewers noted that fuel cell systems for buses may have significantly different stressors compared to those for light-duty vehicles. The PI should perform additional diagnostics to obtain insight into sources of performance losses.
FC-17	Fuel Cells Systems Analysis *Rajesh Ahluwalia; ANL*	3.6	X			The PI has developed a model that addresses performance targets, such as cost, thermal and water management, start-up and shut-down time, and transient operation. Reviewers encourage collaborations with more OEMs (including forklift manufacturers) to deepen understanding of components.
FC-18	Mass-Production Cost Estimation for Automotive Fuel Cell Systems *Brian James; Directed Technologies, Inc.*	3.3	X			This project received exceptionally high scores from the reviewers regarding its relevance to DOE objectives, as it provides essential benchmarks for the current state of the Program. The addition of workshops was suggested, to allow OEMs to interact with and better understand the model. Project ends in the 2nd quarter of FY 2011.
FC-19	Direct Hydrogen PEMFC Manufacturing Cost Estimation for Automotive Applications *Jayanti Sinha; TIAX, LLC*	3.2	X			Reviewers noted that this highly detailed research is essential for informing overall policy on the realistic prospect that fuel cell systems will be an affordable alternative to other technologies. The analysis highlights which areas need attention to further reduce cost. A sensitivity analysis of membrane thickness is recommended. Project ends in the 2nd quarter of FY 2011.
FC-20	Microstructural Characterization of PEM Fuel Cell Materials *Karren More; ORNL*	3.0	X			Reviewers noted that this team has exceptional skills and equipment to explore the microstructural information from the MEAs. Work to characterize the carbon corrosion and catalyst particle changes around polymer electrolyte ionomers in the MEA catalyst layers is recommended.
FC-21	Neutron Imaging Study of the Water Transport in Operating Fuel Cells *David Jacobson; NIST*	3.5	X			Reviewers thought that the approach of the PI is very effective and efficient and the project is highly relevant to DOE objectives. The PI should address water transport and distribution in the fuel cell stacks to diagnose the failure mechanism of individual cells in stacks.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-22	Nitrided Metallic Bipolar Plates *Peter Tortorelli; ORNL*	3.1	X			Reviewers commented that improved metallic bipolar plates will help overcome the cost and durability barriers that inhibit fuel cell commercialization. The PI should examine more complicated flow fields over larger plates and perform *ex situ* evaluations before and after cycling. Project ends in the 2^{nd} quarter of FY 2011.
FC-23	Low Cost PEM Fuel Cell Metal Bipolar Plates *Conghua Wang; TreadStone*	2.7	X			Reviewers noted the PI's endeavors to produce a low cost bipolar plate base material to meet the DOE 2015 cost target. They commented that the project should demonstrate performance and corrosion resistance under real-world conditions and for greater lengths of time.
FC-24	Metallic Bipolar Plates with Composite Coatings *Jennifer Mawdsley; ANL*	3.2	X			The reviewers noted that the approach of using highly conductive and ductile aluminum as a new material is new and innovative. The PI should conduct high temperature testing and contact-resistance testing, and a dopant should be identified as soon as possible.
FC-25	Air-Cooled Stack Freeze Tolerance *Dave Hancock; Plug Power, Inc.*	1.9		X		Reviewers regarded this approach as focusing too much on a specific niche market instead of advancing scientific understanding of underlying freeze issues and proposed mitigation strategies. A lack of focus was noted by reviewers. Project will undergo a go/no-go decision process in December 2010.
FC-26	Fuel-Cell Fundamentals at Low and Subzero Temperatures *Adam Weber; LBNL*	3.1	X			Reviewers noted that start-up and operation in subzero temperatures is vitally important for fuel cells in automotive applications. The PI should focus on the effect of water and freezing within the thin catalyst layer structure.
FC-27	Development and Validation of a Two-phase, Three-dimensional Model for PEM Fuel Cells *Ken Chen; SNL*	2.7	X			Reviewers commented that this project is in its early stages, but it is not clear how the model's predictive capabilities will address the cost and durability goals. The PI should compare the simulation results with other simulation groups based on a more commonly used flow field design.
FC-28	Transport Studies Enabling Efficiency Optimization of Cost-Competitive Fuel Cell Stacks *James Cross; Nuvera Fuel Cells*	3.3	X			Operating at high current densities is essential to the practical application of fuel cells in vehicles, said reviewers. The PI should place greater emphasis on fundamentals of mass transport.
FC-29	Water Transport Exploratory Studies *Rod Borup; LANL*	3.2	X			Reviewers noted that the understanding of water management in cells and stacks will lead to enhanced fuel cell performance. It is recommended that the PI conduct *ex situ* measurements of transport properties of porous materials and relate them to improvements in the cell.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-30	Water Transport in PEM Fuel Cells: Advanced Modeling, Material Selection, Testing, and Design Optimization *Vernon Cole; CFD Research Corp.*	2.6		X		Reviewers noted that the PI's approach focuses on using computational fluid dynamics to model water transport with emphasis on gathering experimental data and modeling water transport in MEAs. Reviewers suggest revisiting fundamental material parameters as part of two-phase model validation efforts.
FC-31	Development and Demonstration of a New Generation High Efficiency 10kW Stationary PEM Fuel Cell System *Durai Swamy; Intelligent Energy*	2.4		X		Reviewers noted that to date, the demonstrated lifetime, durability, and efficiency metrics are well below DOE targets. System optimization and redesign are recommended to accommodate future plug-in absorption-enhanced reformer in order to increase system efficiency.
FC-32	Development of a Low Cost 3-10kW Tubular SOFC Power System *Norman Bessette; Acumentrics Corporation*	3.1	X			While this project focuses on non-renewable fuels, reviewers noted that it also is leveraging niche markets to further develop fuel cell technology. Future work should include assuring cell stability, demonstrating stability over thermal cycles, focusing on efficiency enhancement, resolving thermal issues, and continuing cell manufacturing automation.
FC-33	New Polyelectrolyte Materials for High Temperature Fuel Cells *John Kerr; LBNL*	2.7	X			Reviewers scored this project exceptionally high for its relevance to DOE objectives; however, high MEA performance has yet to be demonstrated. Reviewers recommended focusing on imidizoles attached to perfluorosulfonic acid (PFSA) and measuring conductivity versus relative humidity at various temperatures.
FC-34	Membranes and MEAs for Dry, Hot Operating Conditions *Steven Hamrock; 3M*	3.6	X			Reviewers noted that building on proven PFSA chemistry has allowed the PI to improve conductivity and durability while maintaining manufacturability. It was recommended that the PI de-emphasize the model compound polymer degradation work and the competitive kinetics work.
FC-35	Lead Research and Development Activity for DOE's High Temperature, Low Relative Humidity Membrane Program *James Fenton; University of Central Florida*	2.4		X		Reviewers commented that this project strives to provide a standardized MEA fabrication and testing service for membrane fabricators as part of the High Temperature Membrane Working Group. *In situ* tests need to focus on hydrogen crossover, electrical insulation mechanisms, and high frequency resistance to cover the ideal function of the membrane.

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FC-36	Dimensionally Stable Membranes *Cortney Mittelsteadt; Giner Electrochemical Systems, LLC*	3.2	X			Reviewers commented that the development of robust, high temperature, low RH membranes is vital for the successful commercialization of fuel cells, especially for automotive applications. The PI builds understanding of low equivalent weight (EW) material performance and durability and has produced low EW ionomers that meet conductivity and resistance targets. Further characterization and optimization are required.
FC-37	Rigid Rod Polyelectrolytes: Effect on Physical Properties Frozen-in Free Volume: High Conductivity at low RH *Morton Litt; Case Western Reserve University*	2.6		X		Reviewers noted that novel polymer chemistry is being developed to produce materials that are thermally stable and have enhanced conductivity at low relative humidity, but they question the ability of the project to develop a membrane with desired stability. Industrial collaboration to bring different perspectives is suggested.
FC-38	NanoCapillary Network Proton Conducting Membranes for High Temperature Hydrogen/Air Fuel Cells *Peter Pintauro: Vanderbilt University*	2.9	X			Reviewers noted that the team used a novel materials engineering approach to create improved composite membranes. The PI has shown the capability of creating materials that are highly conductive at 120^{o}C, 50%RH. Reviewers recommend including a cost study to prove that an electrospun membrane can be consistent with DOE cost targets.
FC-39	Novel Approaches to Immobilized Heteropoly Acid (HPA) Systems for High Temperature, Low Relative Humidity Polymer-Type Membranes *Andrew Herring; Colorado School of Mines*	3.3	X			Reviewers commented that this project addresses the need to provide conductivity in the absence of water. The project uses materials that are inherently conductive, so its full upside potential is very large. Adding a stack developer for outside testing and validation, when a stable membrane is made, is recommended.
FC-40	High Temperature Membrane with Humidification-Independent Cluster Structure *Ludwig Lipp; FuelCell Energy, Inc.*	1.9		X		Reviewers thought that this project had a lack of disclosure on technical details of approach and results data. Conflicting priorities of protecting intellectual property and sharing information from a publicly funded project made it difficult for reviewers to assess progress.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-41	Novel Approach to Advanced Direct Methanol Fuel Cell Anode Catalysts *Huyen Dinh; NREL*	2.7	X			Reviewers noted that this project addresses stabilizing the dispersion and chemical composition of the platinum/ruthenium catalyst on a support to stabilize catalysis of methanol oxidation. The most relevant objective of this project would be to demonstrate the first stable and possible high-activity catalyst for methanol electro-oxidation.
FC-42	Advanced Materials for RSOFC Dual Operation with Low Degradation *Randy Petri; Versa Power*	2.9	X			Reviewers noted that reversible solid oxide fuel cells can integrate renewable production of electricity and hydrogen when power generation and steam electrolysis are coupled in a system – thus converting intermittent solar and wind energy into "firm power." Project targets are too low and should be increased to prove the project's relevance to the DOE portfolio.
FC-43	Resonance-Stabilized Anion Exchange Polymer Electrolytes *Yu Seung Kim; LANL*	2.6		X		Reviewers commented that the PI presents an innovative concept—to discern if stable alkaline membranes can be obtained while achieving DOE targets. The PI should increase MEA testing and increase the involvement of collaborators.
FC-44	Engineered Nano-scale Ceramic Supports for PEM Fuel Cells *Eric Brosha; LANL*	3.1	X			Reviewers commented that the project's evaluation of alternatives to carbon-supported catalysts is highly relevant, because a new catalyst support that is more corrosion-resistant than carbon-supported catalysts—while maintaining hydrophobicity—should result in a more durable cell. The PI should add thermogravimetric analysis in the presence of catalysts to determine mass loss and degradation of catalysts in a humidified air atmosphere.
FC-45	Effects of Fuel and Air Impurities on PEM Fuel Cell Performance *Fernando Garzon; LANL*	3.2	X			Reviewers said that understanding impurity effects on the rates of fuel cell reactions is very important to improving the performance and increasing the lifetime of fuel cells. The PI should include more work on mitigation strategies for impurities to improve durability.
FC-46	Effects of Impurities on Fuel Cell Performance and Durability *James Goodwin; Clemson University*	3.1	X			Reviewers thought that even though a limited set of impurities has been selected, those impurities studied have been well characterized and provided useful information. The PI should add airborne impurities to the scope of the project.
FC-47	The Effects of Impurities on Fuel Cell Performance and Durability *Trent Molter; University of Connecticut*	2.7	X			Reviewers noted that the team observed little effect from many of their experiments, probably due to testing at 100% RH (which would likely flush the contaminant from the system) and at 800 mA/cm^2 (likely leading to oxidation of the contaminants). The PI should consider testing at different operating conditions.

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FC-48	Effect of System and Air Contaminants on PEMFC Performance and Durability *Huyen Dinh; NREL*	3.2	X			Reviewers praised this project for receiving the highest score of the impurities projects in the category of relevance to DOE objectives. The project has a relatively broad scope, as it attempts to identify, model, and catalog system contaminants and to disseminate this knowledge to the fuel cell community. Electrochemical and fuel cell testing should start as soon as possible.
FC-50	Economic Analysis of Stationary PEM Fuel Cell Systems *Kathya Mahadevan; Battelle*	3.2			X	Reviewers commented that this project is highly relevant to the DOE, as stationary and early market applications have become a larger part of the Program's portfolio. Collaboration was widespread, including 60 partners and 400 current or candidate users. The project has been completed.
FC-51	Fuel Cell Testing at the Argonne Fuel Cell Test Facility *Ira Bloom; ANL*	3.5	X			Reviewers rated this project as one of the highest ranking in terms of relevance to overall DOE objectives, as it is quite important to have standardized test procedures so that informed decisions based on validated data can be made in the marketplace. The PI is commended for leading a well-planned and well-executed project. Protocol verification tests are recommended for dynamic cycling tests.
FC-52	Technical Assistance to Developers *Tommy Rockward; LANL*	3.0	X			Reviewers noted that this project has the potential to make very valuable contributions to the Program as it leverages the DOE investment to provide organizations with answers to their technical questions. The PI should develop a strategic test plan that targets clients, rather than operate on a first-come, first-served basis.
FC-59	Improved, Low-Cost, Durable Fuel Cell Membranes *James Goldbach; Arkema*	2.6			X	Reviewers commented that blending a highly conductive, low-cost ionomer with a stable non-conductive polymer is a solid approach. However, when the blends fail to reach conductivity targets, the approach becomes questionable. Reviewers recommend providing a cost estimate to justify the basis for the low cost claim. Project has been completed.
FC-60	Protic Salt Polymer Membranes: High-Temperature Water-Free Proton-Conducting Membranes *Dominic Gervasio; Arizona State University*	2.3		X		Reviewers thought that the development of an anhydrous proton conductor could enable significant system simplification and cost reduction of PEMFC systems. Reviewers suggest PI pause testing fuel cells until conductivity and stability are proven. Project was deemed a 'no-go' during go/no-go process.
FC-76	Biomass Fuel Cell Systems *Neal Sullivan; Colorado School of Mines*	3.1	X			This is a Congressionally Directed Project. Reviewers noted that this project supports the biomass-to-hydrogen pathway, which is deemed a very important pathway. Reviewers recommended that the PI address the impurities in biogas reforming and how the gas will be cleaned.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed/ Other*	Summary Comments
FC-77	Fuel Cell Coolant Optimization and Scale-up *Satish Mohapatra; Dynalene*	2.6		X		This is a Congressionally Directed Project. Reviewers noted that the product being developed is a potentially valuable coolant that will be compatible with small fuel cells. Coolant is close to production. Independent validation by a national lab is recommended.

Manufacturing R&D

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
MN-01	Fuel Cell MEA Manufacturing R&D *Michael Ulsh; NREL*	3.3	X			The reviewers noted a good collaborative effort; however, there is a need to integrate modeling with hardware diagnostics and to create an overall plan to provide feedback.
MN-02	Reduction in Fabrication Costs of Gas Diffusion Layers *Jason Morgan; Ballard Material Products*	3.1	X			The reviewers noted good progress; however, there is a need for more collaborative efforts. The addition of at least one outside customer to evaluate product quality and performance would add significantly to the credibility of the project's results.
MN-03	Modular, High-Volume Fuel Cell Leak-Test Suite and Process *Ian Kaye; UltraCell Corp.*	3.3	X			There is a Go/No-Go decision pending complete prototype validation. UltraCell should assess the transferability of this process to other products and technologies.
MN-04	Manufacturing of Low-Cost, Durable Membrane Electrode Assemblies Engineered for Rapid Conditioning *Colin Busby; W.L. Gore*	3.3	X			Reviewers asked for more data on the cost reductions. This project needs to move quickly to achieve DOE target loadings for FY 2010 and FY 2015.
MN-05	Adaptive Process Controls and Ultrasonics for High Temperature PEM MEA Manufacture *Raymond Puffer; Rensselaer Polytechnic Inst.*	3.6	X			Reviewers noted good progress in developing adaptive real-time process controls. RPI has demonstrated the feasibility of ultrasonic welding with a cycle time of less than one second as compared to about one minute for thermal bonding.
MN-06	Metrology for Fuel Cell Manufacturing *Eric Stanfield; NIST*	3.0	X			Reviewers noted a need for more industry input on flow field plate manufacturing variability. Informal interactions seem widespread; however, it is not clear how deep the interactions are, how beneficial to the project they are, and whether feedback exists.
MN-07	High Speed, Low Cost Fabrication of Gas Diffusion Electrodes for Membrane Electrode Assemblies *Emory De Castro; BASF*	3.2	X			The XRF application is progressing well; however, reviewers noted that more attention needs to be shown on adaptability to low temperature PEM fuel cells and other technologies.

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MN-08	Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage Vessels *Mark Leavitt; Quantum Fuel Systems Technologies Worldwide, Inc.*	2.7			X	The project did not receive high scores from reviewers regarding relevance to proposed future research. The reviewers noted that the approach (even if fully successful) will have a low impact on reducing the storage system manufactured cost. An increased focus on reducing the need for expensive materials, such as carbon fiber, was recommended.

Technology Validation

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
TV-01	Controlled Hydrogen Fleet and Infrastructure Analysis *Keith Wipke; NREL*	3.6	X			It was observed that this work is very relevant to DOE fuel cell objectives of conducting independent assessment and dissemination of fuel cell vehicle (FCV) information and providing real-world feedback to researchers and partners to improve technology. The information gathered will help improve technology readiness for FCVs and lead to successful market introduction. This project validated DOE targets in real-world conditions. Reviewers commented that there should be an expanded presentation of results from primarily fuel cell events to broader auto events and government conferences.
TV-04	Hydrogen to the Highways *Ron Grasman; Daimler*	3.3	X			Reviewers noted that the project addresses DOE goals for technology validation and aligns well with Daimler's internal program goals. They are moving the technology toward greater commercial readiness. They are addressing not only vehicle technology goals, but also associated issues such as maintenance and codes and standards. Real-world vehicle operation has been shown for Generation I vehicles but not much yet for Generation II. It was recommended that a plan be developed for continued operation and testing of the Generation II vehicles and refueling stations after completion of the present project.
TV-05	Hydrogen Vehicle and Infrastructure Demonstration and Validation *Gary Stottler; General Motors*	3.7	X			Reviewers observed that this project involves real-life vehicle demonstration and performance validation, making it quite relevant to the goals and objectives of the Program. In particular, Project Driveway, a meaningful test and evaluation by a sample representing consumers, is especially relevant because self-serve refueling is also a part of the project. The project exhibits tremendous progress of the fuel cell system in terms of performance, volume, weight, and cost. Reviewers stated that another technology validation phase is needed beyond September 2011.
TV-06	Validation of an Integrated Hydrogen Energy Station *Ed Heydorn; Air Products*	2.8	X			Reviewers noted that the project concept is very relevant to DOE objectives to improve the availability and reduce the cost of hydrogen for vehicle refueling as a co-product of economic power generation. The project had very slow progress in meeting objectives. No system economic analysis has been presented for the OCSD, Fountain Valley station. Reviewers encouraged Air Products to speed up the OCSD demonstration.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
TV-07	California Hydrogen Infrastructure Project *Ed Heydorn; Air Products*	3.0	X			Reviewers suggested that plans be made to continue operation, data collection, and analysis (including economic evaluation) following completion on the project. Station operators should be identified, and a smooth transition to these new operators should be initiated as soon as possible. They further recommended that Air Products investigate and compare the economics of different station concepts (liquid hydrogen station versus pipeline station), evaluate challenges of different technologies, and give cost breakeven points dependent on demand.
TV-08	Technology Validation: Fuel Cell Bus Evaluations *Leslie Eudy; NREL*	3.2	X			Reviewers commented that this project should continue for as long as the bus demonstration projects are providing meaningful data to be collected and analyzed. By assimilating results across a range of environmental conditions and from different manufacturers and operators, a much clearer picture of the status of fuel cell bus technology should emerge.
TV-09	Hawaii Hydrogen Power Park *Richard Rocheleau; Hawaii Natural Energy Inst.*	3.1	X			Reviewers stated that this is a good, broad project that should generate useful, real-world information on systems performance versus key technical targets. The project has encountered significant delays to its schedule due to delays in processing agency approvals. Reviewers recommended that the project integrate with renewable energy storage efforts on the Big Island and that the project accelerate its activities.
TV-10	Tanadgusix (TDX) Foundation Hydrogen Project/PEV Project *Connie Fredenberg; Tanadgusix Foundation*	2.5			X	Reviewers commented that this Congressionally directed project should be moved to the Vehicle Technologies Program as it is no longer considering the use of fuel cells. It is still relevant to DOE objectives, but not the Hydrogen Program objectives.
TV-11	Texas Hydrogen Highway—Fuel Cell Hybrid Bus and Fueling Infrastructure Technology Showcase *David Hitchcock; Texas Hydrogen Highway*	2.9	X			Reviewers stated that this Congressionally directed project should ensure that operational data is shared with NREL. They observed that the team was able to perform the key duties necessary to procure and operate a fuel cell bus and fueling infrastructure in a large state where fuel cells and hydrogen have little presence, and that the project has provided public exposure to an operational hydrogen-powered bus and fueling station.
TV-12	Florida Hydrogen Initiative *David Block; University of Central Florida*	2.2	X			Reviewers commented that this Congressionally directed project, as initially conceived, had some degree of relevance to DOE research, development, and demonstration objectives. However the significant changes over time have made it less relevant.

Safety, Codes and Standards

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
SCS-01	National Codes and Standards Templates *Carl Rivkin; NREL*	3.3	X			Reviewers thought this was a solid project and noted that the National Templates demonstrate a comprehensive coordination to bring stakeholders together to develop data-driven codes and standards to address critical gaps. Reviewers suggested expanding the level of detail of this project to provide a useful tool for experts, and to include balance-of-plant-related impurities for fuel quality standards. Reviewers noted that the project does an excellent job of synthesizing and compiling codes and standards into one resource.
SCS-02	Component Standard Research & Development *Robert Burgess; NREL*	3.3	X			Reviewers appreciated the project's well-coordinated alignment of its test program with industry and SDOs such as the Society for Automotive Engineers (SAE), CSA Standards, and the American Society of Mechanical Engineers (ASME). Reviewers also praised the round-robin safety sensor testing and international collaboration. Reviewers suggested additional outreach to industry stakeholders to better understand industry needs and increased funding to continue sensor and composite overwrap pressure vessel testing. The reviewers also suggested that the project complete a comprehensive list of components under consideration to identify and prioritize gaps in component needs.
SCS-03	Codes and Standards Training and Outreach and Education for Emerging Fuel Cell Technologies *Carl Rivkin; NREL*	3.5	X			Reviewers noted the project's critical role in implementing hydrogen and fuel cell technologies including the focus on forklift and backup power—two examples of early market commercialization. Reviewers also recognized the excellent collaboration with the California Fuel Cell Partnership and local fire departments, holding workshops at locations where hydrogen and fuel cell technologies are deployed, and they noted the coverage given to a variety of projects (such as stationary power, forklifts, etc.). Reviewers suggested increasing project funding to allow for additional training sessions. Recommended actions include outreach programs to regional training and outreach to volunteer first-responder centers.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
SCS-04	Hydrogen Safety Sensors *Eric Brosha; LANL*	3.1	X			The reviewers appreciated the integrated technical approach to R&D and especially the collaboration between the two national laboratories—LANL and LLNL. Reviewers also supported including an industry partner, ESL, into the sensor testing process. Reviewers recognized the project's technical accomplishments such as a stable sensor response time, long-term testing and evaluation of sensor materials, and designs to improve long-term stability. Reviewers recommended stronger coordination with the industry partner, ESL, to better define ESL's role, competencies and contributions. Some reviewers were also concerned about the project's approach to commercialization in regard to cost goals, performance, and calibration requirements.
SCS-05	Materials and Components Compatibility *Daniel Dedrick; SNL*	3.3	X			Reviewers thought this was a solid project that enables the early market deployment of hydrogen and fuel cell technologies such as forklifts. Most of the reviewers commended the test facilities, general collaboration, and thorough engineering-based data collection and test methodology, which are critical to the development of codes and standards. However, some reviewers expressed concern that there is insufficient industry collaboration. Reviewers thought that progress has been slow on material system evaluations and the application of the fatigue crack growth law. One reviewer noted that the fatigue crack growth law is based on the hypothesis of "leak before break" and that it is unclear how the testing program will incorporate the hypothesis into its testing procedures.
SCS-06	Hydrogen Safety Knowledge Tools *Linda Fassbender; PNNL*	3.7	X			Reviewers praised the project's depth and breadth of resources related to hydrogen safety. One reviewer noted that the Hydrogen Safety Best Practices Online Manual and the H2 Incident Reporting and Lessons Learned Database are a "go-to information source" for the successful implementation of hydrogen infrastructure technologies. Reviewers also praised the applied expertise and input from the Hydrogen Safety Panel. However, some reviewers noted that these Web sites need more distribution and increased involvement with energy companies. Specific recommendations include improving the Web site layout and creating better metrics to show who is using the Web site.

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SCS-07	Hydrogen Fuel Quality *Tommy Rockward; LANL*	3.5	X			Reviewers commended the rigorous technical R&D approach for determining levels of constituents in hydrogen. The reviewers also recognized the project's contribution of critical data to the development of an international standard for hydrogen fuel quality—International Organization for Standardization (ISO) Technical Committee 197 Working Group 12. Most reviewers noted the strong collaboration between investigators and stated that this should be considered a model on how to approach such tasks. Reviewers did note the project will need to go more in-depth on durability testing at the cell level and should enhance stack-level testing and system-validation. Reviewers encouraged stronger international collaboration with fuel providers.
SCS-08	Hydrogen Safety Panel *Steven Weiner; PNNL*	3.5	X			Reviewers agreed the Hydrogen Safety Panel (HSP) provides critical expertise for the safety of hydrogen and fuel cell projects. Reviewers recognized the extensive experience and background (NASA, industry partners, etc.) of each panel member as a strength. Also, reviewers identified the safety recommendations, which are based on incident reviews, as an excellent resource. Reviewers expressed concerns for how the HSP is evaluated in terms of its effectiveness and stated that no metric exists to show how much work the HSP has accomplished. Reviewers also expressed concern that the HSP might be overfunded.
SCS-10	Hydrogen Release Behavior *Daniel Dedrick; SNL*	3.4	X			Most reviewers recognized the strength of the project's defensible and traceable research basis for codes and standards development. In particular, reviewers commended the outstanding transformation of scientific analysis into actual safety guidance. Some reviewers also praised the work done on tunnel release and the good data that came out of the project. Reviewers thought the weak collaboration for indoor refueling was a weakness. In addition, some reviewers thought the tunnel release work could benefit from additional clarity and direction.
SCS-13	International Energy Agency Hydrogen Implementing Agreement Task 19 Hydrogen Safety *William Hoagland; SNL*	3.0	X			Reviewers recognized the importance of the project's commitment and role in the international collaboration. Reviewers observed that the project does a good job with data collaboration and has a strong link for input into www.h2incidents.org. However, most reviewers stated that the project focus is vague and Task 19 goals need more clarity. Some reviewers noted that collaboration needs to increase with SDOs such as ISO and the International Electrotechnical Committee (IEC).

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SCS-14	Safe Detector System for Hydrogen Leaks *Robert Lieberman; Intelligent Optical Systems, Inc.*	3.2	X			Reviewers recognized the project's successful R&D efforts toward a commercially available sensor. Reviewers thought the project was well executed and has fostered good collaboration with potential customers. Reviewers also praised the sensor development process and sensor testing, including collaboration with NREL. Reviewers noted the project has yet to address significant technological barriers including cross interference, humidity, and carbon monoxide poisoning. Also, reviewers noted the project should not suggest to the public to install sensors in residential garages. Reviewers suggested the project needs to complete a more thorough cost analysis and clearly identify the size of the end-user market.
SCS-15	Hydrogen Safety Training for First Responders *Linda Fassbender; PNNL*	3.8	X			Reviewers praised the project's relevance and its important role in advancing the safe deployment of hydrogen and fuel cell technologies. Reviewers identified a number of important strengths, including the project's focus on real-time training, accurate targeting of relevant audiences, the well-designed curriculum, the hands-on training afforded by the fuel cell prop, and the ability to move the course to a variety of locations. Some reviewers stated that the project should consider increased collaboration with the DOD, onsite training on the East Coast, training specific to forklift operation, and greater outreach to more audiences and locations.
SCS-17	Hydrogen Safety Training for Researchers *Salvador Aceves; LLNL*	3.6	X			Reviewers praised the relevance of the course and its sound technical approach. Reviewers noted the Web-based course has an excellent graphical layout and the course reaches out to the correct audiences. Reviewers also recognized the technical expertise and facilities at LLNL that were used to develop the training. Some reviewers thought that it might be useful for the course to be tailored to specific laboratory settings. Also, the course might need to be modified for audiences with different education levels (i.e., those with associate degrees and those with doctoral degrees).
SCS-18	Optically Read MEMS Hydrogen Sensor *Barton Smith; ORNL*	3.3	X			Reviewers recognized good coordination and technology transfer as strengths of this project. In particular, reviewers identified excellent cooperation between the government and industry. Reviewers suggested improving collaboration during the testing process with nationally recognized testing laboratories such as Underwriters Laboratories (UL).

Education

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ED-03	Hydrogen and Fuel Cell Education at California State University, Los Angeles *David Blekhman; Cal State-LA*	3.6			X	This project is fully funded. Reviewers stated, "This is a much-needed, well-thought-out undergraduate curriculum for hydrogen technologies. The coursework is combined with substantial laboratory work, which is excellent." Comments suggest increased collaboration with other universities, particularly Humboldt State University (another Education project) and formalized student feedback (information retention and opinion) on coursework.
ED-04	Hydrogen Energy in Engineering Education (H2E3) *Peter Lehman; Humboldt State University Sponsored Programs Foundation*	3.4			X	This project is fully funded. Reviewers appreciated the multi-element, hands-on, generalized approach that allowed the project to reach a larger group of students in a meaningful and relevant way through lab kits, internships, and general engineering curriculum modules. Suggestions include increased collaboration with other universities to increase the number of students reached, formalized feedback to measure student information retention and opinion, and additional industry partnerships to support a stronger internship program.
ED-05	Hydrogen Education Curriculum Path at Michigan Technological University *Jason Keith; Michigan Technological University*	3.5			X	This project is fully funded. Reviewers commended the project's module approach, citing the potential for education programs to easily incorporate the materials into their curriculum. The rigorous collaboration and external review process were also identified as key to creating a comprehensive curriculum. Reviewers encouraged the project to proactively promote the course for broad dissemination at other education institutions.
ED-06	Hydrogen and Fuel Cell Education Program Concentration *David Block; University of Central Florida/University of North Carolina-Charlotte*	2.7			X	This project is fully funded. Reviewers recognized the challenges faced by the project move from the University of Central Florida to the University of North Carolina-Charlotte and thought that the project contributed to the Program's education goals in a limited way. They suggested that the PI develop more substantive collaborations by offering the coursework through remote teaching or at additional educational institutions.
ED-07	Development of a Renewable Hydrogen Production and Fuel Cell Education Program *Michael Mann; University of North Dakota*	2.7			X	This project is fully funded. Reviewers viewed this as a solid project with a comprehensive outreach to general and advanced engineering students at the University of North Dakota and saw potential in promoting hydrogen as an energy storage medium for intermittent renewables in this wind-rich state. They encouraged the development of a national dissemination plan.

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ED-08	Dedicated to the Continued Education, Training and Demonstration of PEM Fuel Cell Powered Lift Trucks In Real-World Applications *Tom Dever; Carolina Tractor & Equipment Co. Inc.*	3.4			X	This project is fully funded. This education project with concurrent hands-on demonstration experience was very well received by reviewers. The educators, a respected forklift distributor, were ideally situated to reach out to potential adopters as a trusted spokesperson in the industry. Reviewers would have liked to see more interaction with material-handling trade associations and a better developed data collection effort; they encouraged similar follow-on projects.
ED-09	Hydrogen Education in Texas *David Hitchcock; Houston Advanced Research Center*	3.0			X	This project is fully funded. Reviewers thought that this project was a solid state-education effort, reaching out to a diverse and comprehensive audience throughout Texas. The project adopted appropriate performance measurement efforts following last year's suggestions. Reviewers encouraged wider reach by moving beyond the "green" community.
ED-10	Development of Hydrogen Education Programs for Government Officials *Shannon Baxter-Clemmons; The South Carolina Hydrogen and Fuel Cell Alliance*	3.5			X	This project is fully funded. Reviewers were impressed with this multi-pronged outreach effort targeting state leaders through in-person interaction and traditional and new media. They commended the group for exceeding goals for the number of outreach events. The suite of information materials, including case studies and payback evaluations, helped to strengthen the case for adopting hydrogen and fuel cells. Suggestions included expanded collaboration and documentation of best practices to serve as a guide for other state outreach efforts.
ED-11	VA-MD-DC Hydrogen Education for Decision Makers *Chelsea Jenkins; Commonwealth of Virginia*	3.4			X	This project is fully funded. Reviewers praised the extensive collaboration and real-world experience (actual and virtual) imparted through ride-and-drives and nationally broadcast videos. Partnerships with the surrounding area, other states, industry, a national laboratory, and a television show provided a broad network to disseminate newsletters, video segments, and other education materials. It is recommended that remaining videos focus on early markets and fuel cell vehicles.
ED-12	State and Local Government Partnership *Joel Rinebold; Connecticut Center for Advanced Technology, Inc.*	3.3			X	This project is fully funded. As a state with an existing fuel cell presence, Connecticut was viewed as a good candidate for education efforts. Reviewers observed that the feasibility and financial models balanced real-world considerations with potential benefits and provided a realistic basis for recommending fuel cells, without overselling the technology. However, some reviewers were concerned with the potential misuse of the simple analyses. It was recommended that best-practices be rolled out in New England, New York, and New Jersey.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ED-13	Raising H2 and Fuel Cell Awareness in Ohio *Pat Valente; Ohio Fuel Cell Coalition*	3.5			X	This project is fully funded. Reviewers commented on the strength of developing audience-specific forums to reach out with targeted and relevant information. Databases, forums, and other networking resources linking state funding, project developers, fuel cell manufacturers, and end-users have been particularly successful and useful. Sharing best-practices with neighboring state organizations and other renewable energy and energy efficiency organizations in Ohio was recommended.
ED-14	H2L3: Hydrogen Learning for Local Leaders *Patrick Serfass; Technology Transition Corporation*	3.4			X	This project is fully funded. Reviewers commended the project's commitment to working *with* local leaders, rather than talking *at* them. These efforts were aided by using peer presenters and informal networking and by implementing train-the-trainer education through collaboration with the Public Technology Institute and National Association of State Energy Officials. Reviewers suggested additional outreach meetings at state and local official meetings beyond hydrogen, fuel cell, or energy focused events where they were "preaching to the choir."
ED-15	Hydrogen Education State Partnership Program *Charles Kubert; Clean Energy States Alliance*	2.6			X	This project is fully funded. Some reviewers thought this was a solid effort, with good collaboration with the National Council of State Legislators. Others viewed the objectives of the project as important to state and local government outreach, but thought that execution was poor, particularly regarding deliverable quality and dissemination methods. Despite the national scope of the project, outreach seemed to be limited to only a few states. Reviewers suggested extensive collaboration with other national local leader organizations, DOE, and the hydrogen and fuel cell trade community.

Systems Analysis

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
AN-01	Infrastructure Analysis of Early Market Transition of Fuel Cell Vehicles *Brian Bush; NREL*	2.9	X			Reviewers suggested that NREL include industry input and market competition.
AN-02	Analysis of Energy Infrastructures and Potential Impacts from an Emergent Hydrogen Fueling Infrastructure *Andy Lutz; SNL*	3.1	X			Reviewers suggested that SNL consider nuclear and renewables such as solar, wind, and geothermal for infrastructure limitations, and extend the analysis beyond the boundaries of California.
AN-03	Agent-Based Model of the Transition to Hydrogen-Based Personal Transportation: Consumer Adoption and Infrastructure Development Including Combined Hydrogen, Heat, and Power *Matthew Mahalik; ANL*	2.9	X			It was recommended that the project be revised to include metro areas, contrast all the methods of hydrogen production, and utilize additional data from industry and government stakeholders. A calibration is required to relate the model output to real situations, especially for consumer purchasing and selection rationale.
AN-04	HyTrans Model: Analyzing the Potential for Stationary Fuel Cells to Augment Hydrogen Availability in the Transition to Hydrogen Vehicles *David Greene; ORNL*	3.4	X			Reviewers suggested that project incorporate a smaller fueling station size; compare all hydrogen generation pathways; obtain input from industry stakeholders; and consider integration of U.S. and international fuel markets.
AN-05	Biogas Resources Characterization *Ali Jalalzadeh-Azar; NREL*	2.9	X			Reviewers suggested that NREL include analysis of on-site conversion of bio-methane to electricity versus export to natural gas pipelines and sensitivity of cost to the level of impurities.
AN-06	Cost and GHG Implications of Hydrogen for Energy Storage *Darlene Steward; NREL*	2.9	X			Reviewers recommended that future work analyze: where hydrogen storage could be utilized; financial factors of payback period and technology maturity and risk; and other energy storage options such as Li-ion batteries.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
AN-07	Hydrogen and Water: Engineering, Economics and Environment *A.J. Simon; LLNL*	3.1	X			Reviewers suggested that this project examine the water use for hydrogen production compared to overall water flow and use. The project recognizes and addresses the potential impact and barrier of water availability and the cost of water treatment on the cost of hydrogen production. Reviewers recommended that the project account for legacy rights that restrict water use in certain regions and that it be expanded to include other hydrogen production pathways.
AN-08	Analysis of Business Cases with the Fuel Cell Power Model *Marc Melania; NREL*	3.1	X			The model will be periodically updated to address the reviewers' comments from the merit review. Future updates will include validating the model with fuel cell companies, data from actual Combined Heat, Hydrogen and Power (CHHP) applications, review by notable business school and collaborate with EPRI for future input.
AN-10	Fuel Quality in Fuel Cell Systems *Shabbir Ahmed; ANL*	3.3	X			It was recommended that ANL's future work focus on analyzing the impact of fuel contaminants on fuel cell performance, the cost of contaminant removal, and fuel cost.
AN-11	Macro-System Model *Mark Ruth; NREL*	3.4	X			Reviewers suggested including a training plan for public use; considering more out-of-the-box analysis applications for the model; focusing the model on analysis projects and scenarios; and defining model's capabilities.
AN-12	Life-Cycle Analysis of Criteria Pollutant Emissions from Stationary Fuel-Cell Systems *Michael Wang; ANL*	3.5	X			It was noted that ANL's analysis with their GREET model is essential to the Program's benefit analysis. It was suggested that future "well-to-wheels" analysis efforts compare analysis results with data from the California Air Resources Board, the AQMD, and EPA; validate model results for target scenarios; and conduct additional sensitivity analysis.
AN-13	CO_2 Reduction Benefits Analysis for Fuel Cell Applications *Paul Friley; BNL*	3.1	X			Analysis with the MARKAL model is ongoing. Reviewers suggested adding other hydrogen production technologies to the carbon reduction analysis; expanding the range of sensitivity variables; reducing the number of assumptions for biomass with carbon capture and sequestration; and including industry and academia validation of the modeling results.
AN-14	Pathways to Commercial Success: Technologies and Products Supported by the HFCIT Program *Steve Weakley; PNNL*	2.6			X	Reviewers commented that the project provides valuable information about commercial benefits and subsequent product development that result from DOE R&D. However, the project will be removed from the merit review in the future because the project is not a research-type project and does not have research-related targets.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
AN-15	Fuel Cell Power Model: Evaluation of CHP and CHHP Applications *Darlene Steward; NREL*	3.6	X			It was noted that the model has been well received by the fuel cell community and has been utilized to compare projects on a transparent basis with a common set of assumptions. Reviewers suggested that the project continue to validate the model with feedback from industry; calibrate the model to real world testing/operations such as CHP biogas to power; and include integration with renewable sources such as wind and solar.
AN-16	Geospatial Analysis of Hydrogen Production Pathways *Matt Kromer; TIAX, LLC*	2.9			X	The model enables a geo-spatial analysis of hydrogen infrastructure for conventional and renewable hydrogen production. The project has been completed and the model for the analysis has been delivered to DOE.
AN-17	Recent Developments in the Hydrogen Demand and Resource Assessment (HyDRA) Model *Johanna Levene; NREL*	3.3	X			Reviewers recommended that the project include hydrogen infrastructure development and obtain company feedback on infrastructure build-out.

Recovery Act

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ARRA-01	Commercialization Effort for 1 Watt Consumer Electronics Power pack *Chuck Carlstrom; MTI Micro Fuel Cells, Inc.*	3.1	X			The project's sound testing regimen and progress made in power density, packaging, and lifetime of the portable power packs were noted. The incumbent technology for battery recharging will offer significant competition, but this product has a potentially large consumer market. More analyses of performance data and lifetime predictions for this product for comparisons to existing battery recharging and other competing technologies were recommended.
ARRA-02	Solid Oxide Fuel Cell Diesel Auxiliary Power Unit Demonstration *Steven Shaffer; Delphi Automotive*	3.1	X			It was noted that there is a significant amount of collaboration with OEMs, which has been beneficial for the project and its progress. A transportation application will be challenging, and the project team should consider accelerating the deployment to the field test. Reviewers suggested that more details on schedules and actions taken against specific barriers be included in future review presentations.
ARRA-03	Highly Efficient, 5kW CHP Fuel Cells Demonstrating Durability and Economic Value in Residential and Light Commercial Applications *Rhonda Staudt; Plug Power, Inc.*	2.7	X			It was noted that a strong project team has been assembled with a focus on the California market. Reviewers recommended that Plug Power strengthen its market assessment and business case and focus on product reliability. Reviewers also noted that technical accomplishments have been good, but progress has been slow, and they suggested additional bench testing in addition to field tests, in order to accelerate the durability verification process. The relationships between the modeling effort and product engineering and between cost analysis and the market price target need to be more clearly addressed. Reviewers recommended that this project compare costs and performance of its CHP system to other competing technologies and to similar CHP systems.
ARRA-06	PEM Fuel Cell Systems Providing Backup Power to Commercial Cellular Towers and an Electric Utility Communications Network *Mike Maxwell; ReliOn, Inc.*	3.6	X			It was noted that the planned number of deployments (over 150) is large enough to have a positive impact on commercialization. There is broad involvement of gas suppliers, customers, and regulators to gain a foothold in the market and to chart a pathway to wider adoption. Reviewers recommended that the costs of the fuel cell units be compared with those of existing technologies, and that next year the project team share lessons-learned and an economic assessment of the technology. It was also suggested that it would be beneficial for the team to develop a public outreach plan to increase public and market awareness of the product.

Project Number	Project Title *PI Name & Organization*	Final Score	Continue	Discontinue / Further Review	Completed / Other*	Summary Comments
ARRA-07	Accelerating Acceptance of Fuel Cell Backup Power Systems *Rick Cutright; Plug Power Inc.*	2.6	X			While backup power appears to be a good application, progress to-date has been slow, with site selection still not completed. Reviewers commented that the market and technical barriers need to be better described and the economic justification for the technology strengthened. Plug Power was advised to provide more detail on the technical approach to specific barriers. It was also suggested that the team re-examine the possibility of operation on hydrogen alone rather than the hybrid hydrogen/LPG system.
ARRA-08	HEB Grocery Total Power Solution for Fuel Cell Powered Material Handling Equipment—Fuel Cell Hybrid Power Packs and Hydrogen Refueling *William Mitchell; Nuvera Fuel Cells*	2.9	X			It was observed that good progress has been made, with the hydrogen generation system and 14 fuel cell systems already deployed at H-E-B. The application is a good market for fuel cells, and it will be valuable for comparisons with battery powered lift trucks. Refrigerated warehouses are a challenging environment, and it was suggested that Nuvera consider reviewing how they evaluate power needs for the fuel cell units. Reviewers stated that it would be useful to see more information on life cycle and durability of the fuel cell units, and that preliminary productivity data should be validated and analyzed to show the overall benefits for the H-E-B warehouse, jobs, and productivity gains.
ARRA-09	7B: Fuel Cell-Powered Lift Truck FedEx Freight Fleet Deployment *Curtis Cummings; FedEx Freight East*	3.3	X			As a greenfield site and a fast paced environment, this is an ideal deployment scenario for this type of fuel cell application. Reviewers noted that good technical progress has been made and that it would be useful for FedEx to provide economic analysis on the lift trucks and how they compare with electric and propane-powered alternatives.
ARRA-10	Fuel Cell-Powered Lift Truck - Sysco Houston Fleet Deployment *Scott Kleiver; Sysco of Houston*	3.6	X			It was noted that Sysco's greenfield site and large number (98) of fuel cell lift trucks provide an ideal deployment scenario. Reviewers encouraged Sysco to continue monitoring progress of the fuel cells and collecting data on their performance; and to clearly identify the value proposition for future deployments. They also suggested that the project team communicate the success of the project publicly to increase awareness of fuel cell applications.
ARRA-11	7B: GENCO Fuel Cell Powered Lift Truck Fleet Deployment *Jim Klingler; GENCO*	3.5	X			It was noted that technical progress to-date had been good, with 59 fuel cell units already deployed at the Wegman's site. The reviewers commended GENCO for assembling a cross-section of partners representing different sectors of the U.S. retail economy, including two hydrogen suppliers. Class I, II, and III lift trucks are included in the project. The reviewers suggested that GENCO continue to monitor progress and provide lessons-learned based on feedback from the operators and customers.

*"Other" includes congressionally directed projects.

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INTRODUCTION

The FY 2010 U.S. Department of Energy (DOE) Hydrogen Program and Vehicle Technologies Program Annual Merit Review and Peer Evaluation Meeting (AMR) was held June 7–11, 2010, at the Marriott Wardman Park in Washington, D.C. This report is a summary of comments by AMR peer reviewers on the hydrogen and fuel cell projects funded by DOE's Office of Energy Efficiency and Renewable Energy (EERE) and the hydrogen production projects funded by the Office of Fossil Energy. The results of this merit review and peer evaluation are utilized by the DOE in making funding decisions for upcoming fiscal years.

The objectives of this meeting were as follows:

- Review and evaluate FY 2010 accomplishments and FY 2011 plans for DOE laboratory programs, industry/university cooperative agreements, and related research, development, and demonstration (RD&D) efforts
- Provide an opportunity for program stakeholders/participants (e.g., fuel cell manufacturers, component developers, etc.) to shape the DOE-sponsored RD&D program in such a way that the highest-priority technical barriers are addressed and technology transfer is facilitated
- Foster interactions among the national laboratories, industry, and universities conducting RD&D

The peer review process followed the guidelines of the Peer Review Guide developed by EERE. The peer review panel members, listed in Table 1, provided comments on the projects presented. These panel members are experts from a variety of related backgrounds involving hydrogen and fuel cells, and they represent national laboratories, universities, various U.S. Government agencies, and manufacturers of hydrogen production, storage, delivery, and fuel cell technologies. Each reviewer was screened for conflicts of interest (COIs) as prescribed by the Peer Review Guide. A complete list of the meeting participants is presented as Appendix A.

Table 1: Peer Review Panel Members

No.	Name	Organization
1	Tarek Abdel-Baset	Chrysler LLC
2	Kev Adjemian	Nissan Technical Center North America, Inc.
3	Radoslav Adzic	Brookhaven National Laboratory
4	Channing Ahn	California Institute of Technology
5	Etsuo Akiba	ETRI, National Institute of Advanced Industrial Science and Technology (AIST)
6	Anthony Androsky	U.S. Fuel Cell Council
7	Laurent Antoni	Commissariat A l'Energie Atomique (CEA)
8	Koorosh Araghi	National Aeronautics and Space Administration
9	Katherine Ayers	Proton Energy Systems
10	U. (Balu) Balachandran	Argonne National Laboratory
11	Viktor Balema	Sigma-Aldrich
12	Farshad Bavarian	Chevron
13	Pierre Benard	Hydrogen Research Institute
14	Guido Bender	National Renewable Energy Laboratory
15	Leonid Bendersky	National Institute of Standards and Technology
16	Thomas Benjamin	Argonne National Laboratory

17	Larry Blair	U.S. Department of Energy
18	Christopher Bordeaux	Bordeaux International Energy Consulting, LLC
19	Rod Borup	Los Alamos National Laboratory
20	Nico Bouwkamp	California Fuel Cell Partnership
21	Robert Bowman	Oak Ridge National Laboratory
22	Craig Brown	National Institute of Standards and Technology
23	Tobias Brunner	BMW Group
24	Tony Burrell	Los Alamos National Laboratory
25	F. Colin Busby	W. L Gore & Associates
26	Robert Buxbaum	REB Research & Consulting
27	Julie Cairns	CSA America
28	Stephen Campbell	Automotive Fuel Cell Cooperation
29	Dan Casey	Chevron
30	Richard Chahine	Institut de recherche sur l'hydrogene
31	Biswajit Choudhury	DuPont Fuel Cells
32	John Christensen	National Renewable Energy Laboratory (ret., DLA-DOD)
33	Mike Ciocco	Independent Civil Engineering Professional
34	William Collins	UTC Power
35	Alan Cooper	Air Products and Chemicals, Inc.
36	Phil Cox	University of North Florida
37	James Cross III	Nuvera Fuel Cells, Inc.
38	Ben Deal	California Air Resources Board
39	Mark Debe	3M
40	Emory DeCastro	BASF Fuel Cell, Inc.
41	Huyen Dinh	National Renewable Energy Laboratory
42	G. Charles Dismukes	Rutgers University
43	Tabbetha Dobbins	Louisiana Tech University
44	Junhang Dong	University of Cincinnati, Department of Chemical and Materials Engineering
45	Daniel Driscoll	U.S. Department of Energy
46	Dave Edlund	Element 1, LLC
47	Erich Erdle	EFCECO
48	Mitch Ewan	Hawaii Natural Energy Institute
49	Chinbay Fan	Gas Technology Institute
50	David Farese	Air Products and Chemicals, Inc.
51	Linda Fassbender	Pacific Northwest National Laboratory
52	George Fenske	Argonne National Laboratory
53	Magali S. Ferrandon	Argonne National Laboratory
54	James Fletcher	University of North Florida
55	Stuart Funk	LMI
56	Jennifer Gangi	Fuel Cells 2000
57	Fernando Garzon	Los Alamos National Laboratory
58	Thomas Gennett	National Renewable Energy Laboratory
59	Don Gervasio	Arizona State University
60	Craig Gittleman	Electrochemical Energy Research Labs
61	Robert Glass	Lawrence Livermore National Laboratory
62	James Goldbach	Arkema, Inc.
63	Andrew Goudy	Delaware State University
64	Joe Graber	U.S. Department of Energy
65	Jason Graetz	Brookhaven National Laboratory
66	Leo Grassilli	Office of Naval Research
67	Karl Gross	Hydrogen Technology Associates
68	Nikunj Gupta	Shell Hydrogen, LLC

69	Monjid Hamdan	Giner, Inc.
70	Jennifer Hamilton	California Fuel Cell Partnership
71	Steven Hamrock	3M Fuel Cell Components Program
72	Jonathan Hardis	National Institute of Standards and Technology
73	Barbara Hennessey	U.S. Department of Transportation
74	Thorsten Herbert	NOW GmbH
75	Andy Herring	Colorado School of Mines
76	Shinichi Hirano	Ford Motor Company
77	Mark Hoberecht	National Aeronautics and Space Administration
78	Clark Hochgraf	Rochester Institute of Technology
79	Jamie Holladay	U.S. Department of Energy
80	Aaron Hoskin	Natural Resources - Canada
81	Thanh Hua	Argonne National Laboratory
82	Jimmy Humphrey	J.L. Humphrey & Associates
83	Ashraf Imam	Naval Research Laboratory
84	David Jacobson	National Institute of Standards and Technology
85	Brian James	Directed Technologies, Inc.
86	Tom Jarvi	UTC Power
87	Craig Jensen	University of Hawaii
88	Scott Jorgensen	General Motors
89	Nick Josefik	US Army Corps of Engineers (USACE-DOD)
90	Zakiul Kabir	ClearEdge Power
91	Alexander Kabza	Zentrum für Sonnenenergie und Wasserstoff Forschung (ZSW) Baden-Württemberg
92	Ian Kaye	UltraCell Corp.
93	Jay Keller	Sandia National Laboratory
94	John Kerr	Lawrence Berkeley National Laboratory
95	Shyam Kocha	National Renewable Energy Laboratory
96	Chet Kolodziej	Freedom Field
97	John Kopasz	Argonne National Laboratory
98	Robert Kozak	Atlantic Biomass Conversions, Inc.
99	Matt Kromer	TIAX, LLC
100	Melissa Laffen	Alliance Technical Services
101	Michael Laughlin	New West Technologies, LLC
102	William Lear	University of Florida
103	James Lee	Johns Hopkins University
104	Clovis Linkous	Florida Solar Energy Center
105	Francis Lipiecki	Consultant, previously at Rohm and Haas
106	Ludwig Lipp	FuelCell Energy, Inc.
107	Nenad Markovic	Argonne National Laboratory
108	Victor Maroni	Argonne National Laboratory
109	Shawna McQueen	Energetics Incorporated
110	Gregory Meisner	General Motors Global Research & Development
111	Tasios Melis	University of California, Berkeley
112	Jonathan Melman	Intematix
113	James Merritt	U.S. Department of Transportation
114	James Miller	Argonne National Laboratory
115	Michael Miller	Southwest Research Institute
116	Eric Miller	University of Hawaii at Manoa, HNEI
117	Robert Miller	Air Products and Chemicals, Inc.
118	George Mitchell	University of Michigan
119	Rana Mohtadi	Toyota Motor Engineering and Manufacturing of North America (TEMA)
120	Karren More	Oak Ridge National Laboratory

121	Gregory Moreland	Sentech, Inc.
122	Jason Morgan	Ballard Material Products
123	Bryan Morreale	National Energy Technology Laboratory
124	David Mountz	Arkema, Inc.
125	Deborah Myers	Argonne National Laboratory
126	Kevin Nguyen	Chevron Energy Technology Company
127	Mike Nicholas	University of California, Davis
128	James Ohi	Consultant to U.S. Department of Energy
130	Kelly Oleary	General Motors
131	Gregory Olson	Sentech, Inc.
132	Jon Owejan	General Motors Electrochemical Energy Research
133	Umit Ozkan	Ohio State University
134	Catherine Padró	Los Alamos National Laboratory
135	George Parks	FuelScience LLC
136	Pinakin Patel	FuelCell Energy
137	Vitalij Pecharsky	Iowa State University
138	Michael Penev	National Renewable Energy Laboratory
139	Robert Perret	Nevada Technical Services, LLC
140	Mike Perry	United Technologies Research Center (UTRC)
141	John Petrovic	Petrovic and Associates
142	Guido Pez	Air Products and Chemicals, Inc. (retired)
143	Peter Pintauro	Vanderbilt University
144	Bryan Pivovar	National Renewable Energy Laboratory
145	Walt Podolski	Argonne National Laboratory
146	Raymond Puffer	Rensselaer Polytechnic Institute
147	Vijay Ramani	Illinois Institute of Technology, Chicago
148	Glenn Rambach	Trulite, Inc.
149	Mark Richards	Versa Power Systems
150	Vernon Roan	University of Florida
151	Ewa Rönnebro	Pacific Northwest National Laboratory
152	Neil Rossmeissl	U.S. Department of Energy, Biomass Program
153	Tecle Rufael	Chevron
154	Mark Ruth	National Renewable Energy Laboratory
155	Jim Saber	NextEnergy
156	Gary Sandrock	Sandia National Laboratory
157	Patrick Serfass	Technology Transition Corporation
158	Travis Shultz	U.S. Department of Energy
159	Don Siegel	University of Michigan
160	Robert Sievers	Teledyne Energy Systems
161	James Simnick	BP America
162	Darlene Slattery	University of Central Florida—Florida Solar Energy Center
163	Petros Sofronis	University of Illinois, Urbana-Champaign
164	Jacob Spendelow	Los Alamos National Laboratory
165	Eric Stanfield	National Institute of Standards and Technology
166	Vesna Stanic	EnerFuel
167	Mike Steele	Advanced Technology Center
168	Marc Steen	Institute for Energy, Joint Research Centre, European Commission
169	Darlene Steward	Hydrogen Technologies & Systems Center
170	Detlef Stolten	Forschungszentrum Jülich GmbH
171	Ken Stroh	Sentech, Inc.
172	Andrea Sudik	Ford Motor Company
173	Wayne Surdoval	U. S. Department of Energy

174	Dr. Robert Sutton	Argonne National Laboratory
175	Karen Swider Lyons	Naval Research Laboratory
176	Satish Tamhankar	Linde LLC
177	Leonard Tender	U.S. Naval Research Laboratory
178	George Thomas	U.S. Department of Energy (retired)
179	Ali T-Raissi	University of Central Florida—Florida Solar Energy Center
180	Michael Ulsh	National Renewable Energy Laboratory
181	Nicholas Vanderborgh	Los Alamos National Laboratory (retired)
182	Mike Veenstra	Ford Motor Company
183	George Vernstrom	3M
184	Vilayanur Viswanathan	Pacific Northwest National Laboratory
185	Gerald Voecks	General Motors (retired)
186	Jesse Wainright	Case Western Reserve University
187	James Waldecker	Ford Motor Company
188	Heli Wang	National Renewable Energy Laboratory
189	Douglas Wheeler	DJW Technology, LLC
190	Robert Wichert	U.S. Fuel Cell Council
191	Mark Williams	URS
192	Keith Wipke	National Renewable Energy Laboratory
193	Christopher Wolverton	Northwestern University
194	Kin Wong	U. S. Department of Transportation
195	Neal Woodbury	Arizona State University
196	Piotr Zelenay	Los Alamos National Laboratory
197	Yimin Zhu	Nanosys, Inc
198	Richard Ziegler	Sentech, Inc.

SUMMARY OF PEER REVIEW PANEL'S CROSS-CUTTING COMMENTS AND RECOMMENDATIONS

AMR panel members provided comments and recommendations regarding selected DOE hydrogen and fuel cell projects, overall management of the Program, and the AMR peer evaluation process. Project comments and scores are provided in the following sections of the report. Comments on sub-program management are provided in Appendix B.

ANALYSIS METHODOLOGY

A total of **216** projects were reviewed at the meeting. As shown above, **198** panel members participated in the AMR process, providing a total of **1,165** project evaluations (not every panel member reviewed every project). These reviewers were asked to provide numeric scores (on a scale of 1 to 4, with 4 being the highest) for five aspects of the work presented. Sample evaluation forms are provided in Appendix C. Scores and comments were submitted using laptops (provided on-site) to an online, private database allowing for real-time tracking of the review process. A list of projects that were presented at the AMR but were not reviewed is provided in Appendix D.

Scores were based on the following five criteria and weights (for all projects except American Recovery and Reinvestment Act projects, which used separate criteria):

Score 1: Relevance to overall DOE objectives (20%)
Score 2: Approach to performing the work (20%)
Score 3: Technical accomplishments & progress toward project and DOE goals (40%)
Score 4: Collaboration and coordination with other institutions (10%)
Score 5: Proposed future work (10%)

For each project, an average score was calculated (from the scores of individual reviewers) for each of the five aforementioned criteria. These average scores were then weighted and combined to produce a final overall score for each project. In this manner, a project's final overall score can be meaningfully compared to that of another project. The following formula was used to calculate the weighted, overall score:

Final Score = [Score 1 x 0.20] + [Score 2 x 0.20] + [Score 3 x 0.40] + [Score 4 x 0.10] + [Score 5 x 0.10]

Some new projects were reviewed, for which the third criterion (Technical Accomplishments) did not apply because of the projects' recent startup. In this case, the other four criteria were scaled proportionately in the weighting calculation. The weighting value for the remaining scores [weight + (40/60 * weight)] was used to establish a final score formula for these projects. The result was the following:

Final Score = Score 1 x {0.20 + [(40/60) x 0.20]} +
Score 2 x {0.20 + [(40/60) x 0.20]} +
Score 4 x {0.10 + [(40/60) x 0.10]} +
Score 5 x {0.10 + [(40/60) x 0.10]}

A perfect overall score of "4" would indicate that a project satisfied the five criteria to the fullest possible extent; the lowest possible overall score of "1" would indicate that a project did not satisfactorily meet any of the requirements of the five criteria.

Reviewers were also asked to provide qualitative comments regarding the five criteria, specific strengths and weaknesses of the project, and/or any recommendations relating to the work scope. These scores and comments were entered into a database for easy retrieval and analysis. The comments are summarized in the following sections of this report.

Reviewers of American Recovery and Reinvestment Act (ARRA) projects used the following criteria:

Score 1: Relevance (20%)
Score 2: Development/ Deployment Approach (30%)
Score 3: Technical Accomplishments and Progress (40%)
Score 4: Collaborations (10%)

Reviewers were also asked to provide summary comments regarding ARRA project strengths and weaknesses and to provide specific recommendations.

ORGANIZATION OF THE REPORT

The project comments and scores are grouped by sub-program (Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Technology Validation; Safety, Codes & Standards; Education; Systems Analysis; and ARRA activities) in order to align with the DOE Program planning scheme. Each of these sections begins with a brief description of the general type of R&D or other activity being conducted. This is followed by the results of the reviews of each of the projects presented at the 2010 AMR. A summary of the qualitative comments is provided for each project, as well a graph showing the overall project score and a comparison of how each project aligns with all other projects in its sub-program area. A sample graph is provided in Figure 1.

The project comparisons illustrated in the report are criteria based. Each rectangular blue bar in the chart represents that project's average score for one of the five designated criteria. These scores (blue bars) are then compared with the related maximum, minimum, and average scores for the same criterion across all projects in the same sub-program. The black line bars that overlay the blue rectangular bars represent the maximum, average, and minimum scores for each criterion.

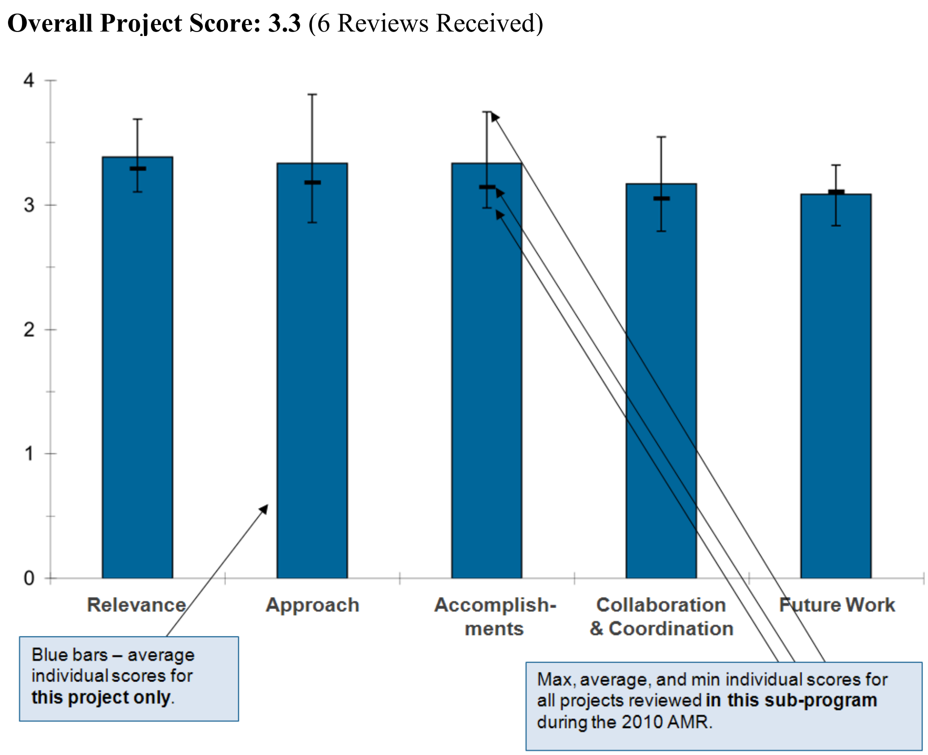


Figure 1: Project Score Graph with Explanation

For clarification, consider a hypothetical review in which only five projects were presented and reviewed in a sub-program; Table 2 displays the average scores for each of the project's five rated criteria.

Table 2: Sample Project Scores

	Relevance (20%)	Approach (20%)	Accomplish-ments (40%)	Collaboration & Coordination (10%)	Future Work (10%)
Project A	3.4	3.3	3.3	3.2	3.1
Project B	3.1	2.8	2.7	2.7	2.9
Project C	3.0	2.6	2.7	2.8	2.9
Project D	3.4	3.5	3.4	3.2	3.3
Project E	3.6	3.7	3.5	3.4	3.4
Max	3.6	3.7	3.5	3.4	3.4
Average	3.3	3.2	3.1	3.0	3.1
Min	3.0	2.6	2.7	2.7	2.9

The Project A chart would contain five blue rectangular bars to represent the values listed for Project A above. A black line bar indicating the related maximum, minimum, and average values for each criterion would overlay each of the blue bars to facilitate comparison with other projects in the sub-program. In addition, each project's criterion scores would be weighted and combined to give a final, overall project score that could be meaningfully compared with those of other projects. Below is a sample calculation for the Project A weighted score.

Final Score for Project A = [3.4 x 0.20] + [3.3 x 0.20] + [3.3 x 0.40] + [3.2 x 0.10] + [3.1 x 0.10] = 3.3

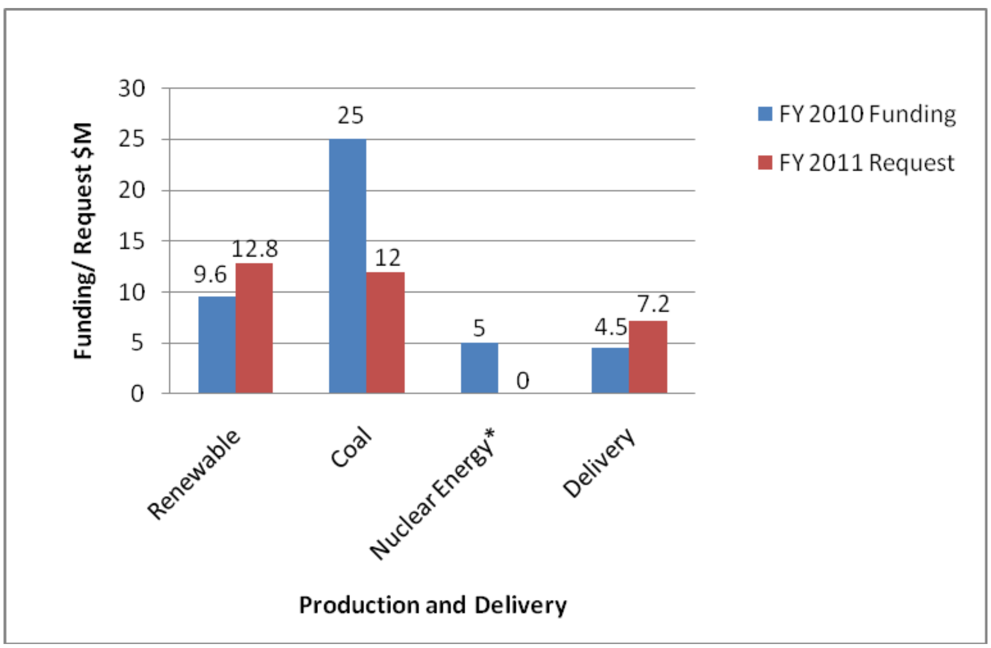
2010
Hydrogen Production and Delivery
Summary of Annual Merit Review of the Hydrogen Production and Delivery Sub-program

Summary of Reviewer Comments on the Hydrogen Production and Delivery Sub-program:

This review session evaluated hydrogen production and delivery research from all DOE activities in the Fuel Cell Technologies Program (FCT) at EERE and in the Hydrogen and Clean Fuels Program at the Office of Fossil Energy (FE). The production and delivery projects were considered by reviewers to be well aligned with DOE goals and objectives.

The hydrogen production projects reviewed represent a diverse portfolio of technologies to produce hydrogen from renewable-based energy sources, as well as coal with sequestration. Production project sub-categories include water electrolysis, bio-derived renewable liquids reforming, biomass gasification, solar-driven thermochemical cycles, photoelectrochemical direct water splitting, biological hydrogen production, hydrogen production from coal, and separations technologies. The hydrogen delivery projects reviewed included work in key research and development areas such as pipeline embrittlement, new fiber reinforced polymer pipelines and linings, and compressors, in addition to analysis work focused on the next stage of development of the Hydrogen Delivery Scenario Analysis Model. Overall, the production and delivery projects were judged by reviewers to have made considerable progress in reducing both projected capital and operating costs and in improving material properties. Reviewer comments, concerns, and recommendations varied considerably by project and are summarized below.

Hydrogen Production and Delivery Funding by Technology



**The Nuclear Hydrogen initiative concluded at the end of FY 2009. Limited research and development on high temperature electrolysis as a potential end-user application may continue under the Next Generation Nuclear Plant Project.*

Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the production and delivery projects were above-average to high, with scores of 3.6, 3.1, and 2.4 for the highest, average, and lowest scores, respectively. The scores are indicative of the technical progress that has been made over the past year. Reviewer feedback, recommendations, and major concerns for each project category are summarized below.

Electrolysis: In general, projects in this area scored favorably and were regarded as well aligned with current program goals and objectives. The projects focused on increasing stack efficiency and decreasing capital cost, along with independent testing and integration with renewable power sources. The reviewers felt that the path forward for PEM electrolysis systems is well laid out and progress is significant. However, it was noted that high pressure operation may not reduce system costs as system complexity is increased and safe gas mixture levels must be maintained. Furthermore, reviewers emphasized that continued independent testing is important to inform the technology area.

Bio-Derived Liquids Reforming: Projects in this topic area included a study focused on increasing mechanistic understanding of hydrogen production from bio-derived liquids, in addition to specific investigations exploring reforming of glycerol, bio-oils, and ethanol. The reviewers noted that much progress has been made in these areas, but stressed the need for further work, including the development of optimized catalysts, investigations into feedstock quality, and demonstration of long-term testing prior to full-scale commercialization. Strengthening collaborations with other researchers and potential end-users was also recommended.

Biomass Gasification: The gasification project reviewed in this topic area focused on developing an initial reactor using a biomass slurry hydrolysis and reforming process for hydrogen production. Reviewers were impressed by the conversions of synthetic and woody biomass feedstocks that have been demonstrated. According to reviewer recommendations, future funding in this area should depend on results of an economic evaluation. Next steps should include investigating effects of variations in feedstock composition and impurities on catalyst performance, use of non-wood flower feedstock, and scale-up to system pilot testing.

Solar-Driven High Temperature Thermochemical Production: The projects reviewed in this topic area were favorably rated for researchers' technical skills and abilities and for collaborative efforts, both domestically and internationally. Reviewers specifically cited strengths in project planning, a focus on key deliverables, and responsiveness to past reviewer suggestions. It was recommended that the primary focus going forward should be on solving critical path issues before scale-up is attempted. Future work should be concentrated on addressing cycle complexity, potentially high capital and maintenance costs, durability under long term cycling, and possible 24/7 operations.

Photoelectrochemical Hydrogen Production: All of the materials R&D projects in this topic area were viewed to be well-aligned with the Program's long-term goals, and reviewers noted that the strong collaborative teaming approach among the projects was effective and necessary for achieving the DOE targets. All projects were cited as having achieved good scientific progress in the range of materials systems under investigation. Reviewers specifically highlighted the new theoretical activities and nano-science approaches as important additions to the research portfolio. It was stressed that further work is needed in evaluating a broader class of material systems, in advancing technology readiness, and in demonstrating technology viability and scalability to large-scale systems.

Biological Hydrogen Production: The projects in this area encompass a portfolio of photobiological and fermentative production methods using various micro-algal, cyanobacterial, and lignocellulosic biomass resources. All projects were highly rated by reviewers. The general consensus was that the researchers are moving toward the DOE goals in this longer-term renewable hydrogen production area. The scientific methods used in the majority of the projects were seen as cutting edge and the collaborations were viewed as effective and productive. Reviewers recommended expanded collaborations, particularly with the DOE Office of Science, and emphasized the need for more feedstock analysis, clearer definition in pathways to technology readiness, and analysis of hydrogen production scalability.

Hydrogen from Coal: The main focus of the six projects evaluated in this topic area was on the development of hydrogen transport and separation membranes for coal-based systems. Reviewers noted progress in all areas and favorably evaluated the projects for their innovation and commitment to meeting DOE targets for flux, sensitivity, cost, and chemical and mechanical robustness. Enhanced collaborations with industrial partners were recommended. The need for further economic analyses was also stressed, particularly since materials cost may be significant in the membrane technologies being developed.

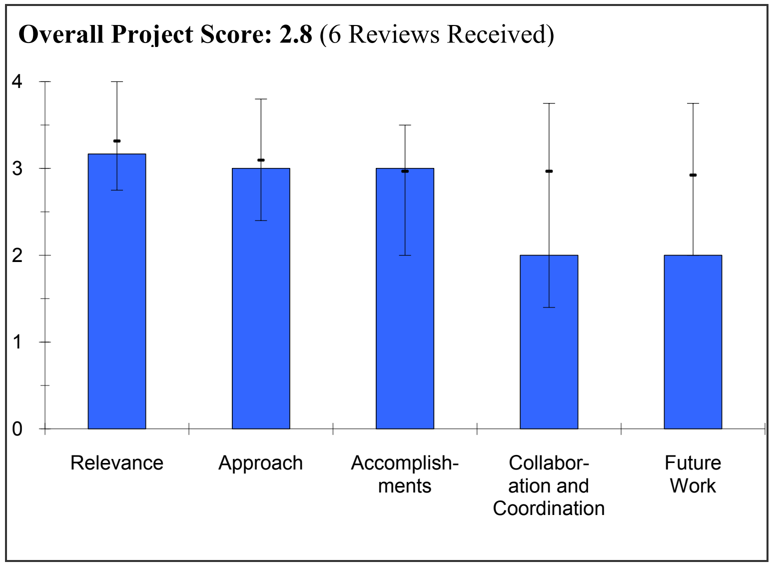
Separations: Projects in this topic area focused on the development and fabrication of several types of hydrogen separation membranes, and according to reviewers, good progress has been made. Projects were generally characterized as innovative, well-designed, and comprehensive in their execution. Reviewers strongly recommended, though, that project teams seek expanded collaborations with metals and materials experts, and with other industry partners that could assist with membrane optimization, economics analyses, and commercialization.

Hydrogen Delivery: Projects reviewed in this area continued to receive high marks from reviewers for the sound progress that has been made, especially in the areas of steel and fiber reinforced composite pipeline technologies. Reviewers complimented teams on their strong technical knowledge and commended the collaboration across industry, national labs, universities, and associations. Specific recommendations were made to better account for cost implications and pressure cycling effects. Reviewers also noted that more work is needed to define real-world, long-term problems that may arise.

Project # PD-02: Biomass-Derived Liquids Distributed (Aqueous Phase) Reforming
Yong Wang; Pacific Northwest National Laboratory

Brief Summary of Project

The objective of this project is to develop bio-derived liquids aqueous phase reforming (APR) technology for hydrogen production that can meet DOE efficiency and cost targets. Specific objectives are to 1) enhance catalyst performance by increasing catalyst activity and hydrogen selectivity; and 2) develop mechanistic understanding of reaction pathways and means to control product distributions consistent with application end-use.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The success of this project would support the DOE objectives for the Hydrogen Program by providing an alternate reforming process that utilizes the inherent moisture content of biomass feeds.
- The APR process to produce hydrogen from carbohydrates is a relatively new approach with potential for distributed reforming of biofuels to hydrogen.
- Hydrogen production from biomass-derived fuels is an important objective to DOE.
- This is a very challenging project due to the complex nature of biomass.
- This project has relevance to the DOE objectives. Low (or lower) temperature APR is an interesting pathway to potentially low-cost hydrogen. Catalyst optimization and understanding is critical.
- The idea seems relevant, but production of hydrogen in remote areas with adequate biomass resources presents a strong challenge in transporting hydrogen to areas of final use. The diffuse nature of biomass resources limits production scale, which compounds the transport problem.
- The project objective is to develop a cost-effective hydrogen production process via reforming of bio-derived liquids. If successful, this would meet the DOE goals for distributed hydrogen production from renewable sources. However, the choice of feedstock and catalyst are critical to achieving the objective.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The research approach for this project is logical and well thought out. The approach addresses the technical barriers and, if successful, it will allow for quantitative comparisons of APR with conventional reforming.
- The APR process operates under comparatively mild conditions that are suitable for distributed biomass processing to hydrogen. The research focuses on the major issues associated with the current APR process, which are insufficient throughput and hydrogen selectivity. The project does focus on the critical barrier in the field.
- The approach is to get a detailed study of platinum-rhenium compared to platinum alone. There is utilization of advanced characterization to understand the role of rhenium. The approach seems successful; however, the addition of a base is necessary to suppress the negative effect of rhenium on the acidity of the catalyst.
- The distribution of products has been greatly improved; however, it is at the expense of a very high residence time.

- Overall, the characterization work is good. The physical characterization work is strong, but chemical characterization (e.g., temperature-programmed reduction (TPR), chemisorption, temperature-programmed desorption (TPD),) is not as strong.
- The balance between characterization and catalyst screening is heavily weighted toward characterization.
- The addition of sorbitol is important for showing process viability.
- The catalyst platform chosen involves noble metal catalysts. For cost-effective large-scale deployment of such a technology, the aim should be to discover and develop cheaper non-noble metal catalysts.
- Much of the work is focused on glycerol and does not address other more relevant feedstocks such as sugars and cellulosic biomass.
- It does not appear that similar work from the University of Wisconsin; Virent Energy and others have been sufficiently leveraged to build on and to advance the APR technology.
- Based on the findings, the work has been appropriately redirected to developing a solid oxide fuel cell (SOFC) application rather than hydrogen production.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Substantial progress has been made; however, since this project is nearing completion it appears that there is still much to be accomplished. Perhaps the remaining process and catalyst issues will be completed in a timely manner, but there is some concern that more time will be needed. The economic analyses to date do appear promising, despite the optimistic assumptions.
- The rhenium-incorporated platinum/carbon catalysts developed in this project showed much enhanced catalytic activity compared to the common platinum catalyst, but decreased hydrogen selectivity. The basic findings on the catalytic reaction mechanism in this project may help the further development of high-efficiency APR catalysts for conversion of glycerol and sorbitol and thus contribute to DOE's goal of utilizing renewable biofuels for energy generation. It is a nice piece of scientific work.
- There is great improvement in fundamental mechanistic understanding of reaction pathways and control of products distribution.
- Network analysis is a good start, but investigators should carry out more complete kinetic analysis of the network for reaction rates if they want a "mechanistic understanding of reaction pathways and means to control product distributions consistent with application end-use."
- Good progress has been made toward understanding catalytic reaction mechanisms. However, some similar results have been previously reported.
- The discovery of KOH (potassium hydroxide) addition and its effect on conversion and selectivity is useful and should help overcome some barriers within the framework.
- The results reported are more of academic and scientific nature and do not sufficiently address the stated objective of developing a reforming process for hydrogen production.
- The results clearly suggest that this approach is more suitable for producing synthesis gas for applications such as SOFC rather than for hydrogen production.
- Given the above, the cost of synthesis gas produced needs to be compared with natural gas or other alternatives as fuel.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Since this project is nearing completion, additional collaborations with a possible end user are lacking.
- Collaboration only on material characterization by high resolution transmission electron microscopy with Oak Ridge National Laboratory (ORNL). Collaborations (or even consultation) with industry would be very helpful for reliable technical and economical evaluations.
- Thanks to collaboration with Pacific Northwest National Laboratory, Brookhaven National Laboratory, and ORNL, a good understanding of the mechanism could be done.
- The reviewer did not observe any outside collaboration.

- The team should partner with a commercial catalyst provider or other entities with experience in practical catalysis.
- Last year, Virent Energy was listed as a collaborator. This year the only collaborator listed is ORNL for some analytical work. It is unclear why the collaboration, and especially the commercialization strategy, is without an industrial partner.
- If the work is redirected as a technology to produce renewable fuel for SOFCs, a corresponding partner in that space would be appropriate for a working relationship.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- It is unclear what future work will be done.
- Perhaps more attention should be paid to the improvement of hydrogen selectivity. The APR reaction mechanism is very complicated and is affected by many factors which are not fully investigated to an extent allowable by the project scale.
- There was no clear proposed future work.
- The project is 80% complete and the last milestone is to demonstrate full conversion of glucose and sorbitol. However, it is not clear how this is going to be achieved.
- Future plans are not stated, so the approach to and relevance of proposed future research cannot be judged. Answers to reviewers' comments from last year are also missing.

Strengths and weaknesses

Strengths
- A good job has been done on defining the process and developing/characterizing the catalysts to be used.
- The APR process operates under friendlier conditions as compared to the traditional high-temperature reforming technology and thus has the potential for distributed biomass processing to hydrogen. The team presented a nice piece of scientific work. The platinum-rhenium/carbon catalysts developed in this project demonstrated significantly enhanced catalytic activity. The findings on the catalytic mechanism and the role of platinum-rhenium alloying may provide valuable knowledge for the further development of high-efficiency APR catalysts for glycerol and sorbitol.
- This is a low temperature process.
- This method allows for the process of biomass without vaporization of feedstock.
- This method uses waste biomass.
- The project demonstrated an extensive characterization of catalysts.
- The team has a good understanding of the mechanisms and how to control product distribution.
- Collaboration exists with other laboratories and universities for characterization.
- A potential linkage to SOFCs exists.
- Various feedstocks are used.
- The project exhibited a strong physical characterization effort.
- The main strengths occur in the development of catalysts, understanding of catalytic reaction mechanisms, and development of approaches to overcome conversion and selectivity barriers.
- The project demonstrated a detailed analysis and characterization to elucidate reaction pathways.

Weaknesses
- More work needs to be done on the potential feedstocks. For example, will feedstocks for this process compete with food crops? Alternate feedstocks need to be explored, and it would be beneficial if some testing of real feedstocks was conducted.
- Hydrogen selectivity may be more important than reaction rate and conversion rate. This is especially true for distributed production where intensive separation and transportation of gas products are likely to be economically disadvantageous. The very low hydrogen selectivity of the Pt-Re-C catalyst may discourage the practical considerations. Also, targeting application for the SOFC system does not seem to be a good strategy

for the intended distributed biomass utilization as SOFCs are considered suitable for centralized power production where bottoming technology could be used to enhance the overall energy efficiency.
- The use of precious metals at high loadings is a concern.
- A high residence time is needed in this process.
- No long-term stability is shown.
- The biggest weakness of the project is a lack of defined metrics for success. A simple listing of the parameters that the researchers think they need to achieve regarding criteria such as selectivity and yield would be beneficial.
- A very small number of catalysts were tested. This project looks like catalysis by chemists with little chemical engineering input. The project needs a stronger emphasis on chemical engineering for kinetic analysis of reaction networks to optimize the network.
- This presentation does not address practical commercial issues. For example, there is no consideration of reactor design or heat transfer. Also, under the optimum conditions for desired conversion and selectivity, the space velocity is too low to be practical.
- Not enough detail is provided on economic analysis to judge the validity of assumptions and to assess practical applicability of the findings from this study in a commercial system.

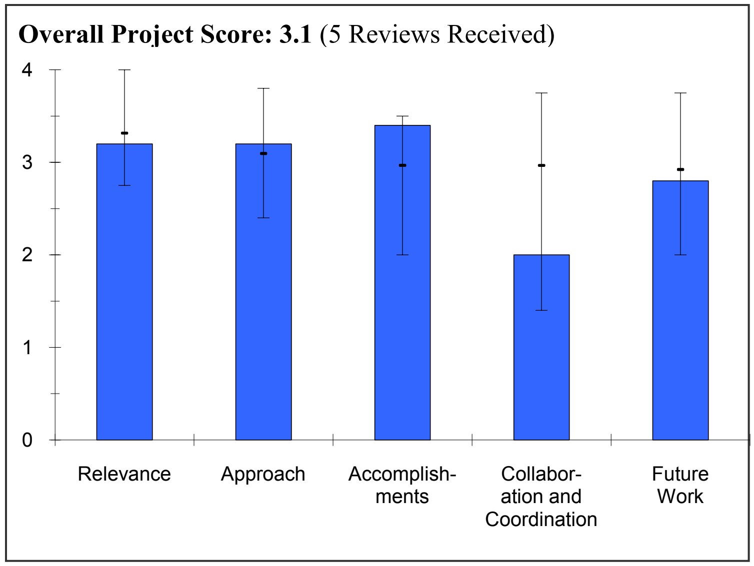
<u>Specific recommendations and additions or deletions to the work scope</u>

- The Project Weaknesses" section above contains many recommendations.
- Selectivity improvement should possibly be prioritized to a higher level.
- Researchers should seek collaborations with potential users of this technology.
- The work needs to address long-term studies (i.e., long-term testing of the catalysts). Also, other basic supports should be considered to decrease the acid character of oxidized rhenium. The project needs to address the role of inorganics present in biomass. If possible, either platinum should be replaced by a cheaper transition metal or the loading should be lowered.
- More work should be done on catalyst synthesis. Researchers need to screen additional catalyst variables including support, total platinum concentration, impregnation method, and reduction temperature and rate.
- A better kinetic analysis is needed.
- Based on the findings as stated, the project seems to have been redirected to develop a process to produce a renewable fuel for SOFCs. The cost and other parameters should then be compared accordingly to see if this is a viable and cost competitive process. The original objective of hydrogen production is clearly not met in the current form.

Project # PD-03: Hydrogen from Glycerol: A Feasibility Study
Shabbir Ahmed; Argonne National Laboratory

Brief Summary of Project

The objective of this project is to evaluate the economic feasibility of producing hydrogen from glycerol derived as a byproduct of the biodiesel industry: 1) for the distributed production of hydrogen and 2) based on the steam reforming of glycerol, followed by purification using pressure swing adsorption. This project will review the availability and price of glycerol, evaluate the hydrogen-from-glycerol process at a distributed hydrogen production facility using systems analysis, and estimate the cost of hydrogen and its sensitivities.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The success of this project would support DOE objectives for the Hydrogen Program by providing an alternative reforming process that utilizes the possibly abundant volumes of glycerol from biodiesel production.
- A realistic and reliable economic evaluation of hydrogen production from biomass (and from any stocks in general) is critically important to guiding R&D efforts for the DOE Hydrogen Program.
- It is important to have independent analysis of the economic feasibility of various processes. Economic analyses performed by individual teams are difficult to compare. Studies such as this will allow a bias-free comparison of different approaches on the same basis.
- Glycerol is a scarce resource. The presenter did a great job in quantifying the resource and he estimated 43 stations that could operate on glycerol throughout the United States. As the volume of production required for low cost is approximately 500 units (according to better assumptions from the Hydrogen Analysis project (H2A) sponsored by DOE), this system would be designed at a significant volume of production disadvantage. There is still potential for it if it leverages ethanol reforming technologies. Glycerol as a feedstock for reforming may be better suited to a fuel-flexible reformer. Typically the fuel feed and treatment system is the biggest difference from one steam methane reformer (SMR) feedstock to another. Thus, the researcher could focus on this type of system aspects.
- This project assumes that the highest value for waste glycerol is to produce hydrogen, which is a false premise. Once the one dollar credit for biodiesel expired, the current transesterification process to make biodiesel from various oils became uneconomical, a number of manufacturing plants closed, and the total amount of biodiesel and glycerol dropped. Currently, the highest value for glycerol, based on life-cycle analysis, is to produce ethylene or propylene glycol, not hydrogen. Since this is more of a niche market, the assumptions appear to be too optimistic. The capital cost and price of crude glycerol are too low and the efficiency of the combined process is too high.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The research to date has been well planned and focused on the feasibility of using glycerol as a hydrogen source.

- Since the cost of hydrogen is sensitive to the price of glycerol, it is important to look at the impact of the hydrogen product (if hydrogen production from glycerol really happens) on the price of glycerol (based on an appropriate hydrogen production scale).
- The project is in its initial stages. The team has chosen to follow the H2A model pretty closely. While this is appropriate for the beginning stages, they may want to consider other options.
- The analysis determines the range of costs to achieve the production targets for hydrogen; however, the PI did not provide any evaluation on pathways to achieve the potential reductions and meet the program goal. This was more of a survey project and has not provided an approach to overcome the barriers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- The results were presented clearly and excellent progress has been made. The conclusions are supported by the research and the numbers appear to be reasonable. It is clear that if the price of glycerol continues to fall, this process becomes economically viable. It would be interesting to include a water-gas shift membrane reactor into the economic evaluation to see if there is a benefit.
- Progress has been very good on this project.
- The process economic analysis was good, but the assumption of $1 million for a system is inconsistent with volume of production (43 systems in total). At a replacement life of 20 years, this equates to two systems produced per year, not500 plants per year as in the Nth plant case}. Additionally, the feedstock cost used was for crude glycerol, which did not consider the cost of cleanup.
- Based on the level of funding, good progress was made to assess if the feedstock could produce hydrogen within the goal of the program. However, there were too many assumptions that cannot be validated to use this analysis as an indicator of what could be achieved.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- This category is not applicable due to the limited scope of the project.
- There is limited collaboration. It would be helpful to collaborate with teams that are working on experimental aspects of the process.
- It would be beneficial to see if this feedstock could be fed into other existing SMR system concepts. Collaboration in the reforming area could facilitate this investigation.
- The PI should have contacted Archer Daniels Midland Company or the National Biodiesel Board to get more facts on glycerol and its value in the products market. DOE has supported a number of analyses and scoping R&D efforts to help the PI provide a more thorough analysis.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- It is unclear if this project will continue or not. The rough outline of future work was somewhat general.
- Proposed future work is appropriate. The team should consider looking at the aspects of the process that can be improved by ongoing research in other DOE-funded programs and incorporating data and results from those studies into their analysis.
- The work should focus more on the economics, availability, and market barriers than on the reforming challenges.

Strengths and weaknesses

Strengths

- This is a well-defined project with logical conclusions.

- Although the project is at a very initial stage, the results show good potential in the economical use of glycerol as a renewable stock for hydrogen production.
- The analysis is based on sound assumptions.
- For a new project with a PI that does not have a great understanding of glycerol, the project was well documented. The approach used some sensible values for categories such as efficiencies and feedstock cost, even though the capacity values were not realistic.

Weaknesses
- The project had no significant weaknesses.
- Partners need to be identified to provide realistic operation parameters, system design, and reforming performance data, e.g., kinetics, conversion, and catalyst cost. More efficient, emerging reaction (e.g., aqueous phase reformation, high-temperature reforming, and membrane reaction) and separation (e.g. membrane) activities need to be included as a case study that may provide useful information for the ongoing and future R&D efforts.
- The project should perform sensitivity analyses of the factors that contribute to the cost.
- The PI did not appear to have much understanding of the biodiesel and glycerol market issues.

Specific recommendations and additions or deletions to the work scope

- If this project continues, it would be interesting to include a water-gas shift membrane reactor into the economic evaluation to see if there is a benefit.
- It would be very helpful to the R&D community if the PI could include some important developing or emerging technologies (e.g., membrane reactors, and membrane separations) in the assumed production flow chart to see their economic validity.
- Uncertainty about future funding makes it difficult for the team to plan their future approaches.
- It would be good to see how glycerol could be used as a fuel additive for other processes. The quantity available does not seem to justify development unless other pathways are identified for glycerol production (e.g., biological or bacterial)

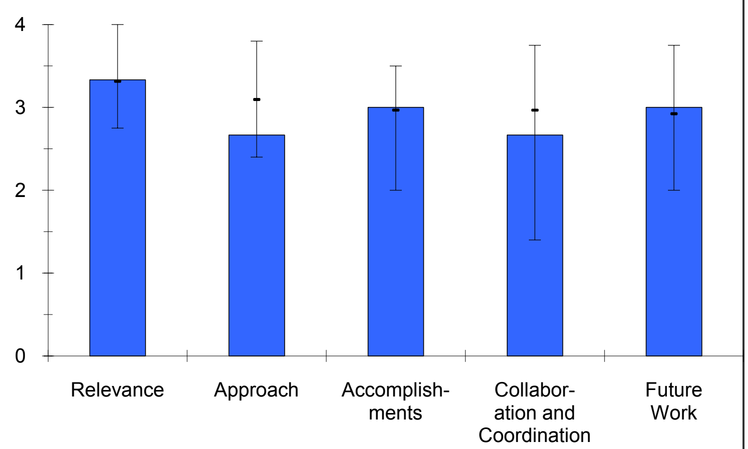
Project # PD-04: Distributed Bio-Oil Reforming

Stefan Czernik; National Renewable Energy Laboratory

Brief Summary of Project

The overall objectives of this project are to 1) develop the necessary understanding of the process chemistry, compositional effects, catalyst chemistry, deactivation, and regeneration strategy as a basis for process definition for automated distributed reforming and 2) demonstrate the technical feasibility of the process. The objectives for fiscal year (FY) 2010 are to 1) demonstrate catalytic partial oxidation/steam reforming of bio-oil to syngas at bench scale, 2) demonstrate long-term catalyst performance, 3) provide mass balance data for the H2A project, and 4) make a "go/no-go" decision.

Overall Project Score: 3.0 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Developing an advanced reforming approach to produce hydrogen from "bio-oil" derived from pyrolysis of biomass supports the Hydrogen Program goals. Such an approach, if successful, could have an important impact on renewable, distributed hydrogen production.
- The project is well-aligned with the DOE RD&D objectives.
- It is not clear that hydrogen production is the best use of pyrolysis oil.
- Stability issues may make transportation difficult.
- Environmental and toxicity issues associated with pyrolysis oil at a forecourt location, as described in this work, are significant enough to preclude its use in forecourts.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- A two-step oxidative cracking/catalytic steam reforming process for producing hydrogen from hydrocarbon oils is a sound technology approach.
- The reviewer is unclear on whether the use of precious metal catalysts is a concern and if an investigation of alternatives is planned. The formation of carbon species (lightweight, solid particles) is a concern. The team should look into this and try to get a better understanding of the reaction network and product distribution.
- The approach appears to be essentially Edisonian with little or no guidance from theory or fundamentals. Hence, many more catalysts should have been tested or process variables should have been explored much more extensively.
- No catalyst characterization was included.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The project has met many of the reported milestones and has made good progress despite having lost all funding for FY 09.

- The team is tackling a challenging process quite well. So far, the progress has been substantial.
- Progress in the last year seems minimal. The main accomplishment seems to have been a 2-1/2 day run, a couple of new catalysts, and minor temperature variation. This is not a lot of progress for $500,000.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- The project requires collaboration with multiple entities. Two universities are providing expertise in cracking and catalyst development. A collaboration with an oil company is providing help with feedstock effects.
- The collaboration with the partners may be better defined. For example, what is the role of the University of Minnesota versus BASF Corporation?
- Cooperation seems limited to obtaining three catalysts from outside sources, including one a commercial catalyst.
- Chevron and Colorado School of Mines collaborations were not discussed in any detail and no impact on the work was mentioned.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- It is a good idea to focus on testing of catalyst life and obtaining mass balance data for use in analysis before proceeding forward with a prototype. A detailed well-to-wheels systems analysis will be required since the technology requires a number of steps.
- It may be overly ambitious to go to a prototype system in 2011. The team has several more questions that need to be addressed at the bench scale.
- The team needs to look for a cheaper catalyst. Just because nickel did not work once does not mean it cannot work. A United Technologies Research Center program had a similar result, but they modified their nickel catalyst to make it work. This type of catalyst modification is simple, allowing catalysts to be easily modified one day and screened the next.
- Long-range testing is a good direction. The PI should calculate the effect of run life on process costs in determining "how long is long" and carry out tests for a relevant length of time.

Strengths and weaknesses

Strengths
- The project used excellent facilities, R&D infrastructure, and a qualified staff to conduct the project to completion.
- The team chose a good research area. Bio-oil from biomass pyrolysis could become an important and prolific energy source.
- The progress on bench scale process is impressive.
- The system is giving reasonable conversions.

Weaknesses
- The use of methanol as a diluent is a weakness. On a large-scale use model, methanol supply and distribution becomes a problem.
- The team should not move to a scale-up stage too fast. The project's future plans may not be realistic.
- There has been a lack of accomplishment. The project has no apparent efforts on catalyst fundamentals, though for the amount of money invested between 10 and 100 catalysts should have been tested instead of only three.

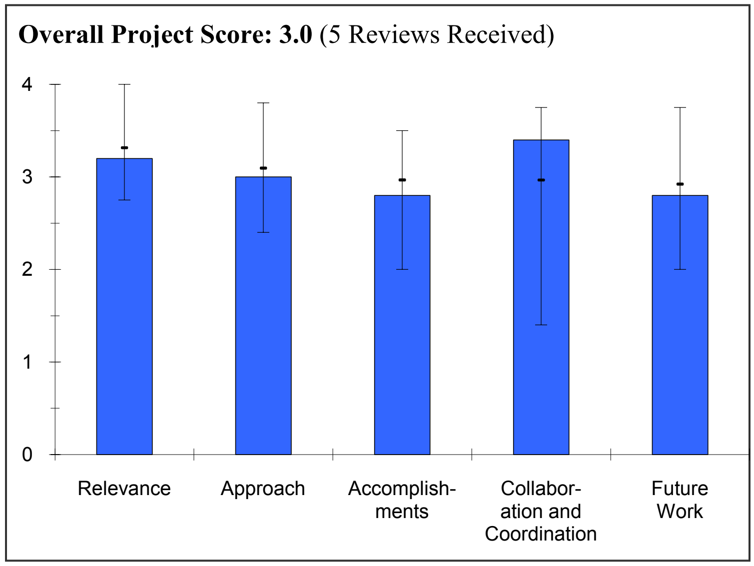
Specific recommendations and additions or deletions to the work scope

- The project should continue to be funded.

- This project needs to systematically study bio-oils of different qualities and types derived from different feedstocks. A task should be added to investigate alternative means for bio-oil stabilization other than alcohols. For example, hydro pyrolysis front-end processing could be used instead of solvent addition.
- Long-term testing is necessary. Two days is not enough to see poisoning effects.
- The project should include a detailed feed analysis for various feedstocks with attention to contaminants (e.g., sulfur, nitrogen, chlorine, metals). This analysis should identify which feedstocks would pose problems for catalyst poisoning, corrosion, etc.
- The project should detail the costs throughout the value chain and identify the most promising candidates to cut total cost.

Project # PD-05: High-Performance, Durable, Palladium Alloy Membrane for Hydrogen Separation and Purification
Ashok Damle; Pall Corp.

Brief Summary of Project



The overall objective of this project is to develop, demonstrate and perform an economic analysis of a palladium alloy membrane that enables the production of 99.99% pure hydrogen from reformed ethanol at a cost of less than $3/gallon gasoline equivalent. The objectives for the past year were to 1) continue optimization and characterization of the membrane formation process, 2) conduct extensive testing of palladium alloy membranes in pure gas streams and in syngas/water-gas shift (WGS) reaction environments for parametric evaluation of their performance, 3) demonstrate membrane performance milestones for Phase III "go/no-go" decision, and 4) complete the techno-economic modeling in collaboration with Directed Technologies, Inc. to determine the influence membrane parameters have on the cost of hydrogen production.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project is critical to the success of the Hydrogen Program. The development of robust, inexpensive membranes is necessary to meet DOE's RD&D objectives.
- Production of pure hydrogen is critical for the use of fuel cells.
- Membrane separation is only one approach to purifying hydrogen for fuel cell applications.
- Relevance is fair. There are purity and pressure issues when membranes are used for separation. In most cases membranes do not ensure 99.99% pure hydrogen over lifetime and membranes reduce the pressure of hydrogen product gas.
- DOE seems focused on membranes as the leading separations approach,
- Palladium membrane implementation is under investigation by others and is especially useful for operation in WGS environments. Palladium-gold alloy membranes show improved performance and durability within cost targets.
- Improving the cost effectiveness of ethanol reforming for hydrogen production is in line with DOE goals. It would be helpful to emphasize module reactor development in addition to membrane development while keeping commercial targets in mind.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The team's approach for this project is excellent. The approach addresses the technical barriers and, if successful, will result in a significant progress in membrane technology.
- Approach for development, testing and cost analyses are well defined. Work on membrane characterization after impurity testing is needed to better understand how to improve membrane tolerance.
- Investigating the full performance spectrum, including flux, durability, and cost, is a good approach.
- Looking at the impact of impurities is a critical task.

- Consider porous metal substrate as a membrane support.
- Including integrated membrane reactors for system intensification, is critically important to reducing cost.
- Membrane design and fabrication is well-conceived, but the performance testing is not well defined. There seems to be significant variation in the performance level requirements for various tests. Sensitivity of hydrogen recovery or hydrogen purity to pressure differentials and flux rates is not documented. Testing to demonstrate performance targets should also be performed at other target levels. Alternatively, target parameter sensitivity to other performance parameters should be documented.
- Suggest developing a protocol for performance testing.
- The approach is well thought out and is focused on the key barriers, with the exception of addressing the manufacturing cost barrier.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Great progress in membrane development and economic analysis. Thinly coated membrane material on a porous substrate is a solid approach. Polarization research and the economic analysis comparing the flux, cost, and percentage of hydrogen recovery is very important.
- Progress has been made evaluating reaction environments, and determining the effect of carbon monoxide, hydrogen sulfide, and steam content in the feed gas on the hydrogen flux. Techno-economic modeling and determining maximum operating conditions was completed.
- 500 hours of stability testing is not sufficient to determine the life of a membrane with a 5,000 hour lifespan.
- Need more information on actual durability results because it appears that the flux is degrading with time. Why do stability test show that hydrogen purity is 99.8% after 500 hours?
- Technical progress seems limited and improvements marginal since the last review.
- Actual cost analysis information is needed including a comparison with pressure swing adsorption (PSA)., PSA, including the pressure loss effect, is the current industry workhorse.
- Results appear unclear at this time. Results seem to be a combination from several different membrane types and test conditions . The PI needs to look at one membrane test setup and one set of test conditions.
- The cost analysis focuses on hydrogen recovery performance in the range of 70%- 90%. The target recovery is greater than 80% in FY 10 and greater than 90% in FY 15. Documented performance in long term tests is only about 67% recovery and one test showed recovery of 82%. The performance table dated FY 09 (but presented as "Accomplishments" for FY 10) showed recovery greater than 60%. There was no clearly documented test at target levels showing recovery levels in accord with the FY 10 target. This deficiency could be a fault of the presentation or it could be because no appropriate testing was done. Nevertheless, the choice of hydrogen recovery as the cost sensitivity parameter demands that hydrogen recovery performance be adequately documented.
- The lack of a well-defined protocol for performance testing inhibits understanding of progress toward resolving issues and overcoming barriers.
- Progress has been slow. Much of the FY 10 presentation was presented in the previous AMR presentation. Significant progress has been made however, the results appear mixed. While durability and resistance to impurity improvements are demonstrated, the flux and hydrogen recovery values are inconsistent. It is not clear what the optimum operating mode (e.g., integrated WGS), operating conditions (e.g., temperature and pressure), and performance should be.
- Results for flux and hydrogen recovery values are confusing and attributed to different experimental conditions. It is unclear if there is a specific value or range under typical operating conditions (e.g., slides 10 and 11). Slide 10 indicates 78% hydrogen recovery is notable but no temperature conditions are noted. Compared to hydrogen recovery rates using PSA, 78% is not impressive.. On slide 11, #4 uses 90% recovery at a lower flux. The recovery and flux values reported on slides 13 and 14 need clarification.
- No details were provided on how the cost estimating was done. Information should be provided on the mode, operating conditions, flux, and recovery values that were used in the techno-economic analysis to arrive at the $2.99/kg of hydrogen mentioned in slide 11, #7. A comparison between this separation process and PSA as a base case needs to be done.
- The statement on slide 8 that any improvement in the separation has minimal effect on the overall cost of hydrogen is a concern. It suggests that efforts on this approach are not worthwhile. The statement, "Greater

membrane recovery however significantly reduces the cost of hydrogen," needs to be quantified and compared to demonstrated value.
- The results of the techno-economic analysis show that ethanol conversion efficiency is critical. More information is needed to show how the membrane separator influences the conversion efficiency.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- The collaborations with the Colorado School of Mines, ORNL, and others are excellent. The addition of a potential end user is critical.
- The project had very close collaboration to address the different aspects of the development of a commercial membrane. From membrane fabrication to cost analysis, all of the issues are being investigated.
- This project includes a good list of collaborators with right skill sets.
- The project would benefit from collaboration with a PSA manufacturer or industrial gas company as the end-user.
- The work performed by partners was not specifically identified in the presentation. However, material characterization by ORNL was referenced in the oral presentation. Materials science work at Colorado School of Mines was not identified. Additional detail on collaboration will help promote access by other program efforts to specific and unique skills and facilities.
- Collaboration efforts seem appropriate and well-coordinated. The presenter stated that an end user has been identified. End-user participation is critical in developing a meaningful product.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The proposed future work is detailed and well thought out.
- Future work is very well defined. The project is only 65% completed and is already in the stage of scaling up and collaborating with end users.
- The cost of hydrogen seems to depend on developing a good strategy for increasing hydrogen recovery
- A lot of work remains to determine commercial viability of this approach.
- More emphasis is needed to ensure consistent experimental design for example, the targets should include the costs measured for 5,000 hours an end-of-life target purity of 99.99% , as well as the additional cost associated with loss in membrane pressure.
- Testing protocols need better definition to assure that material components will meet target performance levels and do not remain a barrier to implementing scale-up design and fabrication. The plan to scale-up to fabrication plan is good assuming appropriate membrane material characteristics have been defined..
- Techno-economic analysis results indicate Phase III work, as described, might not be justified Unless there is clear value demonstrated for the efforts, it would not make sense to proceed to the next phase.
- Without results incorporating the membranes in the reactor (i.e., WGS) or the reformer, this approach does not appear to offer sufficient economic benefit. The plans, as described, do not seem to include any actual testing of a membrane reactor.

Strengths and weaknesses

Strengths
- To date, the researchers have done an excellent economic analysis detailing the strong influence that percent hydrogen recovery has on the cost of hydrogen compared to other factors such as membrane cost and permeance (flux). In addition, the recognition of and the subsequent addressing of the potential boundary layer/polarization effects will be valuable information for membrane development.
- Long-term evaluation of the hydrogen flux in the presence of WGS products has been accomplished.
- A determination on the effect of impurities (hydrogen sulfide, carbon monoxide, water) completed.
- A determination of the cost has been accomplished.
- The membrane is being designed for high pressure changes and high levels of impurities

- This project is comprised of a good technical team with lots of good ideas.
- Good overall progress was made on membrane separations as a generic approach.
- The palladium-gold alloy membrane appears to be effective for separation and purification of hydrogen, especially in WGS environments.
- Membrane development, fabrication and manufacturing capabilities are clear strengths of the project team. Collaboration with appropriate entities has added value.

Weaknesses

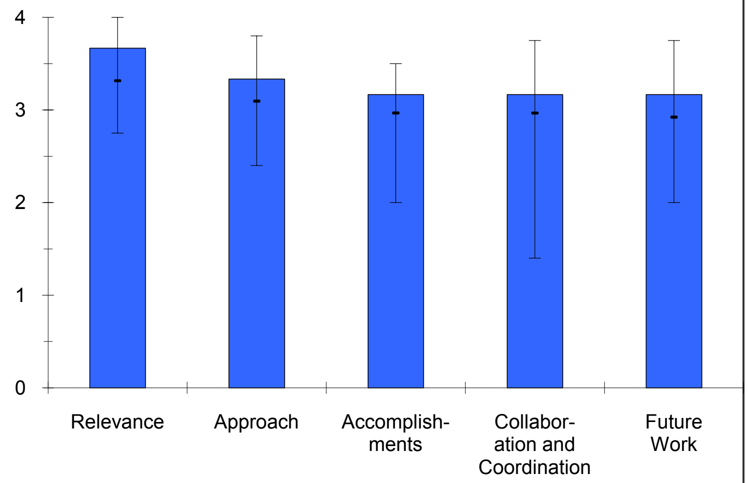
- More extensive testing of membranes needs to be conducted in the presence of contaminants, especially hydrogen sulfide.
- A comparison with commercial membranes is needed.
- The team must understand the mechanism of deactivation due to carbon monoxide and hydrogen sulfide. Conduct postmortem analyses of the membrane and determine if there are any issues with embrittlement.
- A focus on hydrogen recovery is needed.
- An end user has yet to be added
- The team needs to establish the right basis for comparison. For this application it is a PSA system.
- Hydrogen sulfide should be taken care of before it reaches the reformer and WGS reactor since catalysts will likely be deactivated.
- The team needs to get the user industry perspective (e.g., industrial gas companies [and fuel providers) on their criteria for deployment.
- Systematic planning and testing has no logical framework that assures efficient progress toward the project goal. No decision points were identified. To be relevant the test conditions need to be more focused on realistic operating conditions. There should be a systematic effort to optimize operating conditions to leverage the advantages of this membrane
- The economic analysis indicates that using ethanol reforming to reduce hydrogen production costs is questionable

Specific recommendations and additions or deletions to the work scope

- None
- Hydrogen purity analysis should be done to ensure the product meets the impurity concentrations standards listed in SAE J2719 & ISO 14687-2.
- This separation approach and PSA need to be compared for pressure loss and economics.
- Essential individual performance targets for each critical path need to be identified and demonstrated. The team should also establish and document a test protocol that assures that simultaneous target performance levels have been achieved.
- There has been a significant body of work to date on palladium alloy membranes for hydrogen separation and the benefits and limitations are well known. It is clear that unless these membranes are incorporated in the reactor to eliminate the number of unit operations, there would be no cost benefit. The project scope should therefore include this type of testing.
- The team should clearly define “go/no-go” decision points. At present, the results reported suggest that the approach will not be able to meet the stated goal of significantly reducing ethanol reforming cost. In the absence of that promise, the project needs to be reexamined.

Project # PD-06: A Novel Slurry Based Biomass Reforming Process
Thomas Vanderspurt; United Technologies' Research Center

Overall Project Score: 3.3 (6 Reviews Received)



Brief Summary of Project

The objectives of this project are the 1) development of an initial reactor and system design, with cost projections, for a biomass slurry hydrolysis and reforming process for hydrogen production; 2) development of cost-effective catalysts for liquid phase reforming of biomass hydrolysis-derived oxygenates; and 3) proof-of-concept demonstration of a micro-scale pilot system based on liquid phase reforming of biomass hydrolysis-derived oxygenates.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Hydrogen production from biomass, or other renewables, is critical to the Hydrogen Program.
- Reducing the hydrogen production cost is critical to the long-term viability of these technologies.
- Hydrogen production from renewable sources such as biomass is a key component of the DOE Fuel Cell Technologies Program.
- This project addresses renewable hydrogen production with potential application to distributed production of high-purity "fuel-cell"-grade hydrogen. The requirements of a very high pressure reactor with corrosion-resilient steels, need for caustic recycle, build-up of contaminants and solids in the process, and uncertainty of contaminants in downstream palladium separator may be barriers that are difficult to overcome. This may prevent commercial implementation.
- This project clearly supports the DOE Hydrogen Program goal of producing low-cost hydrogen from renewable feedstocks. The project needs to consider focusing on renewable feedstocks that are available in the United States and are sustainable at large scales.
- This process was very well communicated this year and it shows very good progress by the team.
- This project is relevant if hydrogen is to be produced from renewables. An extremely large range of capital and production costs exist due to membrane, catalyst, and high reactor cost. A more detailed analysis should be done to improve the accuracy of the cost estimates.
- Although the reactor design has safety factors that exceed the expected operating temperature and pressure, there is still concern of metal fatigue over extended operation. Accelerated coupon testing should be done to prove there would not be a safety issue.
- Although the Raney catalyst technology is well developed, a concern of loss in performance after repeated cycles of pressure and temperature exists. This should be validated in any follow-up work before scaling up.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The biomass slurry-to-hydrogen concept seems to be a working technical approach. The costs for the palladium membrane currently seem excessive, however, due to the conditions where the liquid phase reforming take place and the high degree of technical integration required. Due to the cost, the secondary approach for gasification has to be examined.
- The base hydrolysis has been shown to work, but the complexities and high cost associated with this approach are present.

- The project was originally proposed to use acid catalyzed, but now uses based facilitated reforming. The use of a base is established in the literature for processing biomass. The key challenges will be recovering the catalyst and the unreacted base. The project investigator mentions these issues in the future work section, but they are not to be covered until the last year of the project. Base recovery should have begun earlier.
- The team is using an expensive metal membrane for hydrogen recovery and metal membranes are known for failure. They should have provided analysis to indicate that the metal membrane would be less expensive than traditional pressure swing adsorption (PSA) for hydrogen recovery. The team's approach does not include examining the performance of the metal membrane with the very aggressive base (potassium hydroxide [KOH]) and with the expected products from the wood processing. Ethanol is a much cleaner and easier-to-use material.
- Autoclaving cellulosic biomass in caustic media at high pressure is not a new concept. Antal et al. at the University of Hawaii, for example, achieved similar results (high biomass conversion to hydrogen) with a similar approach using a supercritical gasifier. The team should seek some collaboration for this project.
- Much work was done on base-lining autoclave work on alcohol reforming and not much time remains for the biomass solids, which should have been the focus of the project. This is where the most value to the program would have been gained.
- Bench scale testing of individual components (hydrolyzer, reformer, and membrane) of the integrated system might be worthwhile at this early stage. However, the project investigator needs to consider testing the whole system together to prove the concept from an integrated system standpoint. The earlier this can be done, the more credible the cost estimates will become. For example, testing with a furnace to provide heat for the endothermic reaction will not give one the answer on how to integrate a burner into the system. Issues with impurities will not be addressed by testing with ethanol as a reformer feed.
- The project is following in a very good path to designing a complete system. Details were addressed besides design components (e.g., the reliability impact of palladium membranes). The process is in line with practical implementation.
- The project has an excellent scope of work to address an alternative approach to thermochemical conversion processes. Good progress against the catalyst and selectivity issues has been made.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- This project has demonstrated excellent conversions of synthetic and real feedstocks.
- The use of a base to facilitate wood processing is not a new concept nor is the use of supported Raney nickel. Both can be found in the literature.
- The team is making progress towards the flow reactor.
- Membrane demonstration for less than one hour is nice but not impressive. The membrane represents a large cost for their proposed system and its replacement will have a significant negative impact on the cost analysis. The team needs to demonstrate that they can run the membrane for thousands of hours with a gas mix that represents the expected products from their reactor, including impurities and the potassium hydroxide KOH base
- The team needs to demonstrate KOH recovery, which will be a significant challenge.
- The project investigator has demonstrated the basis of the technology and addressed several issues, such as catalyst type, that would be cost barriers. Many barriers and issues remain, such as corrosion and caustic recycle. The effects of contaminants also need to be addressed.
- Though hydrogen and capital cost projections appear to be close to the DOE targets, this reviewer has serious reservations that these numbers will hold true once all of the issues are identified and resolved. For instance, the caustic corrosion, metallurgy, and contaminations issues often drive the cost prohibitively high.
- The work on the flow reactor with integrated system shows good progress so far. Strong progress has also been made toward hydrogen cost reduction. A palladium membrane cost reduction from 85% of the capital cost to 50% at the current stage shows some promise. However, it still represents a large portion of capital cost that could jeopardize the economic viability of the integrated process.
- Very good progress has been made to date, but more details on the separation process should be shown. The team appears to have addressed most of the issues with the palladium-copper membrane and it would be beneficial to see some of the information behind it.

- Strong progress has been made on milestones for selectivity, catalyst selection, and yield as compared to 2009. Too much range of cost and performance still exists to scaling up.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Most of the collaboration is being provided with hydrogen separation membranes.
- The project has some collaboration, but the team needs to be more specific in their presentation on what the partners are doing.
- Collaboration with Energy & Environmental Research Center (EERC) to do autoclave optimization testing is a plus. EERC has excellent facilities and support staff. Consulting with the forest products industry or similar industries with experience in alkali pulping, biomass feedstocks, and other areas could be advantageous.
- The project has good collaborations with various entities. The team should work with the H2A project model to refine cost as more information is obtained from testing.
- This project went through a competitive process development and a lot of collaboration was not possible. However, the team does not appear to have been working in vacuum, as they have achieved great technology incorporation into their process.
- The team had some collaboration with the University of North Dakota. However, there is not much detail on scope, communications, coordination, and review of the results.
- Clear identification of roles and responsibilities should be present as applied to the reporting of results and scale-up.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- Moving the catalyst to nickel seems like a move that will be unsuccessful, unless this approach will also incorporate regeneration schemes. Sulfur poisoning of nickel is well known, as is carbon formation in heterogeneous gas phase catalysis. Thus, this may work for the pure synthetic feedstocks, but poisoning is very likely with real biomass.
- In terms of cost, reductions are more likely in dealing with the palladium membrane and the caustic base hydrolysis.
- The team is beginning to look at caustic recycle, which is good. However, this should have begun earlier.
- Durability testing will be very interesting. This test should include the membrane.
- This project is worth continuing to the end of this phase to complete lab prototype testing to obtain better data for use in process economics assessments. The project investigator is aware and sensitive to cost issues and has structured a future test plan accordingly.
- The project investigator needs to consider addressing issues with scale up, reactor metallurgy at high pressure, temperature, caustic conditions, and feasibility of using Raney nickel at a larger scale. The investigator should also consider focusing on heat integration.
- The proposed work does address information from previous work; however, it still does not identify who is performing the research and development. It also does not explain the strategy if the catalyst does not scale and the prototype fails the durability testing.

Strengths and weaknesses

Strengths

- This project has an interesting approach.
- This project has potential for low-cost hydrogen production.
- It is leveraging information and technology development from other DOE projects such as membrane technology, which was developed under DOE's Fossil Energy program.
- The project uses excellent facilities and staff for conducting this type of research.
- The proposed recycle and durability study is critical to project success.
- Good fundamental research and development is used to understand reaction mechanism.

- The project has a good research and development plan to evaluate the hydrothermal conversion process.

Weaknesses

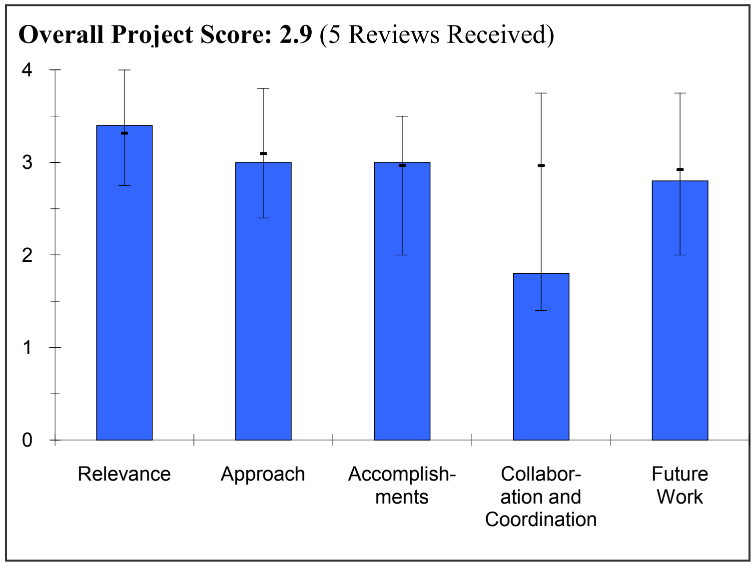
- The development of base metal catalysts needs to examine the poisoning issues with real feedstocks, not just the pure feeds. The cost of the metal catalysts seems tertiary to the overall cost compared with the palladium membrane and the caustic hydrolysis steps.
- The work is not extremely innovative. For example, the use of bases to facilitate the reaction is known and Raney nickel is a well-known catalyst.
- The team needs to demonstrate caustic solution recovery, which will most likely be more difficult than what they expect.
- The cost advantage of using the palladium membrane over traditional PSA has not been demonstrated.
- It is not clear how carbon balance was achieved. This data needs to be reported.
- There are huge hurdles which could prohibit implementation, including the requirement of a high-pressure reactor with corrosion-resilient steels, the need for caustic recycle, and buildup of contaminants and solids in the process.
- This project has a very aggressive schedule. It is unknown if all of the issues can be satisfactorily resolved in six months or that adequate testing can be completed to address technical feasibility related to all of the concerns.
- It appears that the United Technologies Research Center (UTRC) elected to use palladium membrane for purification. However, this appears to contribute to a large portion of the capital cost and also represents a higher technical risk with this particular application.
- Limited data was presented on the different runs. It is unclear based on what was presented if the experimental design had to be redefined due to problems or poor performance. It is unclear what the risk for completion is based on current progress.
- No clear roles and responsibilities are provided for EERC and UTRC.

Specific recommendations and additions or deletions to the work scope

- The team should perform a cost analysis on using the palladium membrane over PSA.
- Continue funding to the end of current project, but future funding should depend on economic evaluation.
- The team needs to look at variations in feedstock composition and impurities and determine their effect on catalyst performance.

Project # PD-07: Composite Pd and Alloy Porous Stainless Steel Membranes for Hydrogen Production and Process Intensification

Yi Hua (Ed) Ma; Worcester Polytechnic Inst.



Brief Summary of Project

The objectives of this project are to 1) synthesize composite palladium and palladium alloy porous Inconel membranes for WGS reactors with long-term thermal, chemical and mechanical stability with special emphasis on the stability of hydrogen flux and selectivity, 2) demonstrate the effectiveness and long-term stability of the WGS membrane shift reactor for the production of fuel-cell quality hydrogen, 3) research and develop advanced gas clean-up technologies for sulfur removal to reduce the sulfur compounds to less than two parts per million(ppm), 4) develop a systematic framework towards process intensification to achieve higher efficiencies and enhanced performance at a lower cost, 5) perform rigorous analysis and characterization of the behavior of the resulting overall process system as well as the design of reliable control and supervision/monitoring systems, and 6) assess the economic viability of the proposed intensification strategy through a comprehensive calculation of the cost of energy output and its determinants (e.g., capital cost, operation cost, fuel cost), followed by comparative studies against other existing and pertinent energy technologies.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project addresses barriers related to long-term stability and hydrogen flux targets for hydrogen separation membranes. Therefore, it is relevant to overall DOE objectives.
- This project needs an additional advanced sulfur cleaning unit (see slide #4) for the membrane developed in this project to work.
- High-flux membrane development is very relevant to hydrogen production.
- The work is focused on realistic and required performance specifications. The team should strengthen economic targets and better communicate how the work is aimed at meeting these targets.
- The project is comprehensive for development and testing of palladium and palladium alloy membranes for hydrogen separation. It is relevant to the Hydrogen Program as well as the goals and objectives in the multi-year plan.
- This project is relevant to the DOE Hydrogen from Coal Program's advanced concept/process intensification objectives by eliminating several unit operations and replacing them with the WGS step. The process concept could enable high-purity production of hydrogen from a gasifier gas stream in a single step assuming contaminant removal steps can be done successfully, though this is a big assumption.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach in this project is not clear. It appears this team is fabricating a number of membranes of the same composition and testing them, but it is not clear what is changed between membranes (except probably thicknesses). This approach sounds Edisonian. No attempt is made in developing sulfur- and carbon monoxide-tolerant membranes. These are the primary weaknesses of this project.

- These tests seemed to be targeted to reproduce the attractive results achieved in previous tests, but they did not seem targeted towards moving the project forward. Cycling tests and the use of larger membranes would add value, as would tests on membranes that were sulfur-tolerant. Also, the membranes tested were largely thicker than the ones targeted to meet the cost objectives of the project.
- This work represents very good technique and is at the leading edge of dense metal membrane development. The reviewer ranks this work in the top 5% of palladium membrane development worldwide. However, a few shortfalls remain that should be addressed.
  - The reported results are from a collection of different, individual membranes. This suggests that one membrane sample (or possibly a reproducible set of membrane samples) is not capable of achieving all the reported technical progress metrics (i.e., flux, selectivity, durability, and economics). The weakness here is that the technology appears to be immature, suggesting that transfer from the lab to industry may be exceedingly difficult or perhaps impossible. The project investigator should be reminded that a low degree of reproducibility represents low manufacturing yields and consequently higher cost.
  - Economics need to be addressed, not only in the abstract (e.g., against DOE targets) but also in the real world (e.g., competing solutions). The project investigator stated that the goal is to achieve a robust membrane in the thickness range of about 7-8 microns, though current results for WGS use membranes that are approximately 18 microns thick. Thus, the project investigator should provide a critical economic comparison with other dense metal membranes, which include three classes: 1) dense drawn tubes, 2) rolled planar foil, and 3) palladium alloys deposited on a porous tube. It is virtually impossible to assess economic viability without this type of comparison.
- For mixed gas membrane tests, a plot of flux versus hydrogen recovery (rather than space velocity) would add value to the project.
- Any results of poisoning experiments (sulfur tolerance) must report the hydrogen recovery as well as flux and inlet sulfur concentration.
- The project approach is good for developing and testing membranes that address the barriers of membrane selectivity and hydrogen flux.
- The technical work plan for the project is well-designed, comprehensive, and considers all of the relevant issues to test the feasibility of the concept at the laboratory scale. The fundamental studies contribute to the state of the art.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Higher fluxes are obtained only in ideal gas conditions.
- The membrane is not tolerant to even very low concentrations (less than2 ppm hydrogen sulfide) of sulfur. Flux is inhibited by the presence of even about 1% carbon monoxide, which is a serious problem for the real-world application of this membrane.
- This group has shown similar, good results with this type of small, palladium-coated membranes for the past decade. The reviewer would like to see more tests with larger membranes or with membrane modules, as well as tests with membrane coats that were more likely to withstand cycling or sulfur. Finally, progress on commercialization is absent. Membranes and results in the 2010 report are virtually identical to those in the 2009 report.
- This project is representative of the best metal membrane work being done at this time. The project investigator has demonstrated steady progress toward the goals and a clear understanding of how to continue this trend.
- The project has successfully achieved hydrogen flux of 359 standard cubic feet per hour per square foot (scfh/ft^2), which exceeds DOE's 2015 hydrogen flux targets. It successfully addressed the barrier of developing steady state and unsteady state membrane reactor modeling simulations. The project also developed simulation models that are useful for predicting membrane performance over a range of conditions. The project obtained good long term hydrogen selectivity over a period of 147 days.
- Substantial progress has been made toward completing the testing of the membrane in long-term tests using simulated gas mixture streams.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- A gasifier partner is not included in this work.
- The membranes are made in house by graduate students and are tested in house only by these graduate students. Based on the present year and 2009 activities, it appears that the only route to commercialization of these results is if the graduate students continue to pursue work in this area.
- The project investigator reports that only one collaborator is involved, Adsorption Research Inc. (ARI). The project lacks a collaborator who can provide module design input and commercialization/industrialization knowledge. This is a significant weakness that should be overcome.
- The project does not demonstrate collaboration or communication with the coal gasification industry. It does not appear to have internal expertise in scale-up, plant equipment, or economic analysis.
- The project has identified a key industrial partner who is making substantial progress in the upfront pressure swing adsorption (PSA) clean-up step (to remove sulfur), which is critical to the technology. No other collaborators are mentioned.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- No plans are presented to study the mechanical durability of the membranes. Hydrogen embrittlement is an issue for palladium membranes.
- Thin membranes have pinholes and, therefore, the selectivity is decreased. The future plan is to make thinner membranes, but the method for fabricating defect-free thin membranes is not explained.
- The project investigator reports a sound strategy for dealing with sulfur tolerance. However, results need to report hydrogen recovery in the presence of sulfur since sulfur will be concentrated on the feed side of the membrane as hydrogen permeates it. The plan lacks a clear and compelling strategy for reducing the membrane thickness from 18 microns to the target of about 7 microns, which is key to achieving competitive economics. Little information was provided on how this will be done without sacrificing selectivity and durability. The success of the project is pinned to this question.
- No evidence exists of a plan to test with a coal-based feed stream. Future work needs to identify the barriers associated with scale-up and a plan to mitigate them. The project indicated it plans to initiate an economic analysis but did not commit to complete an economic viability analysis for commercialization.
- The project is focused on the barriers and plans to conduct economic evaluation before proceeding through into a follow-on phase.

Strengths and weaknesses

Strengths

- A good number of publications are presented.
- This project trains graduate students and post-doctorates.
- This project performs good fabrication and testing of palladium membranes.
- The project presents great flux, durability, and selectivity for small membranes with pure gases and no cycling.
- The project demonstrates a good track record of steady membrane improvement, which is mostly due to significant advances in porous support structure and development and palladium deposition process.
- Project strengths include good membrane testing procedures. Membranes have been developed that exceed DOE flux targets for 2015 and selective hydrogen permeabilities have been demonstrated for long periods of time.
- The project made a good choice in partners. The team demonstrated comprehensive testing capability and considerable progress toward targets. The project is very well organized and addressees the major issues.

Weaknesses

- The membrane is not tolerant to few ppm levels of sulfur.
- Membrane flux is inhibited by the presence of 1% carbon monoxide in the gas stream.

- No plans are presented to study mechanical durability.
- No techno-economic analysis is provided.
- A gasifier partner is not included in this project.
- It is unclear who the end user of this technology would be.
- The project uses small membranes, and different membranes are used to test flux, lifetime, and selectivity. The project did not show any real progress in membrane size, connections, sulfur tolerance, cycling, collaboration, or commercialization.
- The project has a lack of connection to real-world economics. This could be overcome through proper selection of an industrial/commercial partner to help with module design and economic evaluations, relative to competing technical solutions (not only DOE targets).
- The project investigator should be devoting more effort to demonstrating a reasonable degree of reproducibility. For example, report results for 10 nominally identical membranes that are fabricated according to a method that is expected to yield the best membrane performance. It is currently unclear how many meet the expected performance. A statistical treatment of the results (e.g., flux, selectivity, durability, and sulfur tolerance) will provide enormous insight into further required effort.
- There is no indication that the project has plans to interact and communicate with the industrial coal gasification community. A plan is needed to obtain membrane performance data using a coal gasification-based slipstream. Though the project's future work indicated "initiation" of an economic analysis, no plan is presented to complete an economic viability analysis. The project investigator did not discuss plans for significant scale-up of the membrane reactor module.
- The project needs to collaborate with industry partners beyond ARI to be in position to conduct integrated system testing, obtain reliable process economic data, and potentially be in position for technology transfer.
- The team needs to build an integrated system and run pilot tests using real coal-derived gasifier effluents.

Specific recommendations and additions or deletions to the work scope

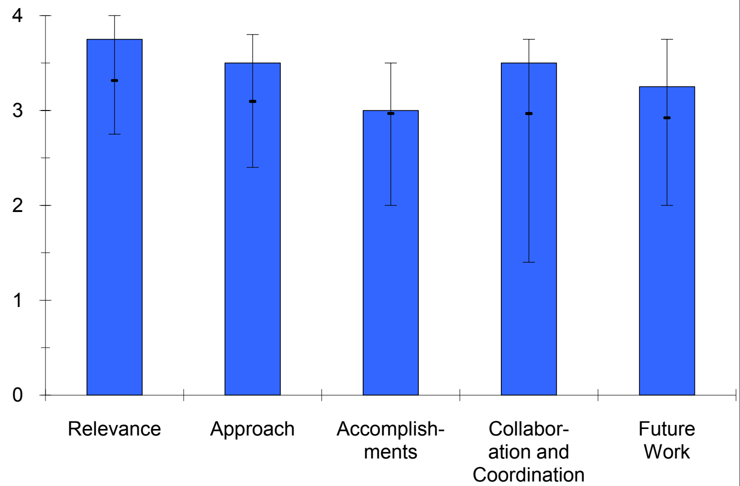
- As per slide #2, the work is 100% completed. The future plans are not well defined and the approach is not clear. Therefore, this project can be terminated. DOE should revisit the project once this team finds an end user to work with them.
- This project is completed. For future projects, it would be nice to see a demonstration of cycling stability with these membranes or membranes made with palladium-silver, palladium-copper, or palladium-gold coats. These are materials that might be expected to withstand cycling. Hydrogen sulfide should be added to the gas. The team should test larger membranes in addition to modules made of several membranes or membranes with much larger surface areas. The team should also increase collaboration with other groups.
- The strategy for achieving suitable thin membranes that are economically viable needs to be explicitly communicated. The strategy should be convincing and compelling.
- The team should bring on a partner to assist with commercialization. In addition, a comprehensive economic analysis should be conducted against the best alternative solutions (e.g., membrane, PSA).
- The team should begin to address reproducibility using statistically significant populations of membranes made according to best methods.
- It is recommended that this project complete an economic viability analysis and design of module(s) for scale-up. The project uses expensive membrane materials (e.g., palladium and gold), and the process may not be economically viable.
- This project should continue to be funded. Besides ARI, the team should engage user industry partnerships to assist in process and economic analysis as well as development of integration approaches as the project moves forward. The project should generate sufficient information at the end of the current phase to be able to design and test an engineering prototype to run pilot trials using gas streams generated by a real coal gasifier. The project team should also be ready to begin technology transfer activity.

Project # PD-08: Development of Robust Hydrogen Separation Membranes
Bryan Morreale; National Energy Technology Laboratory-Office of Research and Development

Brief Summary of Project

The objective of this project is the development of robust hydrogen separation membranes for integration into coal conversion processes, including integrated WGS membrane reactors. Studies suggest that incorporating separation membranes into coal conversion processes can reduce costs by 8%. Task one is the performance testing of external membranes and the National Energy Technology Laboratory (NETL) hydrogen membrane test protocol. Task two is the development of robust metal membranes.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project is relevant given that it is attempting to develop a robust hydrogen separation membrane for integration into coal conversion process. Also, the performance testing of hydrogen separation membranes fabricated by other researchers working in this area are being tested in this project.
- Development and testing of this type of hydrogen permeable membranes is very relevant to the DOE Hydrogen Program goals.
- The work is clearly focused on sulfur poisoning mechanisms and speculates methods to improve sulfur tolerance of palladium alloy membranes. However, little else is being reported that is strategically relevant to program goals.
- Although a fair amount of effort appears to have been directed at defining standardized membrane testing protocols, this is not specifically on task with program targets.
- The overall goal of this project is to develop robust dense metal hydrogen separation membranes for integration into coal conversion processes. This project is highly relevant to DOE Hydrogen Program objectives as well as the goals and objectives of the multi-year plan.
- Hydrogen separation is critical to the Hydrogen Program. This project has established a joint laboratory and university collaboration. Through this collaboration, it has established a hydrogen membrane testing protocol to allow competing developing technologies to undergo unbiased performance verification on a common basis.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The pure palladium and 60 weight percent Pd-40 weight percent Cu (60Pd-40Cu) membranes are tested under different temperature conditions, the former at 450°C and the latter at 350°C. Based on this test result, two types of decay mechanisms, namely, corrosive decay and catalytic poisoning, were presented. It would be valuable to show the effects of the two membranes tested at the same temperature (350°C or 450°C). It would be interesting to see if these two decay mechanisms occur in both cases, or if the "corrosive decay" mechanism is valid only in the case of pure palladium. The experiments conducted to date are unclear. This shows a well-thought-out approach was not followed.
- It is stated in one of the slides that the approach is to develop a multi-layered membrane system that utilizes the catalytic activity seen with palladium sulfide. However, the results show that the flux decreases drastically (from about 12 to less than 4) in 150 hours of testing and the flux continues to decrease beyond 150 hours (slide

#13). The rationale in developing a multi-layered membrane system that utilizes the catalytic activity of Pd_4S is unclear.
- The topic is broad, including the development of robust membranes, but the materials tested come from a rather narrow range of options of binary Pd-Cu alloys. The reviewer would like to see results with a broader range of membrane materials tested over a broad range of temperatures, both with and without sulfur.
- The technical approach proposed to improve sulfur tolerance of palladium-alloy membranes is weak, primarily because the probability for the approach to yield success is low. The team should become intimately familiar with the large body of data addressing coatings on metal membranes. This work has been underway for decades and yet little awareness was communicated in the briefing. In particular, metal coatings are well known to be fraught with problems (e.g., pinholes, cracks, and intermetallic diffusion). Oxide and other inorganic coatings are also known to be problematic. For example, coatings can be dense, pinhole-free coverage is difficult to obtain, and hydrogen permeability is often low.
- The project applies engineering principles, membrane technology, and coal conversion processes to define a sequential protocol for study and testing. This approach provides a better understanding of membrane surface fouling by unwanted contaminants, such as hydrogen sulfide.
- The project has comprehensive experimental test rigs combined with materials and a computational support capability. The research endeavors are focused, specifically on poisons and structural integrity testing.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- A test protocol has been developed and test systems have been modified to allow testing of various membrane geometries.
- Several multi-layered membranes have been fabricated.
- Flux is very low (see slide #19) and dropped to zero in two hours.
- This project included some interesting results on palladium-copper, but the reviewer has seen some of these results before. A broader range of materials tested would have been beneficial.
- Too much of the briefing time was spent describing the development of standardized membrane test methods. While this is necessary (as is, for example, instrument calibration), it does not specifically address program goals.
- The relevant reported progress was related to understanding sulfur poisoning. However, this was earlier work, which leads the reviewer to wonder what the team has been doing on this program.
- A test protocol was developed and test systems were modified to allow testing of various new membrane geometries at various performance levels. It was found that the loss of flux was due to hydrogen sulfide, which caused the corrosive decay of palladium membranes. The corrosive decay was associated with the reaction of palladium and hydrogen sulfide to form palladium sulfide.
- The project demonstrated excellent progress and results in just nine months with research focus initially on sulfur contamination, which is the major barrier.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Several partners are listed on the slide, but the role of each partner is not explained with the exception of the important roles played by collaborators from Carnegie Mellon University (CMU) and the University of Pittsburgh (Pitt).
- Collaboration is limited to researchers from Pitt and CMU, but there are no industrial collaborators. Therefore, there is likely no route to commercialization on the horizon.
- The list of collaborators is impressive on first examination. Then, one sees that most or all of the collaborators are related to NETL. The reviewer did not see mention of industrial/commercial partners, which suggests that the connection to commercialization could be strengthened.
- The project partners include two universities, a national laboratory, and a research organization Gas Technology Institute (GTI).
- There is no evidence the project includes, or has plans to include, a partner from the coal gasification industry.

- Partners are well integrated and each contributes their area of expertise to the testing protocol. GTI is part of the project, but no mention is given to its role.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Information of future plans are absent in the slides and no discussion about these plans were included in the oral presentation.
- Future work is not very well defined. The plan seems to be to do everything and the reality is certain to fall short. The reviewer would have liked more information of a specific plan, such as testing these membranes at these conditions.
- The proposed approach is based on establishing protective coatings to yield sulfur tolerance. Decades of effort to develop protective coatings have not yielded any practical solutions. However, the project investigator is proposing repetition of the same techniques that have been examined previously (e.g., metal coatings and inorganic coatings).
- The suggestion to use a coating formulation that is similar to an hydrodesulfurization (HDS) catalyst is intriguing. However, the rationale for this is unclear. The coating layer must be resistant to poisoning by sulfur species, active for dissociation of hydrogen, and permeable to hydrogen atoms. It is unclear what properties of HDS catalysts lead the investigators to believe these functional requirements will be met. HDS catalysts loose catalytic activity in the absence of sulfur. The side of the coating facing the inside of the membrane will be devoid of sulfur and rich in hydrogen, which is not good for an HDS catalyst. It is unclear why the team thinks they can deposit a dense, pinhole and crack-free layer of these materials (or any inorganic coating).
- Previous work conducted by the reviewer for DOE in the mid-1990s demonstrated the short-term effectiveness of glasses in retarding hydrogen sulfide corrosion of stainless steel. However, the glasses were not an impermeable barrier to hydrogen sulfide. The reasoning behind suggesting that glasses can meet the required functionality is unclear to the reviewer.
- Proposed future work includes testing with contaminants other than hydrogen sulfide, such as chlorine and nitrogen-based compounds. Future work also includes integration of the WGS reactor and membrane separator.
- The future work plan is based upon sound and important technical criteria.

Strengths and weaknesses

Strengths

- The team has a good understanding of engineering principles, membrane technology, and conversion processes.
- The facility to test membranes operates under NETL-established test protocol.
- The project investigator provided unbiased performance verification testing of membranes made by other researchers.
- The project demonstrated very good computational capability.
- The team used a nice setup. They conducted nice work with palladium-copper alloys and sulfur.
- The team developed a solid understanding of sulfur poisoning of palladium and palladium-copper alloys. The work also shows that palladium sulfide coatings are tenacious and yet they grow to remarkable thickness. This should provide important clues into potentially fruitful future directions aimed at increasing sulfur tolerance. Typically, protective oxide coatings on metals are effective because the oxidation stops once a thin surface coating forms. It is unclear to the reviewer why the tenacious sulfide coating does not stop further sulfidation.
- The project includes a good combination of in-house technical expertise and willingness to address the effects of a range of contaminants on membrane materials and performance. Progress is being made in understanding how loss of membrane flux occurs due to unwanted contaminants.
- This is an excellent project that combines computational and experimental protocols that have already demonstrated capabilities for standardization and controlled testing of various types of membranes. The laboratories and the various teams of project investigators that are participating in this project are outstanding.

Weaknesses

- Flux is very low.
- No future plans are included.

- The approach followed in this project is not good.
- The project seems to be going “everywhere and nowhere” in terms of materials and collaboration.
- The project has a lack of compelling arguments as to why the proposed future work in coatings will yield success.
- The project shows no evidence that it is making progress on understanding the challenges associated with implementing a hydrogen membrane into a coal gasification plant.
- It is probably worthwhile to work with a “user industry” team to obtain direct commercial guidance and to gain consensus on process and process economic considerations. It is also probably worthwhile to advertise the capability to ensure that all DOE membrane projects are subjected to the standard testing protocols developed under this project.

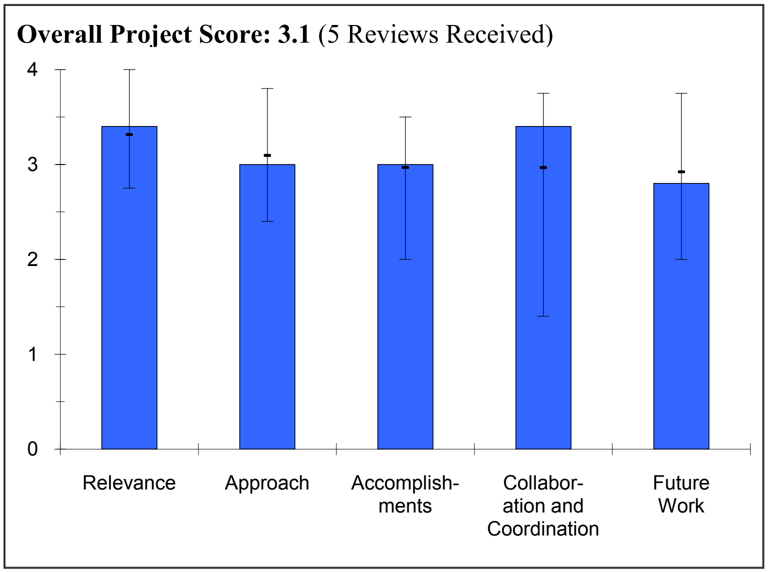
Specific recommendations and additions or deletions to the work scope

- Continuation of the computational study is recommended.
- It is recommended to maintain the test facility to test the performances of membranes fabricated by outside researchers.
- The project should pick a few more membranes to test, perhaps palladium-gold alloys. Also consider a lower-cost, higher-flux option.
- The project should pursue collaboration in a direction towards commercialization.
- The team should review the body of prior work on metal membrane coatings, including both metal layers and inorganic layers. The team should also develop a strategy for either stabilizing layers that provide all the requisite functionality or come up with an alternative program plan.
- The project should take a closer look at the mechanism for palladium sulfide formation and layer growth.
- It is recommended that the project add an industrial partner from the coal gasification community.
- DOE should continue to fund this project.

Project # PD-09: Scale-Up of Hydrogen Transport Membranes for IGCC and FutureGen Plants
Carl Evenson; Eltron Research Inc.

Brief Summary of Project

The overall objective of this project is to create hydrogen transport membranes that: 1) are a cost-effective hydrogen/carbon dioxide separation system, 2) retain carbon dioxide at gasifier pressures, 3) operate near WGS conditions, and 4) tolerate reasonably achievable levels of coal impurities. Objectives for June 2009 to May 2010 include 1) scale-up of membrane manufacturing, 2) lifetime testing, 3) impurity testing, and 4) design of a 12 lb/day membrane reactor.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The objective of this project is to develop a cost-effective hydrogen/carbon dioxide separation system that retains carbon dioxide at gasifier pressures, operates near WGS conditions, and tolerates reasonably achievable levels of coal impurities. These objectives are relevant to the DOE Hydrogen from Coal Program.
- Metal membranes are very relevant to hydrogen generation and delivery.
- This project is definitely aimed at the technical goals, but no communication of accomplishments toward economic goals is presented. Economics cannot be ignored. More importantly, the technology under development must be assessed in comparison to other available technical solutions, not only DOE economic targets, which can become obsolete as other solutions are developed.
- Mixed gas testing should be reported with a plot of flux versus hydrogen recovery since comparison to pure hydrogen flux under equivalent hydrogen partial pressure provides the necessary reference point. Sulfur poisoning results must also be reported with the hydrogen recovery since impurities are concentrated on the feed side of the membrane as hydrogen is removed.
- The project clearly supports DOE's Hydrogen Program. The overall goal is to develop a cost-effective hydrogen/carbon dioxide separation system. The objective is to develop a hydrogen recovery system that retains carbon dioxide at coal gasifier pressures, operates near WGS conditions, and tolerates impurities.
- The Eltron membrane project is yet another technology that can recover hydrogen at high purity from integrated gasification combined cycle (IGCC) syngas streams, which is critical to the economic production of hydrogen from coal and which forms the basis of the DOE Hydrogen from Coal Program element.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Approaches for the past year were to procure membrane materials prepared by different manufacturers and processes for testing and evaluation, select the preferred manufacturing process and catalyst deposition technique for scale-up in process development unit, collect lifetime data on a 6-inch tubular membrane with electrodeposited catalysts, and deposit catalyst on a 5-ft tubular membrane.
- Without knowing the composition of the membrane, these approaches seem to be fine. The reviewers have to take the presenter's word that this membrane is good.
- More information on seals to the membrane tubes would be useful (e.g., hot-seal or cold seal). Additionally, more information on membrane designs and materials and on cycling or sulfur content would be useful.

- The technical results communicated in this briefing are primarily scale-up of the membrane fabrication (and module), some durability data, and some mixed gas data, including sulfur tolerance.
- The module design is fairly standard (tube in shell) and yet the team has been slow to recognize mass transfer challenges associated with this design. Furthermore, no guidance is provided to the reviewers with respect to the composition of the permselective layer of the Eltron membrane. This is unfortunate, since even some general information would be extremely helpful in reviewing this work. Therefore, the reviewer can only conclude that the very thick (500 micron) permselective layer, which is reported to not contain palladium or copper, must be comprised mostly of Group 3, 4, or 5 metals. These are extremely reactive metals and three concerns arise immediately. First, alloying with the catalytic coating (intermetallic diffusion) is a concern. Second, oxidation from carbon dioxide, carbon monoxide, and water in the feed stream is a concern if the coating is scratched or otherwise damaged. The third concern is hydrogen embrittlement. This is not a good approach. Furthermore, it has been previously investigated and found to be deficient.
- The project seeks to demonstrate performance and economics of metal membranes using coal-based feed streams. The project has developed a high level of expertise in bench-scale testing of membranes.
- The technical approach cannot be assessed due to the “proprietary” nature relative to other hydrogen membranes. It is excellent to focus on lifetime and durability testing, making technical decisions based upon cost considerations (e.g., moving from planar to tubular platform), and partnering with a company that can provide a host test site for prototype testing using actual coal gasifier streams.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Tubular membrane manufacturing was scaled-up.
- Membrane flux decreased drastically upon exposure to gasified coal syngas that was passed through a zinc oxide sorbent bed. It is not clear what is going to be done to prevent this serious problem. Without knowing the composition of the membrane, it is impossible for the reviewers to offer suggestions or recommendations.
- The team found a good collaborator. Some tests on real gases were conducted and the team has switched geometry of membranes to tubes.
- The team’s plans are to work with larger membranes. Cycling tests and tests with sulfur would have added value in addition to some arm's length sales. Still, the project seems quite good and attractive.
- The key successes reported are scaling up to practical membrane modules and forming the partnership with Eastman. If not for these two facts, the reviewer would rate this as fair.
- Unfortunately, the negatives include lack of demonstrated reproducibility; flux decay, which the reviewer believes is inherent in the membrane structure; apparent restriction to low operating temperature, which is expected if the aforementioned alloying/intermetallic diffusion results in membrane structural degradation; and complete lack of economic benchmarking.
- The project reported that “the two key advantages of its dense metal membrane are that it is ten times cheaper than palladium membranes and has ten times better performance.” Tubular membrane manufacturing was successfully scaled up. Membrane lifetime and impurity tests were conducted.
- The project has completed all objectives on track for installing a prototype system in the last quarter of FY 10.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- It is good to see a gasifier partner, Eastman Chemical Company (Eastman), is participating in this project.
- Eastman is a great collaborator.
- Bringing Eastman on board is a big plus as this represents a pathway to commercialization.
- A critical new partnership with Eastman has been developed. Eastman will provide a coal gasification facility in Kingsport, TN, for membrane testing.
- The team did a commendable job obtaining a key partner such as Eastman, as a host site to provide actual gasifier effluents is critical to the success of any membrane development project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The plan to design, build, and operate a 12 lb/day hydrogen unit seems good. Eltron should test this unit in a real gasifier slip stream before proceeding further with the design of the 250 lb/day unit. The stability of the membrane-to-coal gas impurity is a very serious issue and this must be solved before proceeding with the 250 lb/day unit.
- Technically, the future work is perfect. The reviewer would like to see some aim for arm's length sales or publishing of technical information. It seems that the alloys would be sufficiently covered by patents by now, if they ever will be.
- The briefing did not explain how the following deficiencies would be overcome: 1) feed side mass transfer resistance, 2) achieving adequate sulfur tolerance, 3) achieving reproducible membrane performance, 4) resolving the problem of gradual flux decline, and 5) achieving competitive economics. All of these challenges need to be addressed.
- The bright side is that Eastman should be able to either assist with a compelling economic assessment or validate that done by Eltron. The field tests scheduled to be done at Eastman's plant will provide valuable and useful information.
- The project appears to be effectively planning its future work in a logical manner by incorporating appropriate decision points. Future work includes scale-up testing on a coal gasification feed stream, based on construction of a 1.5 lb/day unit to a 12 lb/day hydrogen unit (followed by a "go/no-go" decision to build a bigger one), preliminary design of 250 lb/day unit (followed by another "go/no-go" decision), and later development of a 250 lb/day hydrogen membrane unit.
- The aggressive future work plan assumes success. No alternative research and development plan is presented should performance and cost targets not meet the DOE targets.

Strengths and weaknesses

Strengths
- The team finally managed to get an end user (Eastman) onboard.
- Eastman as a collaborator is a strength. Some work with hydrogen sulfide also contributes to the success of the project.
- The scale-up of membrane modules to practical size is a strength, as is planned field tests on slipstreams.
- The project has developed a high level of expertise in bench-scale testing of membranes. It was reported that "the two key advantages of its dense metal membrane are that it is 10 times cheaper than palladium membranes and has 10 times better performance." A systematic plan has been developed involving "go/no-go" decisions for the scale-up from 1.5 lb/day to 250 lb/day of hydrogen, based on a shell and tube module design.
- The choice of an industrial partner (Eastman) as site for prototype coal gas testing is a strength. Eltron has excellent research staff and research and development facilities.

Weaknesses
- The membrane composition is unknown.
- Flux degrades rapidly upon exposure to the coal gas stream.
- Precious metal catalysts are used. The Palladium catalyst layer will be poisoned by carbon monoxide and hydrogen sulfide in the gas stream, which will limit the dissociation of hydrogen molecules and hence flux will be reduced with time.
- The project is secretive. No cycling work or work with hydrogen sulfide is included. Also, when asked technical questions, the presenter said "Eltron believes...," not "I've found...," or "the experiments support."
- The membrane composition is questionable. The reviewer speculates that this approach has been previously tested and rejected. It is uncertain how the team will overcome the challenge of limited operating temperature and flux decline. It is also uncertain how the team will protect the membrane against oxidation from feed stream components (e.g., carbon oxides, water, hydrogen sulfide). Hydrogen embrittlement and the implication for process control is a concern since it cannot be at low temperature in the presence of hydrogen.
- Information on the projected cost is lacking and it should be expressed as a range since it is probable that all variables are not resolved. In particular, the membrane is extremely thick and most likely a reactive metal that is

relatively expensive to produce in high-purity, fabricated shapes. It is a fundamental mistake to ignore cost analysis throughout the project. In other words, this should not be deferred until the end of the project.
- The lack of relevant information regarding composition of the membrane for this project makes it difficult to evaluate. Though the project is very close to scale-up and testing in the Eastman coal gasification plant, no economic results were reported.
- The loss of membrane performance is cause to be concerned about the commercial viability of the technology under gasifier conditions. This needs to be confirmed and the impact process analyzed before decisions are made to scale up to the larger unit proposed. The project appears to be on the fast track to scale up and it is not clear what targets define the “go/no-go” decision.
- The presentation was silent on the process design and economics task and there is no mention of that in future work. Understanding the cost is critical to success.

Specific recommendations and additions or deletions to the work scope

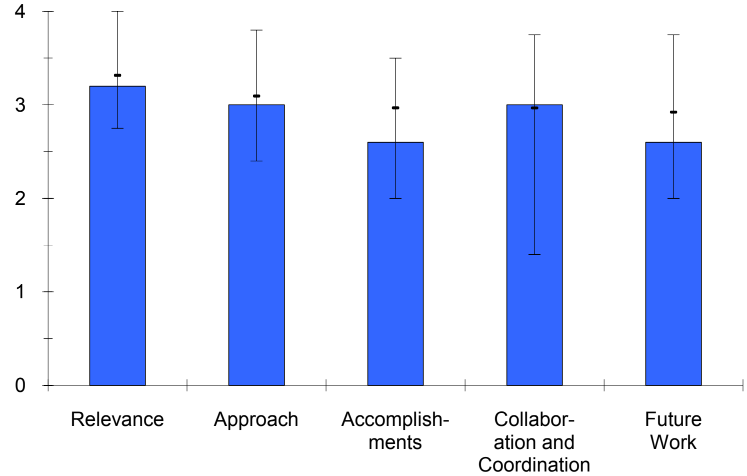
- The project should address the membrane stability before proceeding further.
- Oxidation of refractory materials is a serious issue.
- A technical publication of alloy composition is recommended. Information on arm's length sales would be helpful.
- Further funding should be significantly reduced until the membrane stability issues are resolved. Three concerns related to membrane stability (and durability) have been raised: 1) flux decline, 2) oxidative stability if there is a defect in the catalytic coating, and 3) hydrogen embrittlement under off-design conditions.
- The team should present a complete and realistic economic assessment of the membrane module cost that includes real prices for materials and fabrication. It is not adequate to say metal “X” costs $“Y”/lb, as actual cost may often be many times more for fabricated articles. The analysis should include a comparison to other membrane solutions, not only the DOE targets.
- It is recommended that the project add at least one membrane manufacturer as a partner. Economic viability should be determined for the commercial plant, using data obtained from bench-scale testing.
- Continue to fund this project.
- Though performance tests have been done in simulated gas streams as well in a simulated coal syngas, which contains sulfur, the reviewer thinks the results show an unfavorable response. Thus, the project should give expanded attention in laboratory studies to better understand the performance loss and seek a material or engineering solution before a lot of time and effort is spent running membrane tests at larger scale at Eastman.

Project # PD-10: Amorphous Alloy Membranes for High Temperature Hydrogen Separations
Kent Coulter; Southwest Research Institute®

Brief Summary of Project

The overall objective of this project is to develop advanced and novel energy technologies that will facilitate the use of our nation's abundant coal resources to produce, deliver, store, and utilize affordable hydrogen in an environmentally clean manner. The program will model, fabricate, and test thin film amorphous alloy membranes that separate hydrogen from a coal-based system. The program's objective is to have performance meeting the DOE 2015 targets of flux, selectivity, cost, and chemical and mechanical robustness, without the use of platinum group metals.

Overall Project Score: 2.8 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The scope of this project is to model, fabricate, and test thin film amorphous alloy membranes that separate hydrogen from a coal-based system with performance meeting the DOE 2015 targets of flux, selectivity, cost, and chemical and mechanical robustness, without the use of platinum metal groups. This scope is relevant to the DOE Hydrogen from Coal Program.
- Good alloy membranes are very important for DOE's hydrogen generation goals. Nickel is much cheaper than palladium.
- The program is aimed at meeting the technical goals stated by DOE and the economic goals. The approach is rather novel as well, both in materials selection and analysis/fabrication techniques.
- The project objective is to model, fabricate, and test thin-film amorphous alloy membranes that separate hydrogen from a coal-based system and meet DOE 2015 targets for flux, selectivity, cost, and chemical and mechanical robustness (without the use of platinum group metals). It is relevant to the DOE objectives and supports the Hydrogen Program in addition to the goals and objectives of the multi-year plan.
- This project is yet another membrane separation approach aimed at lower cost materials to achieve economical hydrogen separation and production from coal gasifier syngas. Developing a viable method for separating hydrogen from gasifier gas is critical to the DOE Hydrogen from Coal Program. It would be a great benefit to find a replacement for palladium at comparable performance and much lower cost.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The results show that the amorphous alloy membrane is not thermally stable. Crystallization of amorphous alloys is well known and should not be a surprise to the project investigator.
- The refractory alloys will oxidize easily.
- It is a good approach to use computer modeling coupled with experiment.
- The computational tasks performed by Georgia Institute of Technology (GIT) represent a very useful contribution to the science of metallurgy. However, one has to question the time value of this approach as the project investigator stated that three weeks of supercomputer time is required to complete the evaluation of one hypothetical alloy composition. This greatly limits the utility of the approach.
- Fabrication of ultra-thin alloy foils by vapor deposition is also a very interesting and useful technique since the size of foils that can be made is large.

- No evidence supports that the project approach will achieve results needed to move forward on a timely basis.
- The project approach is based upon sound fundamental scientific principles and is managed by experts in different institutions who bring together a full complement of technical know-how. The technical approach is sharply focused on the technical barrier of replacing palladium with amorphous metal alloys and it is difficult to improve significantly.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- So far, only a few samples are fabricated and none of them are tested.
- It is early in the project. An investigation of a higher nickel content alloy would be beneficial so the team could go to higher pressures and lower temperatures, which would add value. Still, the team is learning.
- The project investigator admitted that little effort has been expended due to a slow start on the computational tasks. As a result, the reviewer is taking this into consideration and is expecting to see much more progress over the next year.
- The vapor deposition process seems to result in carbon incorporation in the resulting foil films (see composition reported for nickel zirconium binary alloy fabrication). It is uncertain why this occurs. If this is inherent in the process, it must be fixed. The fabrication process must be capable of producing alloys of the desired composition and purity. Otherwise, the technique is of limited value.
- As a relatively new project (start date is October 1, 2009), no significant technical accomplishments were noted. It appears the most of past eight months have been spent planning the project and budget.
- Although the project is early, preliminary work completed shows a promising methodology.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Georgia Institute of Technology (GIT) and Western Research Institute (WRI) are collaborators.
- The collaborators' roles are defined.
- Good collaborators are included in this project. More materials expertise would be beneficial since they would have known to expect metal rearrangements at the high temperatures of the study. They would also be familiar with the high hydrogen contents.
- During the next year, a commercialization partner should be brought on board. The other partners represent a sound basis in the fundamentals.
- The project appears to collaborate and work well with academic and research entities. GIT will select alloy compositions using first principle calculations for membrane permeability. Southwest Research Institute (SwRI) will perform fabrication of the alloy membranes, and WRI will test the membranes. However, there is no evidence that they plan to collaborate or partner with someone in the industrial coal conversion community.
- The project has close, appropriate collaboration with other institutions and partners are full participants and well-coordinated.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- The focus of the second year effort will be to evaluate the performance and fabrication options to produce membranes.
- It is not clear who will perform the preliminary evaluation of membrane performance before the membrane system is tested in the gasifier stream.
- The team needs to get the membrane durability up before moving to sulfur, which means the membranes will probably have to operate at lower temperatures.
- The reviewer faults the program plan for placing much effort (at the expense of both manpower and money) on theoretical calculations of amorphous metal alloys that do not address a key requirement of stability in hydrogen at operating temperatures. This is an easy experiment to conduct. The project investigator stated (in questions

after the briefing) that a few known glassy alloys with high reported transition temperature will be tested for stability. This should have been done as the first task, rather than being delayed until this point in time.
- It appears the proposed future work will move slowly. The first year of the project will be used to demonstrate an amorphous alloy, based on six membrane samples to test and establish thermal stability.
- The project has just begun and the proposed work plan is solid.

Strengths and weaknesses

Strengths
- The project did well at modeling to predict alloy compositions. It was also a good move to add WRI as a partner in the team.
- The project is innovative and exploratory.
- Alloy structural and property calculations have been validated with published results and ultra-thin film fabrication methods.
- The project is a good example of how different research groups can work together to achieve a common goal. It appears their goal is more about providing information to others who are interested in commercializing membrane separation for hydrogen recovery than commercializing the unit themselves.
- This is an excellent collaborative study. Though the project is in the early stage and has started late due to staffing reasons, much progress has been made to lay the foundation for the remainder of the project.
- The systematic approach with the use of combinational screening techniques and computational modeling is a plus and should lead to some very useful results.
- The project will be using actual gasifier feeds to test their membranes, thus being able to identify contaminant performance issues early before designing and scaling larger units.

Weaknesses
- The project lacks a research plan to stabilize amorphous phase.
- The project lacks collaboration with researchers involved in industrial operation.
- The reviewer did not see a plan to prevent oxidation of the refractory alloys.
- This project is innovative and exploratory.
- It is unclear whether the fabricated ultra-thin foils are free of pinholes. A statistical treatment of this question is required. In other words, if "X" m^2 of foil is fabricated, the occurrence of pinholes (or other defects) should be provided. In addition, conclusions should be drawn on whether the fabricated foils can be handled and at what thickness. The reviewer has some experience testing the foils made at SwRI and believes they can be difficult to handle.
- A fundamental question must be answered immediately about whether it is possible to have a stable (with respect to crystallization) glassy metal alloy of a composition likely to be permeable to hydrogen that is stable at operating temperature and in the presence of hydrogen. If the answer is no, this program should be stopped or redefined. Assuming the answer is positive, the project should determine how a catalytically active protective coating will be applied to the glassy membrane and how the coating will be stabilized. A large body of experience exists that shows simple metal coatings are not stable with respect to intermetallic diffusion in the presence of hydrogen at operating temperatures.
- No evidence is provided that shows the project team is working to understand specific challenges involved in implementing and operating a membrane process in an industrial coal gasification plant.
- Though this is an academic collaborative study, the reviewer recommends that the team try to include someone from the user industry to provide process and economic guidance. Sometimes, the best catalyst or the best material can be developed, but in the end it has no major impact on the final cost of using the technology. When at the proper stage of the project, the team should try to collaborate with NETL and have their membrane concept tested using the NETL testing protocol.
- There are no permeability tests or contaminant tests scheduled until the third year. It is suggested that some earlier testing be included in the research plan.

Specific recommendations and additions or deletions to the work scope

- The team should get stability under control, and then move to testing with sulfur, etc.

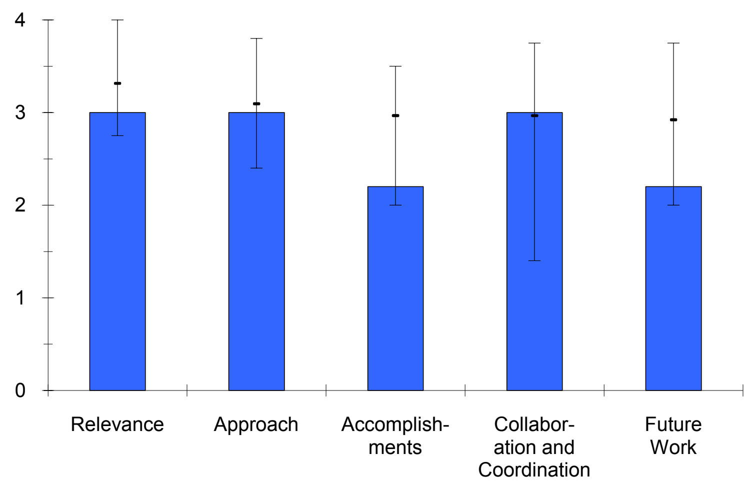
- Demonstrate that developing a stable glassy metal alloy is possible, and then offer a compelling plan to achieve success with respect to applying a catalytically active protective coating.
- The team should address the alloy foil composition with respect to carbon inclusion (Ni-Zr binary alloy results). If this is a problem, a solution must be found soon.
- It is recommended that the project develop a broader vision that includes an understanding of the challenges involved in operating a hydrogen membrane separator in a coal gasification plant.
- Continue to fund this project.
- Add a task to do some early process and economic evaluation to develop a sense of the elements important to the eventual cost of the membrane system.

Project # PD-11: Experimental Demonstration of Advanced Palladium Membrane Separators for Central High-Purity Hydrogen Production
Sean Emerson; United Technologies' Research Center

Brief Summary of Project

The objectives of this project are to 1) confirm the high stability and resistance of a palladium-copper tri-metallic alloy to carbon and carbide formation and, in addition, resistance to sulfur, halides, and ammonia; 2) develop a sulfur-, halide-, and ammonia-resistant alloy membrane with a projected hydrogen permeance of 25 $m^3/(m^2 \cdot h \cdot atm^{0.5})$at 400°C and capable of operating at pressures of 12.1 MPa (~120 atm, 1,750 psia); and 3) construct and experimentally validate the performance of 0.1 kg/day hydrogen palladium-copper tri-metallic alloy membrane separators at feed pressures of 2 MPa (290 psia) in the presence of hydrogen sulfide, ammonia, and hydrogen chloride.

Overall Project Score: 2.6 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The objective of this project is to develop an advanced palladium-based alloy membrane to separate hydrogen from a coal gas stream. There is not a gasifier partner in this team.
- Better membranes are very relevant to meeting hydrogen production targets, which are the aims of the project.
- A project objective is to confirm the high stability and resistance of a palladium-copper tri-metallic alloy to carbon and carbide formation. In addition, resistance to sulfur, halides, and ammonia is also very important.
- Most of the technical goals appear to have been addressed, although sulfur tolerance was weakly addressed and the reviewer is not convinced that any new advancements were made. For example, sulfur tolerance of palladium-copper and palladium-gold alloys has been public knowledge for decades.
- This program mostly fails to meet the economic requirements necessary to be commercially feasible. The developed membrane and module is prohibitively expensive for all but a small handful of applications that are limited in market size. This work has done very little to advance the state-of-the-art of palladium membrane technology.
- Most project aspects, such as testing for membrane flux and durability, are relevant to the Hydrogen Program. The objectives of the project include testing of palladium-copper and tri-metallic membranes for hydrogen purification that are resistant to poisoning by syngas contaminants.
- Development of advanced palladium membrane separators for central high-purity hydrogen production is critical to the Hydrogen Program and fully supports DOE research and development objectives.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach is to use atomistic and thermodynamic modeling calculations to predict palladium-based ternary alloy compositions, fabricate the membranes, and evaluate membrane performance.
- Modified palladium-copper membranes seem like a reasonable approach. The tests seem reasonable. Unfortunately, no work was done on the majority of the goals. Instead, only stability and resistance to sulfur were covered.

- It appears to the reviewer that two different branches exist in this program. One involves module development and testing based on existing Power+Energy (P+E) commercial module designs. The other branch is the UTRC effort to develop a ternary alloy that has better performance (e.g., flux, cost, sulfur tolerance) than can be found with known palladium-copper alloys.
- The briefing did not convey any new learning from the modularization of palladium-copper membranes in P+E's module design.
- UTRC has an interesting approach of developing a ternary alloy of palladium-copper-X (X denotes a third metal) that retains the body-centered cubic (BCC) phase under operating conditions and has sulfur tolerance. However, it is not a totally novel approach. Regardless, the reviewer thinks this approach has merit.
- The project approach is to use membrane property simulations by atomistic modeling and experimentally verifying performance of hydrogen separation membranes. Unfortunately, chronic membrane failures have made many of the results inconclusive.
- The approach identifies the key barriers, and the research plan is focused on key performance verification including stability, sulfur, and other contaminants found in gasifier streams. Lab testing protocol clearly addresses the addition of contaminant at levels appropriate for gasifiers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.2** based on accomplishments.

- The diffusivity of membrane is low due to segregation of metal oxide.
- The team successfully developed an atomistic modeling screening approach for sulfur tolerance.
- Too many defects exist in the tube. Tube performance is not satisfactory.
- The project has reasonable results. No progress was made on anything but sulfur; and nothing was presented on ammonia or halides. The price is still prohibitively high. It is not clear that the ternary is any better than plain palladium-copper. Perhaps the team should have tried more than one ternary.
- The reviewer is hard pressed to point to any key results of this work. The modularization effort is not new work. The briefing reported that P+E used its existing module design and existing membranes as a reference point for new membranes developed by UTRC. The UTRC ternary alloy membrane does not offer improvement in performance or cost over the existing palladium-copper membranes. Indeed, the project investigator reported that the cost of a commercial-scale membrane module assembly would be hundreds of millions of dollars (as expected for a relatively thick-walled tubular membrane). There is no reason to believe this work will result in any commercial value for the stated goal of DOE.
- Mixed-gas test results should be plotted as hydrogen flux versus hydrogen recovery, with a reference line showing the flux obtained with pure hydrogen at a similar hydrogen partial pressure. Poisoning experimental results must state the hydrogen recovery since impurities are concentrated at the feed side of the membrane as hydrogen is removed.
- Though the project has quantified the effects of hydrogen sulfide, carbon monoxide, carbon dioxide, nitrogen, and water on hydrogen permeability and demonstrated sulfur resistance of the palladium-copper alloy, significant membrane failures tend to occur. The hydrogen permeability needs to be substantially improved. When a single-tube separator with the ternary alloy composition was tested, it was found that a compositional barrier formed on the membrane surface. A polishing process was identified to remove the barrier, which allowed performance improvement of the membrane by a factor of two compared to 2009 results. There are unresolved problems that still exist at the bench scale, including issues with membrane flux, durability, and performance.
- Considerable progress has been made to understand sulfur tolerance and test the performance of the membrane under a wide range of conditions. Too much detail was presented in slides to ascertain key results and accomplishments, and what is yet to be achieved. However, the summary slide lists key accomplishments thus far.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The team is collaborating with P+E to fabricate membrane tubes.

- Informal collaboration with two universities is occurring.
- A gasifier partner is not included in this project.
- The team has a good partner and seems well-coordinated. Given the poor results with the ternary, it would have been nice to know the identity of the third element.
- P+E is a good addition, as their experience in modularizing small-diameter, thick-walled tubular membranes is very good. UTRC itself brings a lot of commercialization experience, but perhaps not so much in the areas of chemical processes.
- The project collaborates with P+E to manufacture the separators and fabricate alloys.
- Thermodynamic phase modeling has been done in collaboration with Metal Hydride Technologies. The Colorado School of Mines is also a valid collaborator. However, there is no evidence the project has actually approached a coal gasification plant operator to test their scaled-up membrane process with a coal gasification-based feed.
- The team has close and appropriate collaboration with membrane fabricators and institutions. Partners are full participants, and the team is well-coordinated.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.2** for proposed future work.

- The project includes future plans to construct ternary alloy separator for testing, conduct DOE testing protocol tests to validate performance and durability, and move to larger scale demonstration (e.g., 100 lb/day of hydrogen) with real gasifier exhaust in a follow-on effort. Before the team embarks on this plan, they must develop a technique to fabricate defect-free membranes. Also the transition metal (TM) oxide segregation problem must be avoided.
- It is unclear how the team plans to address the problems with sulfur tolerance and price that have appeared. There is no indication that plans exist to study 1% ammonia, for example, or halides.
- The project investigator stated that the project is essentially completed, so this question is probably not relevant. However, the reviewer strongly urges the DOE to require compelling arguments for achieving economic feasibility before funding further projects that are similar to this one. Thick-walled (on the order of 50 micron) small-diameter tubes simply do not show any chance of economic feasibility if palladium is a major constituent. Tubes made of Group 3, 4, and 5 metals also have significant costs of fabrication, and proposed programs aimed at using these metals to avoid the cost of palladium must be scrutinized for convincing arguments for achieving success.
- Proposed future research includes constructing ternary alloy separators for testing. Tests are planned to validate that performance and durability can meet DOE targets. The project also proposes to move to a larger-scale demonstration unit (e.g., 100 lb/day of hydrogen) with real gasifier exhaust. This project has encountered significant problems in conducting membrane tests due to failures of the membranes themselves and low flux rates. These problems need to be resolved well in advance of any consideration to move to a larger scale demonstration with a real gasifier exhaust.
- Plans clearly build on past progress and are sharply focused on barriers. It appears that this project ends with lab verification of performance. Then, based upon preliminary economics, the team may elect to move to a larger size unit to test feasibility with actual gasifier exhaust.

Strengths and weaknesses

Strengths

- The team is conducting modeling to predict alloy compositions.
- The team has demonstrated the ability to test membranes.
- This team uses a good approach, and they are likely to produce some valuable and practical products. The project team has good collaboration.
- The technical approach of developing a ternary alloy of palladium-copper- X to achieve a BCC structure that is resistance to sulfur poisoning bears further investigation.
- This project tested membranes that had been exposed to a number of impurities that closely approximate industrial plant conditions. The results were valuable in understanding factors affecting performance of hydrogen membrane separation processes.

- Key performance issues are identified and are an integral part of the project.
- Membrane fabrication testing is an "art in itself," and the project includes team members who are industry experts.
- All operating performance targets, especially sulfur and ammonia tolerance, appear to be attainable.
- This project has an excellent team of companies and institutions and adequate research facilities or expertise that will be beneficial to completion of the project.
- The team uses NETL testing protocol.

Weaknesses

- A gasifier partner has not been identified.
- The team was unable to fabricate defect-free membrane tubes.
- The team was unable to prevent segregation of TM oxides.
- Flux is not high enough.
- The method is too expensive, and sulfur tolerance and flux are still below desired levels. No publication of the third element is provided, and no results with halides or realistic ammonia contents are included.
- Adding oxophilic elements (metal X) to the palladium-copper alloy must be approached cautiously. One commercial supplier in Asia offers a ternary of palladium-copper-X where X is an oxophilic transition metal, and this alloy is susceptible to oxidation in air as well as in the presence of reformate when at operating temperatures. The project investigator stated that the third alloying element X in the UTRC alloy does oxidize in air. This represents a potential weakness in the composition that could make it unsuitable for commercial applications.
- Long-term durability testing of any new alloy needs to be done under realistic operating conditions.
- The economic analysis should have signaled a problem much earlier in the program. Effort should have been redirected long ago to a pathway that offered promise of meeting acceptable economics.
- Chronic failures of the membranes resulted in pinholes and leaks, and it appears that membranes manufactured and tested in this project have not performed as desired. Significant improvements are needed in the hydrogen membrane flux to achieve DOE flux targets of 200 $scfh/ft^2$ in 2010.
- Nothing is reported about how the project will attempt to improve the hydrogen flux, which is about four times short of the target.

Specific recommendations and additions or deletions to the work scope

- The team should focus effort on fabrication methodology to minimize defects in the membranes.
- The team should select a ternary composition to avoid oxide segregation.
- The project is nearly done. The team should test high ammonia and hydrogen chloride and publish the third element.
- Any further work on this program (or similar programs) should be deferred until the cost objection is overcome.
- It is recommended that the project focus on improving flux and manufacturability of membranes that will achieve DOE targets.
- Continue to fund this project.

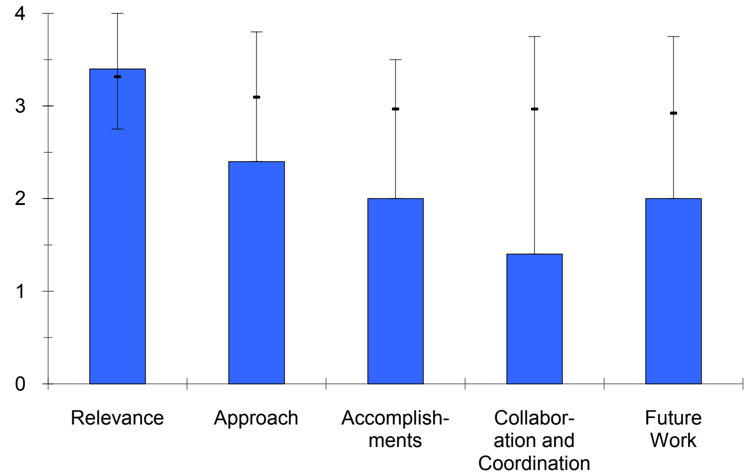
Project # PD-12: Supported Molten-Metal Membrane (SMMM) for Hydrogen Separation

Ravindra Datta; Worcester Polytechnic Institute

Brief Summary of Project

The objectives of this project are to 1) enhance hydrogen diffusion via more open lattice, 2) enhance hydrogen dissolution via a more open lattice, and 3) enhance density of surface dissociation sites. The goals of Phase 1 are to 1) select a molten metal and catalyst, 2) select suitable porous supports (porous metal, with or without a diffusion barrier, or ceramic), 3) develop membrane fabrication protocols, and 4) establish basic feasibility of the SMMM.

Overall Project Score: 2.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The objective of this project is to develop a SMMM for hydrogen separation. There are serious issues regarding the stability and structural integrity of the molten membrane.
- This project is certainly in line with DOE goals.
- The stated objectives and program plan are clearly aimed at the DOE goals. This is a novel approach, and the reviewer is unaware of any direct precedent.
- The goal is to develop hydrogen separation membranes to recover hydrogen produced by coal gasification.
- The project appears to support the Hydrogen Program.
- The objective of this project is to validate the technical and economic feasibility of a SMMM for hydrogen separation, which clearly supports the Hydrogen Program objectives.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- The approach lacks several important points. For example, the identity of the porous substrate to be used in this work is unknown. The structural integrity of SMMM on porous support tubes is also unknown.
- This is an innovative approach. The team should have looked at membrane materials that are more likely to wet but not alloy with the substrate.
- The technical approach seems to lack direction and priority. The project investigator could make more efficient use of time and money by prioritizing the technical challenges and then addressing them in a thoughtful and systematic fashion, which was not evident in the briefing. Indeed, a lack of recognition of fundamentals (e.g., wetting, surface tension and capillarity, and alloy) seems to pervade the effort to date.
- It appears that the approach of the project is to develop a molten metal alloy membrane that will enhance hydrogen diffusion and dissolution via a more open lattice. There is no evidence that the project has a plan to address the challenge of achieving immobility and stability requirements for the molten metal alloy membranes.
- This is an interesting and innovative membrane approach worth studying to lower cost and possibly improve performance regarding operating issues. However, many issues in membrane fabrication and contaminant tolerance could and should be integral to the fabrication and materials choice decisions. Economic considerations need to guide even the early research, as many technical hurdles exist here.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.0** based on accomplishments.

- The project investigator has attempted to fabricate membranes but was not successful. Poor selectivity was reported.
- Practically speaking, the results were poor. Still, the project is innovative and the team is still at an early stage of the work. The team has not even developed good membrane development methods.
- The briefing did not communicate any significant positive results toward the objectives. This is a high-risk project, and unless the project investigator can refocus on the immediate technical challenges and demonstrate progress, the program should be either halted or redirected.
- Early work in the project has to do with the construction of a porous Inconel support. Progress was made on identifying candidates for molten metals and supports. Electroless/electroplating methodologies for SMMM fabrication were developed.
- The project is early (only nine months old), but good progress on screening methodologies, identifying materials, and conducting some preliminary tests has been made. It is too early to judge this project relative to meeting targets.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.4** for technology transfer and collaboration.

- No outside collaborators participated in this project.
- The team should collaborate with a metals and materials expert. They need a better method to test wettability and undesirable alloy formation. Still, this may be an area where some ignorance helps.
- No other collaborators were communicated in the briefing materials. This may be sufficient at this early stage, but if the program is to continue it is vital to involve collaborators with modularization and commercialization experience.
- No evidence is presented that the project has established any outside collaboration or technology transfer. The team appears to have no plans to do so.
- All work is done at the sponsoring organization with no identified outside collaboration. There needs to be partnering at some point with actual membrane fabricators and a team member that can provide process economic guidance.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- The project investigator is going to look for alternate support and metal compositions to fabricate membranes.
- If metal alloying and inorganic wetting are both unsolved problems, it is not clear that a combination will not suffer from the combination of the problems.
- The project investigator presented no compelling plan for the future in the slide presentation. Only during questioning did he hint at plans to test zirconia substrates because he has reason to believe they may wet better with the molten metal phase. When questioned about the catalytic metal phase, the project investigator offered ideas that dissolving (alloying) a catalytically- active metal in the molten metal might offer good results.
- Overall, the briefing did not offer a convincing plan to overcome the two most important technical challenges, which are finding a substrate that is wetted strongly by the liquid metal phase and finding a way to catalyze hydrogen dissociation at the surface of the liquid metal phase.
- Proposed future work of the project includes an investigation of alternate supports and molten metals as well as feasibility and permeability studies on SMMMs.
- Future plans build on early results and generally address overcoming barriers. The plan includes investigating alternate molten materials and supports, but needs to include continued work in fabrication methods, which is integral to the choice of and behavior of the materials and the support.

Strengths and weaknesses

Strengths

- The project investigator has extensive experience in fuel cells and hydrogen catalysis. He has developed the technique of supported molten-metal catalysis.
- The co-project investigator is a leading researcher in palladium and palladium alloy membranes.
- This is an novel, innovative project, but risky.
- The project's theoretical basis may justify the research associated with SMMMs for hydrogen separation.
- This project offers a different membrane approach with distinct operating advantages, if successful.

Weaknesses

- No good plans to fabricate a durable SMMM were presented.
- The team needs an expert on metals and materials. The project may never work, but the lack of expertise makes it hard to tell if the problem is fundamental.
- This project is very high risk.
- An inert porous structure/material must be found that is wetted strongly by the molten metal. This is a requirement to the liquid metal being held in the pores (capillarity) at reasonable trans-membrane pressures. No evidence currently exists that a solution is possible.
- A molten metal that is permeable to hydrogen and chemically stable in the presence of reformate must be identified. The project is risky, and feasibility has not been established yet.
- A catalyst for dissociating hydrogen may be needed depending on properties of the molten metal phase. This catalyst needs to remain active when in the presence of the liquid metal phase and reformate. Again, no evidence has been presented that this is possible.
- The project does not demonstrate an approach of how to address the requirements of immobility and stability of the liquid molten metal membranes in the environment of a coal gasification plant. The team does not appear to have secured the expertise to address the challenges associated with operating a membrane unit in the industrial coal gasification plant.
- There are numerous technical and cost issues associated with this approach. Therefore, there is a low probability of success. It is worth pursuing at least to the point of showing that the idea is technically feasible, but that must include a demonstration of contaminant (e.g., sulfur) tolerance as well as flux and selectivity. This was not included in the plan.

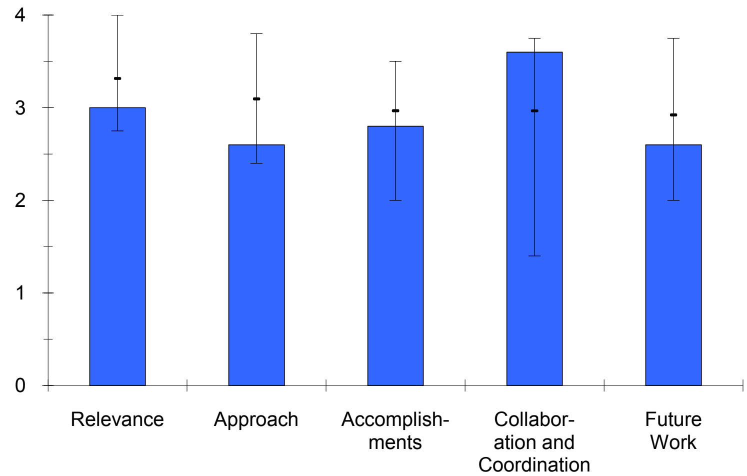
Specific recommendations and additions or deletions to the work scope

- This project should be discontinued.
- The team needs an expert on metals and materials.
- This project must be significantly reorganized. The above technical challenges need to be addressed individually to establish a reason to believe the overall approach has technical merit.
- The question of a liquid (molten) metal wetting the porous support can be addressed easily and quickly by measuring the contact angle of a drop on the material in question. From the contact angle, one can calculate the capillary force within the support material's pore structure. If the bubble pressure is not at least 200 psi, success is unlikely.
- Hydrogen permeability of the molten metal phase can be experimentally probed, but the project investigator has some literature data and this should have been communicated. Specifically, the reported hydrogen permeability, if known, for tin should be communicated. The diffusivity/solubility is not reported.
- It is even easier to evaluate the chemical reactivity of candidate molten metal phases in reformate at the intended operating temperature.
- The probability of identifying suitable catalytic materials should not be overlooked. The feasibility to have discrete metal particles (probably not due to alloying with the molten metal phase) is unclear. Inorganic compounds might be considered rather than metals.
- It is recommended that the project add a partner from the industrial coal gasification community to understand the challenges associated with operating an industrial membrane unit.
- Develop a preliminary economic model and use it to conduct preliminary economics to guide the research.
- Continue to fund this project.

Project # PD-13: R&D Status for the Cu-Cl Thermochemical Cycle-2010
Michelle Lewis; Argonne National Laboratory

Brief Summary of Project

Overall Project Score: 2.9 (5 Reviews Received)



The overall objective of this project is to develop a commercially viable process for producing hydrogen that meets DOE cost and efficiency targets using the copper-chloride thermochemical cycle. The features of copper-chloride thermochemical cycle that promote meeting targets and overcoming barriers are that: 1) the 550ºC maximum temperature allows coupling with the solar power tower, which is near commercialization; 2) the conceptual design uses commercially practiced processes; 3) the high yields in thermal reactions mean that no catalysts are required; 4) preliminary Aspen flowsheet indicates it is possible to meet the efficiency and cost targets. Key challenges are to 1) inhibit copper crossover and achieve stable cell performance in the electrolyzer, 2) identify and cost materials of construction, and 3) reduce steam demand for hydrolyzer.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The copper-chloride thermochemical cycle remains one of the most promising approaches for large-scale water splitting that meets the DOE targets for hydrogen cost and efficiency.
- This project has a well-designed process for generation of hydrogen. Good attention has been paid to the impact of cost as well as design and material selection.
- The production of hydrogen fits in the DOE portfolio.
- This is a program to desirably supply "green" hydrogen via water splitting using a combination of thermal and electrical energy, with the latter preferably from renewable resources.
- The program proposes to utilize thermal energy to synthesize hydrogen.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- The primary focus should remain on potential showstoppers such as copper crossover in the electrolysis step.
- Copper crossover is a critical path. It is unclear whether anyone on the team calculated what level can be accepted and turned this level into a membrane specification.
- The project uses a good approach in systematically identifying weak sections of the technology and pursuing solutions for these issues.
- The electrolyzer is very inefficient. Even if the team hits their target voltage and current, the performance may not be substantially better than that of water electrolysis.
- The critical path is the electrolyzer development, which is where the majority of the team's effort should be focused.
- The voltage reduction targets (a decrease of 0.07 V in ten years) do not seem very aggressive.
- The chemicals being used are very aggressive, but minimal effort is discussed in the approach on to how the team will handle these materials.

- The team is building a large-scale demonstration reactor before having identified proper materials. This does not seem logical.
- They are using the H2A project to identify operating conditions that are needed to meet their cost targets, which is good.
- The team uses two lab-demonstrated chemical transformations that are coupled with an electrochemical step and further processing to, in effect, split water. At the present 0.7 V for the electrolysis, the latter would consume at least one half of the overall needed energy. An apparently ongoing study of the inherent thermodynamics should allow an estimate of the minimum voltage for this electrolysis.
- The proposed chemistry, which uses a combination of hydrolysis and electrolysis, involves difficult processing conditions. Clearly, some excellent engineering design is required to make this successful. The approach did not address a solution to some very key issues. No mention of anticipated yields is provided. The concept of using a "solar power tower" obviously means that the process cannot operate at night and, thus, will be subject to periodic “on” and “off” cycles. The technical and financial implications of this operational mode were not addressed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The progress has been very good on improving the electrolysis performance, although there is still a substantial gap to reach the DOE target with a cell that exhibits a long operating lifetime.
- Very good progress has been made in moving towards integrating the individual systems.
- It is unclear why or how the team expects to find a membrane. The desired qualities of a membrane that could work are not stated. It appears that if a good membrane cannot be found, the process as described will not work.
- When asked about the current membrane status, the project investigator responded that Pennsylvania State University used a proprietary membrane that has worked 100 hours to date. While this is very encouraging, a minimum of 1,000 hours of durability is expected.
- The team shows modest improvement on the electrolysis. They need to demonstrate longer life at lower voltages, and they need to immediately test at the higher temperatures.
- The team is scaling up the chemical reactor now. This appears premature since they have not finished the benchtop reactor development.
- The team constantly refers to their H2A analysis, but it is not in the material. They are using the H2A to direct their work, which is very good. More projects should do this. They should have included the H2A analysis, perhaps in the supplemental section. In particular, inclusion of the electricity cost would be valuable. If they use grid electricity, many of the benefits of this project disappear, whereas if they use solar power, then it is unlikely they will meet the cost targets. Therefore, it would be interesting to see how they did this.
- The chemicals to be used are extremely aggressive, especially at the operation temperatures. They did not discuss any materials compatibility testing, though this is a very important area. It seems that the team should have done materials compatibility testing before constructing a large-scale demonstration.
- Excellent progress was made in the copper-chloride hydrolysis to Cu_2OCl_2 reaction and in the selective decomposition of the latter to oxygen and copper-chloride. However, many issues remain with the electrolysis step, and details were not well conveyed. Thus, it is not clear exactly what the actual membranes were. They should be chloride instead of chlorine carriers, and they should be anion- (i.e., Cl-) instead of cation-exchange membranes as is written in the slides. Also, the catholyte and anolyte concentration steps are clearly a remaining challenge.
- The project seemed to be in early stages, and most of the results appear to be planning activities and organizational tasks.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- A great international team with highly relevant partners is identified.
- In the past, this project had poor collaboration with some of the partners. It seems they have remedied the situation.

- Excellent collaboration has been shown with the other cited laboratories. More input from Pennsylvania State University in detailing progress on the electrolysis would have been welcome.
- Argonne National Laboratory plans to outsource most of the work, much of which is to Canadian interests. The competency and qualifications of these organizations were not discussed.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- The membrane may be a key component to success. It would be useful to include a membrane development plan and how this piece of the system is expected to be improved to meet the critical challenge of reducing crossover.
- The future work listed by the team seems appropriate. More effort needs to be directed to the electrolyzer development.
- On slide 15, the team discussed the large-scale demonstration, but this is not mentioned on slide 16, which discusses future work. It seems premature for them to proceed with the large-scale demonstration when they have so many other critical issues to address.
- The proposed future work addresses most of the remaining issues.
- Some of the technical challenges were mentioned (e.g., copper crossover in the electrolysis step and management of hot, strongly acidic media), but the technical steps proposed to address them were not.

Strengths and weaknesses

Strengths
- The project involves good collaborations with competent partners such as Commissariat à l'Energie Atomique (CEA) and universities. The focus should be on solving the problems that are potential showstoppers.
- The international team members bring a wealth of experience in the specialties relevant to making this program a success.
- This is an innovative process that appears to have a good probability of success.
- Good progress has been made at this point.
- A large team has been assembled.
- The remarkably selective chemical steps of the process, which are conducted without need of catalysis, is a strength.
- The strengths clearly must involve the people working on this activity. Unfortunately, the Canadian technical and intellectual resources were not discussed.

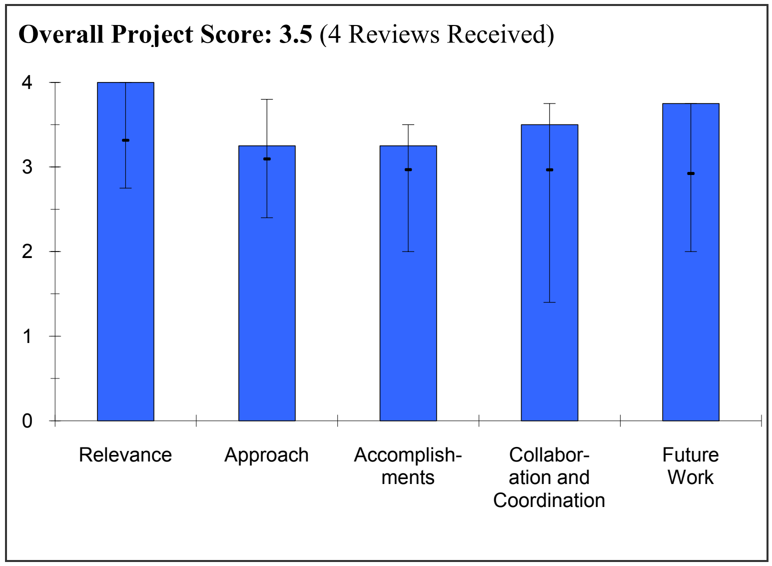
Weaknesses
- The use of unusual process conditions and equipment (e.g., the ultrasonic nozzle and sub-atmospheric pressure for the hydrolysis reaction) could limit the ultimate practicality of the cycle.
- Since little was presented on the membrane, by default, this is the weakest link. It is not known which properties of a membrane are critical. For example, it is unclear whether only a custom membrane will work. In addition, the level of difficulty of making this custom membrane for a commercial process is unknown.
- The approach does not seem logical. For example, the team reports having large-scale reactors to perform the testing, but they say that in their future work they will identify compatible materials. It would make more sense to identify compatible materials and then build large-scale demonstrations.
- The copper-chloride electrolysis is very inefficient. It is not clear how this system would be superior to high-temperature electrolysis.
- The team has made some limited progress on electrolyzer life testing, but they need to be testing for thousands of hours as opposed to a couple of hours.
- The team correctly identified corrosion as a significant problem, yet they did not report any work in that area. They need to do some corrosion testing since this is a large gap in the project.
- The team needs to get thermodynamic data for all three steps of the cycle. Specifically, this data is needed for the optimization of each and for estimating the true potential maximum energy efficiency of the process.
- More transparency is needed on the electrochemical step, including details and discussion.

- Although the program seemed new and just becoming focused, the plan for making necessary progress was inadequate.

Specific recommendations and additions or deletions to the work scope

- The project needs more membrane work on lifetime, characterization under high acid concentrations, development of membrane cost models, and a back-up plan if the membrane does not work as expected.
- The team needs to identify a critical path to success and follow it. Critical areas include electrolyzer development, reactor optimization, and materials. Once these are addressed, then they should consider scaling up.
- The team needs to address the corrosion issue. If they cannot find any materials that have a reasonable life under the operating conditions, then there is no point in continuing this research. However, they did not report any work in that area. It should not be the main focus of their work, but they should be looking at it in parallel. Perhaps instead of scaling up the reactor, they could do some materials testing.
- These "thermochemical" processes have been around for decades. This project needs to address the key technical issues, which are copper crossover and material design, and work those tasks before a "system" is built. A “go/no-go” milestone written around the electrolyzer would be a good addition.

Project # PD-14: Hydrogen Delivery Infrastructure Analysis
Marianne Mintz; Argonne National Laboratory



Brief Summary of Project

The objectives of this project are to 1) provide a platform for comparing alternative component, subsystem and system options to reduce the cost of hydrogen delivery; 2) analyze delivery options (e.g., wind-to-liquid hydrogen); and 3) develop new tools that build off existing DOE-sponsored tools (e.g., the H2A project, with a focus on production; Fuel Cell Power Model; and the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation Model [GREET]).

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Although DOE focuses on future large-scale stations, it is important to not forget that in the near to intermediate term, the implemented stations will be much smaller. This must be dealt with first. The cost of dispensed hydrogen if multiple stations use the same design should be investigated instead of funding and building multiple "unique" station designs.
- The topic is very relevant to research and development objectives because it helps define future funding priorities within DOE. As far as immediate relevance for near-term costs, it is less so.
- This project is necessary to explore the costs of options for hydrogen delivery. This allows the Delivery Team to focus on high-cost items to target research.
- This project is very important to assessing the hydrogen delivery infrastructure, and it supports the DOE research, development, and deployment objectives.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- More actual data from industry and current projects can always improve models like this, especially when it relates to the verification of underlying assumptions.
- This project provides a good framework from which to analyze tradeoffs, but more description of uncertainty is needed. More information on the uncertain processes or technologies that make a large difference in final cost and why this occurs would be helpful. For example, the level of confidence in the team's cost estimations would be helpful if construction of infrastructure began today. Expert opinions on which aspects are most subject to change would be useful. Information on the ramifications on the future direction of research is requested, though perhaps this in the main report and not contained in the presentation.
- A stronger tie-in with current technology costs for comparison could be provided. Information on how to bridge the gap from now until a final state and how to phase this in may be helpful Also, a little more comparison to costs for onsite SMR should be included. Again, this information may be contained in the full report, and the presentation was simply too time-constrained to go into detail. Overall, the approach seems sound.
- A great approach and interaction with stakeholders to get accurate input is presented.
- The approach of developing a flexible, user-friendly spreadsheet-based tool will be beneficial for the industry to utilize and analyze various strategies. It would be useful to have further information regarding the validation of

the assumptions in the tool. The approach for the fuel station footprint needs to be further explained regarding the actual constraints and options.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Input from gasoline station owners is urgently needed on station footprint feasibility since future hydrogen stations and equipment will most likely be added to or replace existing gasoline stations.
- The team presented very interesting work on the tradeoffs for cold gas pathway. The team demonstrated a more realistic station footprint. Version 2.2 of the H2A Delivery Scenario Analysis Model (HDSAM) was posted.
- The team provided good accomplishments and sharing of the technology and model.
- The HDSAM updates and the cold gas pathway are useful progress items. The fuel station footprint and wind-to-liquid hydrogen are only at the beginning stages and need further development.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- A good referral to other organizations with whom the project participants work was provided.
- Other national laboratories and DOE are involved with the project, which shows good collaboration.
- Good collaboration is demonstrated with other partners, but perhaps a bit more emphasis on their individual contributions should be given in the presentation. The project provided great acknowledgement on industry input toward station design.
- The collaboration with the other national laboratories is appropriate. It may be useful to have some collaboration with an energy company.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.8** for proposed future work.

- The team should not get wrapped up in exploring all potential options, especially when current pathways can be strengthened.
- The renewable hydrogen pathway analysis mentioned is very important. The costs for renewable hydrogen are not well understood and they show great variability. Determining these costs may be very important in influencing policy and decision making.
- Great plans for future work are presented.
- The future work with corresponding projected dates appears to be focused and builds on the past progress.

Strengths and weaknesses

Strengths
- This project includes many of the variables that are involved with getting hydrogen to the vehicle users.
- This project provides overarching direction and good framework for future research and development. Having easily changeable values that accommodate different input assumptions is also a strength of the model.
- The model provides the best assessment to focus research.

Weaknesses
- The project looks far into the future, and the further out, the more uncertain the reality of the specific pathway becomes.
- The project should include realistic footprints. Distances can generally be minimized by adding specific solutions and measures to the station (e.g., fire walls).
- Relevance to near-term costs could be a little more explicit. For example, information on how the model results guide next steps would be useful.
- The project needs industry input to calibrate models.

Specific recommendations and additions or deletions to the work scope

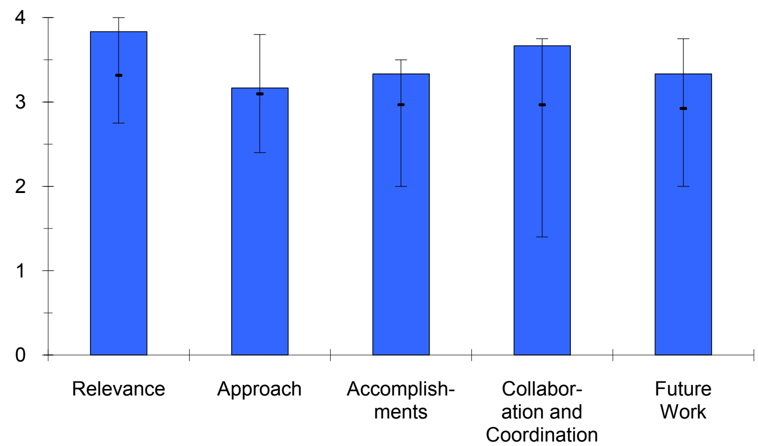
- Information is requested on how much additional use of unused capacity of hydrogen stations can bring the cost down.
- A slightly more explicit comparison (although some is already given) to onsite SMR costs would help frame the results. This would be especially useful going forward with regard to the renewable option. One of the discussed pathways for renewables is distributed natural gas production from renewable sources and renewable credits being given for SMR hydrogen production at the other end. The reviewer is unsure if this is beyond the scope of the project since this would really be natural gas production and delivery rather than hydrogen. A good understanding of these costs would still be helpful in order to compare to other scenarios such as wind or solar to hydrogen.
- The reviewer recommends including sensitivity analysis results as well as some explanation and/or validation assessment of assumptions.

Project # PD-15: H2A Delivery Analysis and H2A Delivery Components Model
Olga Sozinova; National Renewable Energy Laboratory

Brief Summary of Project

The objectives of this project are to 1) update and maintain the H2A Delivery Components Model, 2) provide a cost analysis on hydrogen delivery infrastructure, 3) support other models and analysis that include delivery costs, and 4) expand the H2A Components Model by designing new components.

Overall Project Score: 3.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- The team should make sure to continue the verification process with industry as it will be an enormous undertaking for a company to implement a wind-to-hydrogen project and transport it by rail over a distance greater than 1,500 km.
- This project is relevant and provides valuable information on pathways for hydrogen delivery. The addition of a railcar and renewable pathway is extremely important for future infrastructure growth analysis.
- The approach, as outlined in the "Approach" slide in the presentation, outlines the main relevance of the work. It is necessary to maintain commonality to provide a basis for all models using hydrogen costs.
- The modeling work for components allows the delivery team and others to focus on the greatest cost issues.
- The project is highly relevant to the overall delivery objectives as it concerns analysis of delivery costs. This project's outcomes are also coupled to the other cost analysis tools, in particular production and scenario-based models.
- The H2A model aligns with the DOE research, development, and deployment objectives and is an important tool for assessing the hydrogen delivery infrastructure.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- It appears that most aspects/variables are evaluated and included based on industry input.
- The project has good potential to feed information to the H2A model but should also develop simple-to-read, detailed summaries for industry to expand information on preferred pathways to economical delivery.
- Again, the approach seems sound. However, a disconnect appears to exist between the objectives and the approach. Maybe the approach is simply a guideline for the project and all the objectives are guided by that approach. Also, the title of the presentation is "H2A Delivery Analysis and H2A Delivery Components Model," but, in slide 9, no explicit indication of where "Delivery Analysis" fits in is shown. The reviewer is unsure if the delivery analysis was conducted using the Scenario Evaluation, Regionalization and Analysis (SERA) model.
- This project uses a very good approach that allows flexibility in configuration and evaluation.
- Details relating to the unique aspects of this project were lacking in the presentation. However, it was clear that the general objectives and intended outcomes of the modeling effort were well-established. This model also appeared to be highly flexible and capable of interacting with other related H2A (production) and HDSAM models, and the results from this H2A delivery model feed into more detailed analyses which can, for example, provide spatially and temporally variant information.
- The approach of the project of collaborating with the industry to develop the cost input is very good.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Make sure to report transportation distances in miles, not kilometers. Even though it may make more sense to use kilometers, industry uses miles.
- Excellent progress was made on updating the H2A delivery components.
- Significant progress has been made, but some questions remain about the SERA model. When looking at the map of pipelines, several seem to go straight through mountain ranges. Therefore, adding some realism based on natural gas pipeline locations and distances would be valuable. It appears this information is available based on slide 29. If the simplified map as shown is used, then perhaps a case study would aid in determining the uncertainty of costs.
- Very good accomplishments have been made to date. The reviewer is not certain if that much emphasis should be spent on rail analysis.
- The upgrades to the refueling stations in terms of dispensing pressure (to 700 bar), as well as inclusion of multiple dispensing options, clearly broaden the scope and relevance of the resulting cost data. While research aimed at understanding the impact of station footprint on cost was useful and appeared to be significant, this information seems like it could have been garnered via more cursory sensitivity analyses rather than the more detailed perspective that was taken.
- The studies related to rail as a delivery method, particularly for long distance transport, appear to be very interesting. The only criticism of the model is its static nature, which does not currently handle economies of scale with respect to component cost. One would imagine that low- versus high-volume component cost should be very different and should impact this analysis.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- The team is collaborating with an impressive group of companies on this project. Railcar vehicle builders should be included as well, due to potential challenges with developing and building code-compliant railcars with compressed composite hydrogen storage.
- Excellent partners are listed, but the team should try to incorporate more current hydrogen suppliers and state governments working with industry.
- This project is highly integrated and requires tight collaboration with other institutions.
- Good collaboration with others and industry is presented. The team should consider input on rail viability and costs from ethanol producers and their industry associations.
- This project relies heavily on interacting with multiple developers and specialists to obtain reliable input information. The investigators appear to be very proactive in seeking out the correct institutions for this information (e.g., talking with railroad entities for the rail analysis). Additionally, the highly interactive nature of this H2A delivery cost model with other related cost models (e.g., production and HDSAM) was made evident and appears to be in place.
- The collaboration effort appears to be highly coordinated with this project.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Make sure to consider fueling and defueling times of hydrogen transported by rail and potential related challenges for compressed hydrogen.
- A "go/no-go" decision on natural gas pipeline delivery is excellent since there is no need to waste resources if the pathway leads to a dead end. It is important to include buildup of hydrogen from wind scenarios.. Additional information on emissions is critical. Perhaps some help from Argonne National Laboratory could be incorporated.
- Future work seems appropriate and in line with the approach and objectives.
- Good plans are presented for the future.

- The future work clearly builds on progress and is appropriately tasked toward refining previous "mature" delivery components as well as expanding into new directions in terms of delivery options (i.e., hydrogen from wind).

Strengths and weaknesses

Strengths
- The project is very inclusive.
- Railcar delivery is very important, and the reviewer is glad to see it included. The project provides good industry values on delivery that assist with analysis.
- This project is important because it maintains commonality with other models.
- The model allows the costs that are first addressed to be those that most greatly affect the overall costs of hydrogen delivery.
- This project is comprised of a very competent team capable of analyzing diverse delivery pathways. The team is able to quickly modify the model to efficiently make delivery cost comparisons.

Weaknesses
- It is not clear what the current baseline is for hydrogen delivered cost.
- The project has some low volume delivery pathways that should be dropped since these are short term. The 100 kg station information is not currently relevant and should be wrapped up so the focus can be on the larger stations.
- The project seems to be a bit disjointed from the way it was presented. It is unclear whether the main purpose of this work is to maintain the components and delivery costs or to develop new models such as SERA. Some background information would be helpful.
- The model needs accurate and representative data for calibration and good predictions.

Specific recommendations and additions or deletions to the work scope

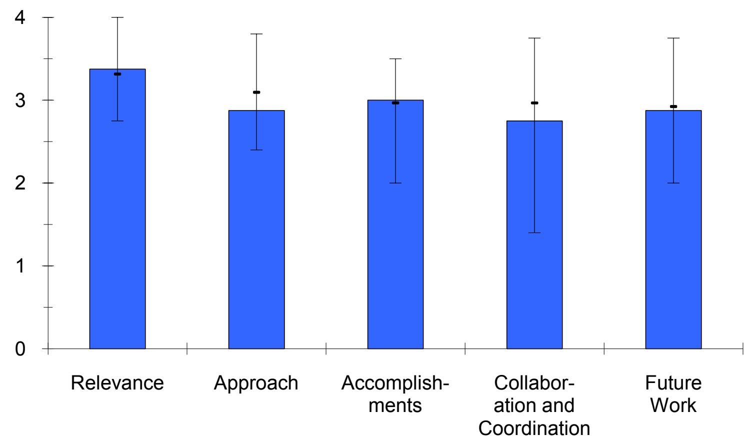
- The current baseline of hydrogen cost and improvement over last five years should be added.
- Continue with the detailed analysis of the railcar delivery. Renewable delivery is critical for California and should be examined in detail for specific station pathways in the Los Angeles and San Francisco Bay areas.
- The team should run the model at multiple assumed component volumes to get some sensitivity of the extent of delivery infrastructure versus component cost.

Project # PD-16: Oil-Free Centrifugal Hydrogen Compression Technology Demonstration
Hooshang Heshmat; Mohawk Innovative Technologies

Brief Summary of Project

The objective of this project is to demonstrate key technologies needed to develop reliable and cost-effective centrifugal compressors for hydrogen transport and delivery, including 1) flow of 500,000 to 1,000,000 kg/day; 2) pressure rise to 300-500 psig up to 1,200-1,500 psig; and 3) contaminant-free and oil-free hydrogen.

Overall Project Score: 3.0 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The reduction of hydrogen cost per kilogram at the production plant is best for current technology improvement, since the production plant is where the majority of hydrogen production will occur for the foreseeable future.
- Pipeline delivery would be the long-term low-cost supply option and will require compressors for pipeline transportation.
- This is an excellent engineering project. The reviewer is not sure it supports mid-term goals, but believes it is very relevant in long-term goals. The project may be at too early of a stage for a decision on mid-term delivery methods.
- This project is very relevant to the hydrogen delivery arena. Compressor costs at the terminal/plant can have a significant impact on hydrogen delivery cost.
- This project supports DOE Hydrogen Program's effort to advance hydrogen compression from central production for pipeline transportation, as long as research and development is being supported in other areas of central production and delivery of the hydrogen to the pump (e.g., central production, pipeline, and storage and dispensing).
- Centrifugal compressors are a vital enabling technology for hydrogen pipelines and would lower forecourt costs as well.
- Oil-free compression leading to contaminant-free and oil-free hydrogen is a major step in the direction necessary for fuels from hydrogen to work.
- This project could be more relevant if it also developed compression for dispensing at 6,250 psi and/or 12,500 psi.
- The project target was clearly explained, but additional linkage of targets to the overall DOE research, development, and deployment objectives would be useful.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- It is unclear whether input was obtained from hydrogen plant operators. If not, communication with engineers at a plant that operates with current hydrogen compression equipment would be helpful. Often compressors are cause of plant shutdowns if they break down, resulting in several millions of dollars in revenue lost per day of non-operation.
- The team incorporated an excellent use of computational fluid dynamics (CFD) and finite element analysis in design prior to constructing test rig.

- The project does not appear to be integrated with other efforts. Technical barriers are being addressed and have yielded some results, but work needs to continue to address further barriers.
- A focus on technology development and new concepts is the right approach for development of ultra-high capacity hydrogen compressors under these conditions since existing capabilities are limited.
- The approach is fairly comprehensive and very representative of how typical development happens in the large compressor industry.
- Testing of a single compressor stage is key to validate the success of theory.
- Design development tied to manufacturability is key to successful manufacturing of the technology.
- The approach addresses materials, design, modeling and simulation, and fabrication/manufacturing. The project team should consider looking at Design for Manufacturing and Assembly early in the design.
- The design work has proceeded well.
- Mohawk Innovative Technology, Inc. (MITI)   is carrying out all aspects of design, including CFD and mechanical stability.
- Flexibility in drive (electric or turbine) is a plus.
- Foil bearings and seals are the key to this project. It is unclear whether work has been for hydrogen compression or other scenarios. The presenter mentioned that the work is from NASA''s Space Shuttle Main Engine work with liquids, which raises questions on how well they are working with gas and what issues have arisen.
- Materials are believed to be the critical initial design component. More work and discussion of the materials issues would be helpful. There was much discussion of stresses but little information about embrittlement and hydrogen loss due to leaks.
- Compressors may not be run at a constant rate as the design requires. Further work to deal with turn-up and turn-down is necessary.
- The approach should be expanded to the complete compressor including the gearbox assessment.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- It is unclear why within a 15-year durability target this equipment would be rated for 30,000 start and stops, as stated during the presentation project. This would translate in close to 5.5 starts and stops daily, though none of the pipeline-connected hydrogen production plants shut down this many times.
- Model projections look promising to meet or exceed DOE targets with an opportunity to demonstrate in a test rig this year.
- Good progress has been made on overall design of the compressor, including stage selection and other compressor parameters.
- Good progress has been made on numerical and CFD design of impellers and stress criteria.
- A very solid approach is used to design and manufacturing, with progress on track.
- The project seems to progress down the front-end design work. However, the project needs to proceed to on-stream testing of the design concept to determine strengths and weaknesses in the design and to show how this design differentiates from other competing DOE-funded projects in this area. This will help DOE downselect the "winning" design.
- The design and CFD work in 2010 clears the way for prototype construction.
- Good progress was made in materials selection.
- Only preliminary testing has taken place to date. The reviewer is concerned about the long life requirements of pipeline delivery and the testing necessary for such. The design incorporates these issues but not enough information was provided to warrant sufficient durability.
- More work should be done on hydrogen embrittlement and the potential related issues.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- It is unclear whether or not input was obtained from hydrogen plant operators. If not, communication with engineers at a plant that operates with current hydrogen compression equipment would be helpful. Often

compressors are the cause of plant shutdowns if they break down, resulting in several millions of dollars in revenue lost per day of non-operation.
- The presentation did not reflect input from entities besides DOE, MITI, and two consultants. It was unclear if any input was received from national laboratory material scientists on potential challenges.
- A small list of partners was provided, but they appear to have the experience necessary to execute design and testing.
- There appears to be very little collaboration or coordination among organizations.
- Mitsubishi Heavy Industries (MHI) and MITI are the right partners for this approach given that– they are the technology leaders in this industry.
- Good work was achieved with MHI. The team should consider consulting with a metallurgist with expertise in the hydrogen embrittlement area to review the design and selection of wetted parts.
- Collaboration with MHI (or equivalent) is essential to move this technology toward commercialization.
- The companies necessary to produce and sell these compressors are involved and coordinated.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- Data from single-stage device tests will demonstrate whether or not all of the efforts and calculations were correct.
- Reducing the design to practice with single-stage performance testing will be a large step in this year's plan.
- Single-stage testing is critical to validation of the approach.
- Economic estimate updates will help the program determine feasibility of this approach.
- The team needs to get to proof-of-concept testing on a full compressor system.
- Construction and operation of a prototype and completion of an economic analysis will be critical.
- The team is at the point where testing is appropriate and is the major priority for future work. More information on the testing plans would have been useful. Most concern lies with the durability of the compressor as a system.

Strengths and weaknesses

Strengths
- This project appears to cover all variables.
- The novel design used in this project is promising.
- No oil is used in this project.
- The project's projected low cost and small footprint are strengths.
- This project provides good engineering analysis of a compressor design.
- An excellent technical team and approach are used in this project.
- Solid design and analysis is combined with MITI and MHI's expertise in compressor and bearing technology.
- Near-term applications in chemical and refining industries provide a "market pull" independent of hydrogen-powered transportation.
- It is important to the program's objectives to develop efficient, contaminant-free compressors.

Weaknesses
- No (single-stage) system testing data is provided.
- The presentation gave very optimistic numbers compared to DOE targets.
- The project funding is high for results provided so far. The project should be further along, and a single-stage compressor should be in the manufacturing stage. The team needs to account for two years and $1.5 million.
- No significant weaknesses were determined.
- The design is for constant use with little turndown. The reviewer is doubtful that this service will be available within the next 20 years, so further work on turndown is necessary.
- Further testing on materials is necessary. The reviewer has low confidence that the foil bearings and seals will be as leak-free as the project investigator indicates.

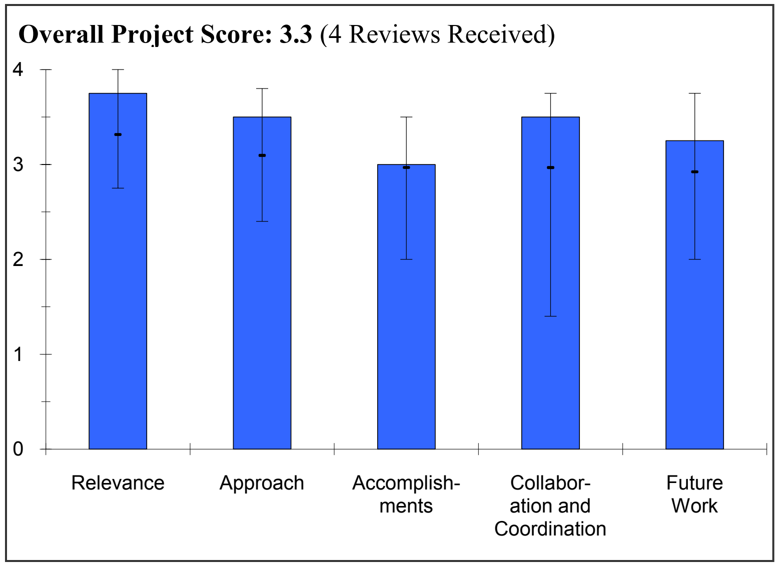
Specific recommendations and additions or deletions to the work scope

- The project should add a benchmark compressor (rated for same throughput) set up to compare cost, durability, reliability, maintenance, and efficiency numbers, even if the new compressor design has not been tested.
- A production rate of 200,000 kg/ day is a long-term need. The team should consider feedback from the analysis group for pipeline compression requirements in the transition phase. They may be able to coordinate scale-up of the equipment with transition requirements.
- The scope is appropriate, but more testing and an expedited move to actual building of a single-stage compressor would be preferred.
- The team should work on compressor technologies that do not need consistent service.
- Further testing on materials is needed.
- The team needs to include the reliability and robustness assessment of the design versus existing technologies.

Project # PD-17: Development of a Centrifugal Hydrogen Pipeline Gas Compressor
Frank Di Bella; Concepts NREC

Brief Summary of Project

The overall objective of this project is to demonstrate an advanced centrifugal compressor system for high-pressure hydrogen pipeline transport to support DOE's Strategic Hydrogen Economy Infrastructure Plan. Objectives are to 1) deliver 1,200+ psig and 100,000 to 1,000,000 kg/day of pure hydrogen to a forecourt station at less than $1/gge; 2) reduce initial installed system equipment cost to less than $5.4 million uninstalled based on DOE's HDSAM 2.0 Economics Model, which is a component of the H2A project; 3) reduce operating and maintenance costs via improved reliability; and 4) reduce the system footprint.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Pipeline compressors will be required for the projected low-cost long term-pipeline delivery option.
- The pipeline compressor can provide a critical component to the long-term success of the hydrogen infrastructure, and this project provides a key piece of development to the process.
- Compression cost is one of the key barriers preventing cost-effective delivery of compressed hydrogen for fuel cell vehicles. This project, along with the project conducted by MHI, are critical to reduce the high cost of compression.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- A phased program approach and use of state-of-the-art and acceptable engineering practices reduces risk during development.
- The team established a sound approach to project barriers, and it appears the project will address all issues associated with pipeline compressors.
- The project has a good approach, using various outside experts to provide the key component technology. The reviewer has concerns about the assumption of hydrogen contamination by the outside seal vendor. The team should consider a demonstration of this contamination level.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Passing the "go/no-go" decision on December 9, 2010, is an accomplishment for the team.
- Accomplishments to date show good results towards DOE goals. The material selection method is good; however, more analysis is needed on the embrittlement and stress factors at tip speed.
- Good progress has been made on reaching project objectives.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- An excellent mix of industry, academic, and national laboratory partners is established for this project.
- Collaborations are solid and appear to be appropriate.
- Good collaboration is demonstrated with outside compressor vendors. Detailed input from university partners would have been appreciated to see how they contribute.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- A complete detailed cost analysis is included to confirm the low $4.5 million compressor package cost.
- The amount of future project work and the timeline appear to be a challenge. It will be a difficult task to complete design, component procurement, and testing within a year. DOE should ensure they are on target.
- Good plans are provided for the next step of assembly and testing of detailed designs.

Strengths and weaknesses

Strengths
- Identification of commercially available components and multiple suppliers has been done.
- The project appears to have reliable results and should achieve most DOE targets.
- The project will prepare an effective centrifugal design and demonstration for large-scale hydrogen compression.

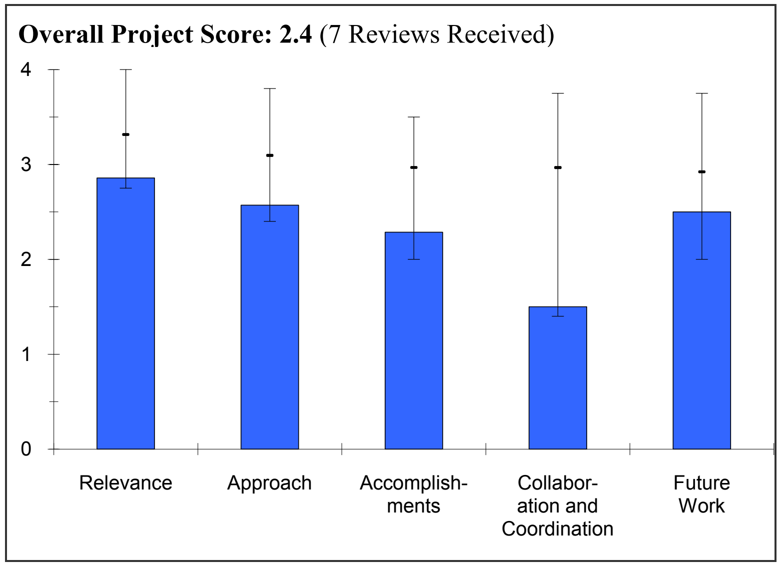
Weaknesses
- The overall vision for the industry is not presented well. The goal of achieving 1,000,000 kg/day is not clearly defined.
- This project relies on assumptions of performance by collaborators in this project.

Specific recommendations and additions or deletions to the work scope

- The team should consider increasing the number of stages and lower the tip speed if the cost is determined within reason.

Project # PD-18: Advanced Hydrogen Liquefaction Process
Joe Schwartz; Praxair

Brief Summary of Project



The overall objective of this project is to develop a low-cost hydrogen liquefaction system for 30 and 300 tons/day that meets or exceeds DOE targets for 2012. Objectives are to 1) improve liquefaction energy efficiency, 2) reduce liquefier capital cost, 3) integrate improved process equipment, 4) continue ortho-para conversion process development, 5) integrate improved ortho-para conversion process, and 6) develop an optimized new liquefaction process based on new equipment and new ortho-para conversion process. Goals for Phase II include 1) process development to establish performance targets for process equipment and ortho-para conversion and 2) development of a preliminary capital cost estimate.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- This is a very relevant project since liquid hydrogen is especially important to keep early hydrogen infrastructure equipment design as simple as possible and to capital cost down.
- Any improvement in the efficiency and cost of liquid hydrogen production will benefit dispensed hydrogen cost per kilogram in early stages. This is especially true because, at lower delivery quantities, liquid hydrogen is very expensive due to the limited number of liquid hydrogen production facilities. The distance the customer is from a liquid hydrogen production point significantly influences the cost of 1,000 scf delivered.
- Liquefaction supports the transition cost goals, but not long-term hydrogen cost goals.
- Improved efficiencies are an important part of developing hydrogen pathways; however, this only partially supports the goals and objectives of the Hydrogen Program. These challenges may be addressed by individual companies and not necessarily DOE.
- Liquefaction is very relevant to mid-term hydrogen infrastructure deployment, and the cost of liquefaction is key to the success of this approach.
- Praxiar, Inc. already employs hydrogen liquefaction as part of its commercial business. Thus, it has a vested interest, for its own business purposes, to improve the efficiency of hydrogen liquefaction. This project could then be seen as government support of hydrogen liquefaction business development. The project investigator stated that this approach will find "limited application in a potential hydrogen economy…" The reviewer would add "…as it pertains to the specific DOE Hydrogen Program objectives," because it may be a different story elsewhere.
- The project's goal to reduce energy and the cost of liquefaction has a direct impact on delivered hydrogen cost reduction.
- Given that hydrogen liquefaction is becoming increasingly important and relevant as a potential delivery option, this project's scope supports the overall DOE effort. In particular, efforts to achieve the delivery cost and efficiency are core to this project's principle objective of developing a new liquefaction system.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- Software modeling may be very valuable for future efforts, development theories, and knowledge pertaining to ortho-para conversion.
- The team has a good approach to build on previous success with Edison Material Technology Center (EMTEC) funding.
- No details are provided on ortho-para conversion experimental work.
- The approach is not clearly defined and the results to date are not clearly stated. The project does not appear to be on track to overcome specific barriers of high energy use for liquefaction. Instead, only incremental improvement has been made.
- This project's approach is fair and is comprised of a combination of modeling and prototype concept testing.
- Praxair's expertise in liquid hydrogen should help in a successful development and delivery of this concept. They are among the experts in the gas liquefaction field; hence, it is assumed they know what they are doing.
- The project has a good approach to use an idea to reduce the energy requirement for ortho-para conversion. The team should review and cite any new literature and research on ortho-para conversion technology developments.
- It is clear that this project builds on work and results from a previous short-term study (EMTEC-funded). At the broadest level, this project focuses on expansion of that program to investigate similar liquefaction processes that involve minimization of the effects of ortho-para conversion during liquefaction. Beyond that, it is unclear what the specific technical approach is since it appears to be proprietary. The programmatic approach, which includes process design and optimization, equipment evaluation, and demonstration, is appropriate.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- The "go/no-go" indicators for efforts on this topic are not clear. From a cost perspective, it may not be worthwhile to continue efforts in this area at some point in the future because, for example, pipeline implementation may outdo this technology when passing 100,000 kg/day production.
- DOE has performance indicators for large-scale liquefaction (300,000 kg/day of hydrogen), but there are no installations in the United States of that scale for liquid hydrogen production. According to the presenter, 30,000 kg/day of liquid hydrogen is the largest existing production volume in the nation. A pipeline as an alternative option may be a better alternative from a cost, efficiency, and environmental perspective.
- The model ortho-para conversion was delivered to DOE but was not included in the presentation.
- The addition of ortho-para to the simulation program is an improvement in the state-of-the-art of modeling.
- Currently could achieve 75% efficiency (11 kW/kg 2012 target), but the reviewer does not expect to be able to get to 85% efficiency (6 kW/kg 2017 target).
- The technical progress is difficult to determine. Model development has shown modest progress with the ortho-para additions to the model.
- The team needs to show the current projections and status of this approach against the DOE targets. None of those was mentioned in the slide package.
- Good progress on ortho-para conversion reduction was documented. The team needs to mention exact numbers on efficiency with the new conversion approach.
- Fair progress on thermodynamic model was made, but it is still hard to determine the exact technology status.
- The project investigator was unable to provide cost figures for current or projected hydrogen liquefaction production, though they must know how much it costs to do the hydrogen gas-to-liquid conversion. Also, DOE targets for 2010 should be given in dollar units.
- The cost of liquefaction may not be important for the Praxair liquid hydrogen business, but it is critical for this DOE Hydrogen Program mission.
- Good accomplishments have been made since the 2009 review.
- The results of modeling efforts were vague (because of proprietary nature), but certain processes appeared to show promise for meeting the efficiency targets. No information on the cost of this process was currently given, but this was listed as future work. Progress on constructing and demonstrating large and small test fixtures were evident, but data from the testing of these prototypes was absent.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.5** for technology transfer and collaboration.

- The team should collaborate with a national laboratory to establish, verify, or evaluate current theories related to liquid hydrogen production and ortho-para conversion, even if this data is not shared with the public. If DOE funds this effort, the numbers should be verified as opposed to Praxair stating that they have the knowledge in house to complete all facets of this program. Alternatively, the process that they use in house can be shown to verify their efficiency and cost numbers. This would provide some sort of process that resembles an independent review. This also follows on the responses given to one of the 2009 reviewer comments.
- No collaboration is demonstrated.
- No collaboration or coordination with other organizations was shown.
- The team needs to have collaboration for verification of existing developments as well as to create more ideas and progress on future developments.
- Collaboration was only with the thermodynamics software provider who was not cited.
- No collaborations were evident. The response to this reoccurring issue, which was given in a supplemental document, was that they are "qualified to complete all facets of the proposed program." While this may be true in some respects, at a minimum, the outcomes of this project should be shared and information exchanged with other relevant projects. External validation of data is also an important part of research and development.
- Part of the spirit of DOE-funded research opportunities is to collaborate with the scientific and engineering community.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- From the presentation and indications from the presenter, it appears that many unknowns exist about current technology numbers, in particular efficiencies numbers. Benchmarks for or to clarification of these numbers would be helpful for both Praxair and DOE.
- Optimizing the ortho-para conversion process will lead to improved efficiency.
- Future work may provide some insight into future process development. However, it will not address critical barriers or provide large efficiency improvements.
- Many achievements must be made for this project to be successful. The current status is not clear.
- Good plans are provided for future work, including the process design of improved liquefaction.
- The future work seems consistent with the objectives and are logical next steps of the current work.

Strengths and weaknesses

Strengths
- Liquid hydrogen is currently the highest density delivery option.
- Modeling numbers suggest good potential for this process.
- A knowledgeable industry leader is working on a project, and they likely know best. The project does have potential to reduce energy use in liquefaction by optimizing process.
- No significant strengths were identified apart from Praxair's extensive experience in hydrogen liquefaction.
- Praxair is the leading expert in the business of providing commercial quantities of liquid hydrogen.
- The project team hopes to deliver a more energy-efficient process for liquid hydrogen.
- This competent team is focused on a very relevant topic.

Weaknesses
- Verification process of efficiency numbers is lacking.
- No clear comparison of benefits was provided for alternative production and delivery methods for hydrogen.
- The project leader has not shown potential for large efficiency gains, which are necessary for future hydrogen pathways. The information provided was limited since it was presented at a very high level and lacking detail.
- The project showed a very poor execution and reporting on DOE targets.

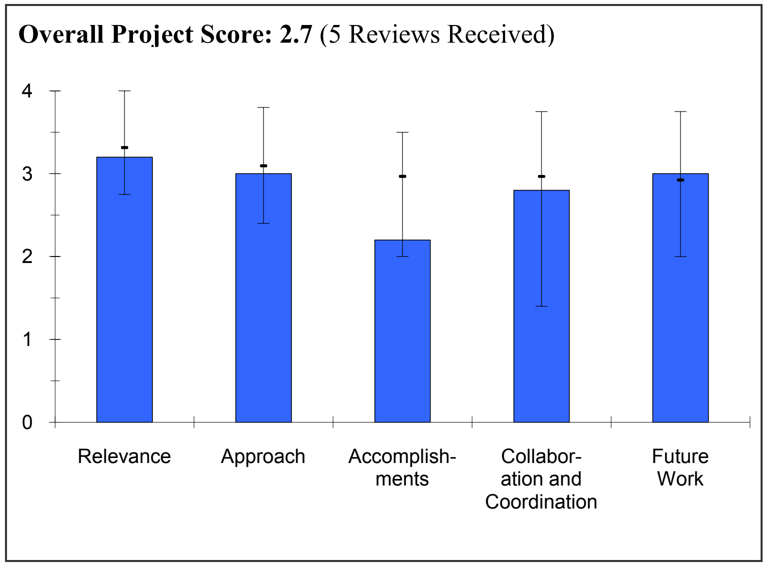
- Due to cost considerations, this project cannot meet the objectives of this DOE EERE mission.
- Technical details of process design improvements are not disclosed to reviewers.
- This project is difficult to review given the very vague descriptions that lack detail. The team is not collaborating.

Specific recommendations and additions or deletions to the work scope

- Clarification of the verification process is needed.
- If possible, add benchmarks with current technology used for liquid hydrogen production.
- Report future work on estimated capital cost as it compares to the DOE target and expected efficiency improvements.
- Clearly outline how the project will improve the liquefaction efficiency. State the status of the industry, and demonstrate how this project will improve industry. For example, if the industry average for liquefaction energy use is 12 kWh/kg, explain that this project has the potential to reduce energy use to 9 kWh/kg. If the project has little ability to reduce energy efficiency, then the project should focus on cost reductions of equipment, which is part of Phase III.
- DOE needs to pursue execution of this project and ensure delivery. Also, the team should adhere to the format of the presentation, such as reporting on current status versus targets.
- Analysis of this approach using H2A/HDSAM is recommended to establish the total costs.
- The team should provide costs of liquefaction in dollars per kilogram of hydrogen. Both current and future targets should be included.

Project # PD-19: Active Magnetic Regenerative Liquefier
John Barclay; Prometheus Energy

Brief Summary of Project



The main focus of the DOE active magnetic regenerative liquefier (AMRL) project is to analyze, design, fabricate, and test liquefier prototypes to develop advanced liquefier technology that meets DOE's targets for capital cost and energy efficiency for delivery of liquid hydrogen. DOE's efficiency target is 75% at 30,000kg H_2/day with $40 MM for a 'turn key facility' (~$353/gpd). In the last year, the objectives have been to 1) design, fabricate, and test the first reciprocating AMRL prototype to successfully span from approximately 290 to 120 K; 2) experimentally answer four key questions that have been identified regarding fabrication of magnetic regenerators with bypass flow of the heat transfer fluid; 3) use measured performance data to validate the sophisticated process simulation model for the design of optimal AMRLs; and 4) incorporate lessons learned from first prototype into the design basis of the second AMRL prototype to span from approximately 290 to 20 K and produce liquid hydrogen with a figure of merit of approximately 0.5.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project is more valuable for liquid natural gas production and distribution than liquid hydrogen production at this point, because liquid hydrogen may be too large of a step beyond current work, especially considering the current delays and complexity of the system.
- Lowering the cost of hydrogen liquefaction is an important goal for improving the economics of hydrogen distribution to the forecourt.
- Relevance to the DOE Hydrogen Program is high due to the ability to potentially provide a step change in the cost, efficiency, and supply of liquid hydrogen.
- Low-cost liquefaction is key to achieving DOE hydrogen delivery cost and performance goals. No cost projections were presented, so it is unclear whether this system would be less expensive than conventional liquefaction processes. Key cost drivers should be presented.
- It would be helpful if the researchers presented their projected efficiency using the same methodology as the DOE program.
- This project appears to be very relevant to identifying new liquefaction technology not based on compression. Since liquefaction is receiving much more attention as a promising method for delivery (and of potential use for cryo-based storage methods), this project is increasingly useful for achieving enhanced efficiency at a reduced cost.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Many different issues have been considered to design and produce a working device/prototype.
- The project currently appears too complicated to be able to successfully address the development of all the new components of the system and conclude the project.

- The technical approach to testing the AMRL prototype is sound.
- The project approach provides good metrics to create a foundation for subsequent steps.
- The project technical approach will allow researchers to test their complex concept in stages. It is not clear how the lessons learned from the reciprocating design will be translated to the rotary design.
- The approach of magnetic regeneration liquefiers is very unique and novel. It is also applicable to this area. The technical approach appears to strike an appropriate balance between materials/components research and selection as well as fabrication and testing, which seems necessary in this project given its relative technical immaturity.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.2** based on accomplishments.

- It is difficult to see how this project can be accomplished on time if it is already eight months behind schedule. The eight month delay cannot be ignored, even though progress was made.
- No clear comparison of improvement is provided since testing data is not available yet.
- The project has not delivered any data to support the thesis that the AMRL technology can accomplish hydrogen liquefaction with higher efficiency than the current practice. The project is behind schedule on these key objectives.
- Technical accomplishments have been delayed and appear to be not due to technology but instead project ownership and materials. More project accomplishments should be shown, however, to determine future needs.
- The project is behind schedule by over six months. The prototype unit is not complete, so overall progress toward answering the relevant performance questions cannot be determined at this point.
- The assumption that the temperature range can be sufficiently expanded seems optimistic.
- Despite timing setbacks related to transitions in company ownership, a great deal of progress was made with respect to component/subsystem selection and fabrication, creation of a prototype design, modeling, and assembly of hardware.
- This project seems highly efficient and focused on making progress towards demonstration of this technology.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Collaboration appears to be sufficient.
- The project should include better comparison with existing technologies and their efficiencies.
- The team has good collaborations with leading academic researchers.
- More collaboration would be preferred, perhaps with Florida State University (e.g., National High Magnetic Field Laboratory) and national laboratories.
- The existing collaborations are appropriate for the project.
- While some collaborations exist, the level of participation in the project for each organization is unclear. However, this team appears to be working very sufficiently despite the lack of collaborators. The only suggested collaborations might be with other liquefier developers for eventual corroboration/validation of results.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The “go/no-go” milestones are unclear.
- One of the current manufacturers, users, or experts of liquid hydrogen production units should be included for industry feedback.
- It is important that the future work is directed towards obtaining data with the prototype.
- The next three months of work are critical to the “go/no-go” decision.
- Given progress to date, the researchers' planned progress for the remainder of FY 10 seems optimistic.

- Future work seems well-planned and reasonable to achieve milestones. It is great to see a "lessons learned" activity based on the first prototype. The organization and project management aspects of this project are very impressive.

Strengths and weaknesses

Strengths
- This project has potential, based on the method used.
- Good theoretical background is used to support the development of the AMRL technology. A sound plan is provided for accomplishing the objectives of proving the technology.
- The project has a nice upside potential to dramatically reduce energy use for liquefaction.
- Low-cost high-efficiency liquefaction is critical to achieving hydrogen delivery cost goals.
- The project is comprised of a highly organized, focused, and efficient team. Progress on the project has been great.

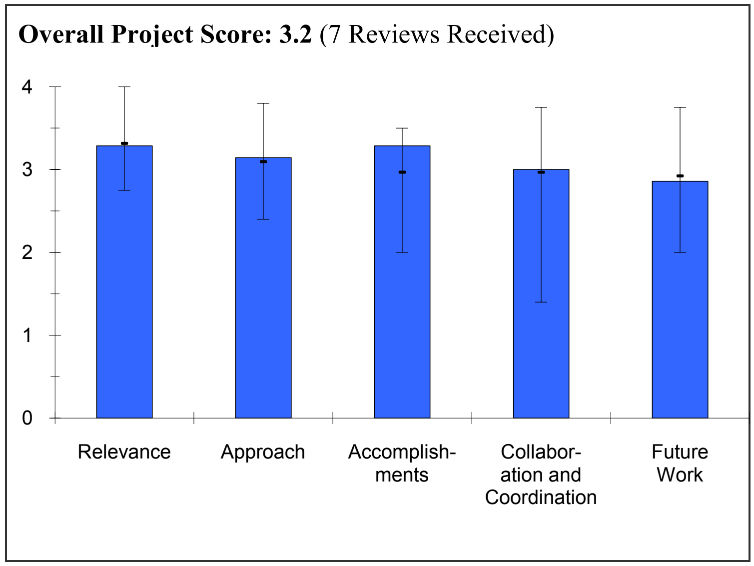
Weaknesses
- This project employs a new method.
- No test data of a complete system is available.
- When presented, too much information was given on calculations.
- The presentation of the project needs to show clearer steps.
- The presentation does not clearly show the different components that must be developed or the status and targeted goals for each component.
- The project lacked progress towards meeting the project objectives, although it is unclear how funding limitations may have contributed.
- This project lacks collaboration and cost projections.
- Changing from a reciprocating regenerator to a rotating regenerator appears to require redesign and recalculation of the force balancing mechanisms, heat shields, etc.

Specific recommendations and additions or deletions to the work scope

- DOE should better define for what components the efficiency numbers are calculated so that better comparisons can then be made.
- The team should include a comparison with existing liquid hydrogen equipment under strict definition that is set by DOE. This way, an "apples to apples" comparison can be made.
- It is worth questioning at an early stage whether this technology could be utilized at large, commercial scales to produce "tonnage" quantities of liquid hydrogen. A better analysis of the potential to scale up this technology is needed.
- DOE cost goals are mentioned, but no estimates on how this system would help achieve these goals are provided.
- An experimental demonstration of full temperature range cooling with no gaps is recommended before detailed design and efficiency optimization are completed. It was unclear from the presentation whether this has already been done.

Project # PD-20: Inexpensive Delivery of Cold Hydrogen in Glass Fiber Composite Pressure Vessels
Andrew Weisberg; Lawrence Livermore National Laboratory

Brief Summary of Project



The objective of this project is to produce glass fiber composite pressure vessels for the delivery of cold hydrogen. Glass fiber vessels reduce hydrogen delivery cost through synergy between low-temperature (140 K) hydrogen densification and glass fiber strengthening. Benefits of glass fiber vessels include 1) colder temperatures (~140 K) increase density by approximately 70% with small increases in theoretical storage energy requirements and can be achieved at gas-terminal scale with liquefied natural gas refrigerators; 2) low temperatures are synergistic with glass fiber composites; 3) glass fiber minimizes high composite materials cost (approximately $6/kg for glass versus approximately $23/kg for carbon fiber); 4) increased pressure (7,000 psi) minimizes delivered hydrogen costs and the same design can deliver up to 12,000 psi or build cascade; and 5) dispensing of cold hydrogen reduces vehicle vessel cost by approximately 25% by avoiding over-pressurization during fast fill.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This project is very beneficial for developing new tanks for transportation of hydrogen and eventually reduction of cost compared to currently used technologies (e.g., tube and liquid hydrogen trailers).
- Compressed gaseous storage for delivery may or may not be the future delivery method. The project will address the near-term issues if it is a success.
- Reduction of cost for hydrogen delivery via new and different approaches helps to overcome one of the critical barriers for hydrogen infrastructure.
- Project objectives appear to be critical for the Hydrogen Program to meet their long-term goals and objectives. The cold glass process improves strength effect.
- This project is not critical to program success but would make a significant contribution to making hydrogen delivery affordable.
- The team made a good discovery of the effects of cold temperatures on increased strength of glass fibers. National laboratories may not be the ideal entity to be designing cryogenic pressure vessels rather than a vendor who does fiber-reinforced polymer vessels as a business.
- The focus on low-cost pressure vessels is aligned with the Hydrogen Program objectives.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Great planning and slow scale-up testing was performed in this project, as the team learned the scale presented some unknown issues.
- Glass fibers are a good approach relative to carbon fibers from a cost perspective. The team needs to establish that glass fibers are as good as carbon fibers in terms of reliability and durability since multiple failures have already been seen.

- The team needs to add the infrastructure for low-temperature cooling at the central hydrogen production plant. Therefore, they will need new and totally different infrastructure. First, the team needs to establish the requirements of temperature from full process (e.g., from manufacturing to refueling to vehicle tank). Then, they can go about developing this material.
- The team made good use of advanced materials to incorporate a composite systems configuration. The project contributes to overcoming of economic barriers. The project has a weakness in evaluating pressure cycling and observing the long-term effects to the integrity of the system.
- The program has progressed from fundamental materials to large-scale tanks. Optimization of polymers and fibers has progressed well.
- Short term applications for transport of other cryogens provide a "market pull" independent of the Hydrogen Program.
- This project has a good approach, but quicker results could be obtained with closer collaboration with a vessel manufacturer.
- The approach for the thermoset liner could be better explained. The previous issues with glass fibers (i.e., chemical exposure) need to be addressed and discussed regarding the validation of the protective coatings. Further validation testing is needed to confirm the cylinder design will pass the certification testing.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The prototype is in process of being built.
- Technical accomplishments to date are excellent. The development so far will be critical to future work.
- Much progress on fiber development has been made, but many material failures have occurred. The team needs to establish the right conditions (e.g., temperature) for this operation.
- The team needs to do full value chain modeling for hydrogen, including refrigeration infrastructure at the central plant with more resolution.
- The team needs to describe and optimize the full value chain process for this concept starting from hydrogen production to distribution and refueling into the car tank.
- The team is focused on meeting the program delivery cost projections, which relate to the cost to transfer to storage systems while increasing lifespan.
- Significant progress has been made in moving from laboratory to commercial tanks.
- Good technology accomplishments have been made, but the pace of development may be increased with a vendor performing more of the vessel development based on the laboratory’s innovations.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The team should consider verifying data with Sandia National Laboratories and codes and standards organizations to determine if this work can be covered under current codes and standards when integrated in trailer and/or railcars. If a gap exists, there should be an effort to address it, because it may cause a hold up for the commercialization of this technology.
- The project has excellent partners and collaborators with alternative goals, which helps move the project along.
- The team urgently needs to collaborate with IGC (and other energy companies) and original equipment manufacturers on the refrigeration and refueling approach.
- Collaboration with industry appears to be good, and partners are fairly well-coordinated.
- Lawrence Livermore National Laboratory has done an excellent job of engaging industry to move this project forward.
- Collaboration could be stronger with a vessel manufacturer rather than working at the laboratory.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- Regulatory initiatives are extremely important and should be incorporated with the container field demonstration. Industrial partnerships will also be critical to the regulatory initiatives and should be involved.
- Much proposed future work is presented, but the team first needs to determine the sweet spot (e.g., temperature and pressure) for this approach.
- Plans for future work need to include full-scale testing of pressure cycling of the storage tank to a projected industry life. Then, the team should conduct a full-scale burst test.
- The team presented a good set of objectives for this near-commercial technology.
- Vessel development should be moved to a competent vendor.
- Future work should include additional analysis of potential failure modes to ensure the project addresses them.

Strengths and weaknesses

Strengths
- The caliber of organizations involved is strong.
- The resulting product and use of cheaper materials are both advantageous.
- A potential end user (Spencer Composites) has shown interest in using the product for integration in existing aerospace, maritime, and energy terminal equipment.
- The team's technical knowledge of testing is strong. The project plan and processes are sound. A cost breakdown of advantages over other methods (e.g., steel at $485/kg capacity versus glass/epoxy at $130/kg) is provided.
- Technical execution of development of glass fiber concept has been performed.
- The project has good coordination with the hydrogen industry.
- The project has good involvement of industrial partners.
- The team has demonstrated continual improvement of technology and evolution from concept to real devices.
- The team has demonstrated a novel and insightful knowledge of materials and composites from a fundamental view.

Weaknesses
- This is a new product.
- The safety and standards will be critical to the adoption of this technology. More work should be done concerning this area. This project has great potential to reduce cost for hydrogen delivery; however, the reviewer is concerned with the safety aspects of the final project. Stakeholders may think "this is great but is it still safe?" Great emphasis should be placed on the safety aspects of this technology. Also, it is unclear how volume production of these vessels can be consistent and provide a confidence in the vessel uniformity. The expected cycle life of the full-scale vessels needs to be addressed.
- The team has demonstrated poor coordination on requirements and on determining feasibility of the overall solution.
- The project investigator should talk with other storage tank industries to learn what they are developing as far as new materials.
- The project has no significant weaknesses.
- National laboratories are not manufacturers of vessels.

Specific recommendations and additions or deletions to the work scope

- The team should verify codes and standards. If a gap exists, then it should be addressed.
- The targeted lifetime and number of expected usage cycles should be defined.
- For the production method, the team should find out how the textile industry can contribute to this project, due to the used production methods for weaving fabrics at a fast pace. This may be of use when developing a reliable and fast production method for these storage tanks, which will bring the cost down and possibly improve quality. This last point does not appear to be addressed in the project yet. The team should work with the H2A group to determine when and what would be necessary to add to the model when full-scale tests prove positive (assuming that occurs). The team should also collaborate with hydrogen providers to see which steps they would suggest adding to Phase III (if this has not already been done).

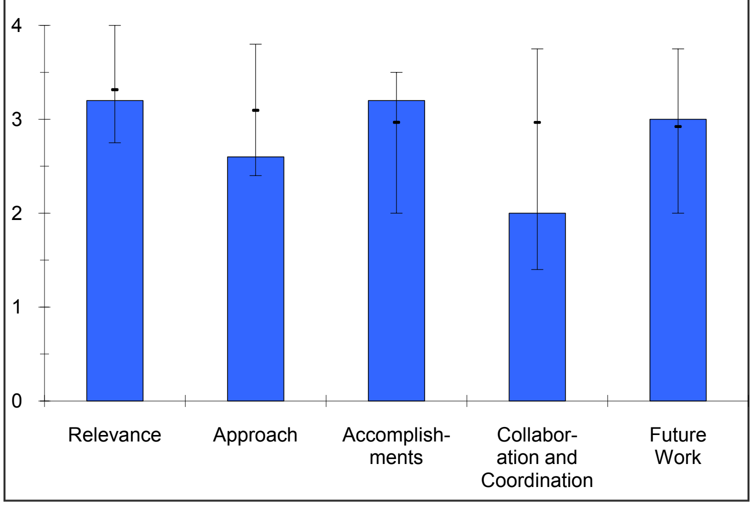
- The project should add the full process design and optimization (from cost perspective) to the scope over the lifecycle of the solution (from manufacturing to storage in the car tank).
- The team needs to pressure cycle the storage tank to projected life and then conduct a burst test. The barrier requirements for water and oxygen should be reviewed.
- The team should move vessel manufacturing from the laboratory to a vendor location.

Project # PD-21: Development of High Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery
Don Baldwin; Lincoln Composites

Brief Summary of Project

The overall objective of this project is to design and develop the most effective bulk hauling and storage solution for hydrogen in terms of cost, safety, weight, and volumetric efficiency. This will be done by developing and manufacturing a tank and corresponding ISO frame that can be used for the storage of hydrogen in a stationary or hauling application. The objective for the first year of this program (2009) was to design and qualify a 3,600 psi tank and ISO frame that will hold 510,000 in^3 (approximately 8,500 L) water volume. The objectives for the second year of this program (2010) will be to perform trade studies for a 5,000 psi vessel and. based on the results of the trade studies, move forward on the design, manufacture, and the qualification of a 5,000 psi vessel/system.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The ability to reduce the cost of delivered gaseous hydrogen is critical to the Hydrogen Program.
- Good progress has been made to date toward alignment with program objectives.
- This project includes the development of a workable truck delivery system which is likely to be necessary for initial hydrogen fuel cell vehicle delivery.
- The presentation indicates that this technology will never achieve DOE volumetric capacity target. The target probably needs to be adjusted.
- The design, development, and construction of a bulk hauling and storage solution is the general objective for this project. While the eventual utility of a bulk hauling/storage container is understood, it is not clear that this topic really requires in-depth research or is really a technological hurdle. Additionally, the benefit of demonstrating this technology (i.e., through a prototype) as opposed to a simple stop at the analysis step is unclear.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- This project has established a good approach with task steps and identification of the most cost-effective increase of pressure (5,000 psi).
- This approach is effective towards meeting the barriers defined in the project objective.
- The approach seems sound for developing a tank trailer and appears to include all necessary tests; however, the “Approach” slide discusses a trade study where it really meets an additional DOE target. The “Approach” slide also mentions that "other methods to increase capacity will be researched," although no other methods were discussed. Cooling research does not seem appropriate because of the nature of the composite.
- Under task 6 (on the “Approach” slide), the project investigator states "other methods to increase capacity will be researched." It is unclear to me what other methods might be applicable. The composites will not be able to

hold cooled or liquid hydrogen. A sorption or metal hydride system may not be appropriate for the scope of this project.
- As the approach was presented, some inefficiencies surfaced in the approach. In particular, the cost reduction studies seem like they should come prior to the development and qualification of various pressure tanks. That is, the anticipated performance and cost tradeoffs should be understood at the outset of the project, enabling one pressure technology to be up-selected. A great deal of progress was made for demonstrating hardware (i.e., container and tanks), establishing manufacturing capabilities, and completing qualification (e.g., various mandated tests).
- The approach should include additional modeling and failure mode assessment tools to provide the appropriate level of reliability.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The project achieved a tube trailer capacity of 600 kg, which is approximately double that of existing technology.
- Technical targets are being met. The safety factor does not appear to be correct for people.
- All necessary qualification tests were completed for the 3,600 psi vessels this past year. Likewise, ISO container tests were completed.
- The planned trade study appears to have only been completed in a trivial manner, and it is unclear whether 5,000 psi is the correct pressure for the next vessel.
- Based on the current project objectives, a great deal of progress was made on demonstrating hardware (e.g., container and tanks), establishing manufacturing capabilities, and completing qualification (e.g., various mandated tests). It is unclear that such demonstrations are truly needed for all of these various pressures. In other words, the reviewer is not sure why both 3,600 and 5,000 psi were targeted instead of simply trying to demonstrate the more challenging technology.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- No collaboration occurred in this project.
- No partners for the project were identified.
- There appears to be private collaborations regarding fittings and other equipment; however, the presentation did not make that clear.
- No collaborations were evident in this project.
- There was no collaboration on this project.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The team proposes an increase to 5,000 and 8,300 psi that will meet future DOE targets.
- The project is focused on the pressure cycling of liquid natural gas, not hydrogen.
- The criteria for the trade study are unclear and should have been discussed more clearly. It could be on tank capital or a full delivery cost; it is unclear which one will lead to the best selection.
- The completion of tradeoff studies (i.e., between cost and performance) are good, but they should have been done much earlier. It is not clear what the technological hurdle is in demonstrating a 5,000 psi tank after the 3,600 psi tank has been accomplished.
- This project seems like a waste of resources.
- The qualification of higher pressure tanks will be useful

Strengths and weaknesses

Strengths
- The investigator has an excellent background conducting research in this area.
- The team is developing tank-trucks that can be used in the near future, which is needed for the initial delivery and dispensing system.
- The team is competent at designing, manufacturing, and qualifying hydrogen vessel (and container) technology for passenger vehicular and hauling applications.

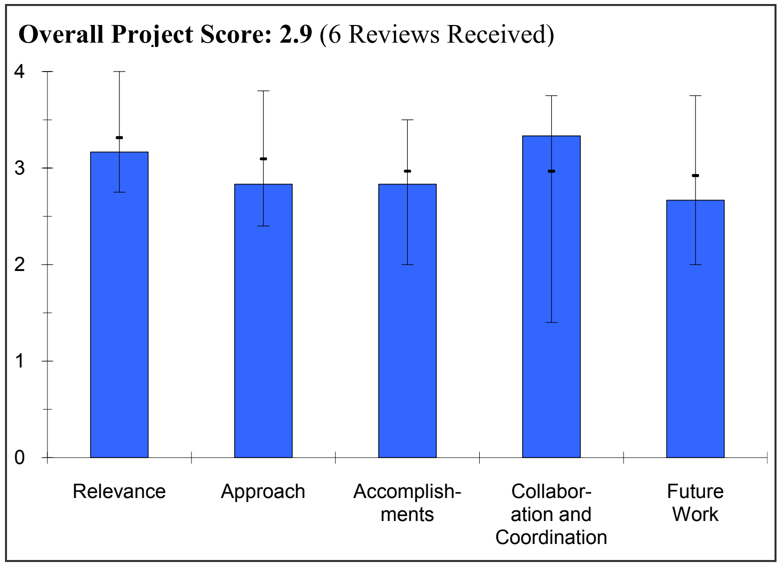
Weaknesses
- The project had a big jump in pressure from current conditions of 3,600 psi to end conditions at 8,300 psi.
- The trade study to select the next tank pressure needs to be better defined.
- Inefficiencies exist in the approach. Cost-performance tradeoffs should be done prior to a demonstration. The project is not very technically deep and no collaborations have occurred.

Specific recommendations and additions or deletions to the work scope

- The team should consider comparing projected cost with that of stationary storage steel tanks used in the forecourt.
- Consider approval from the U.S. Department of Transportation for use as a hydrogen shipping container. A trade study at higher pressure should be completed, and a burst testing should be conducted after higher pressure cycling.
- The trade study to select the next tank pressure needs to be better defined.
- The assessment of vibration and mechanical loading on the cylinder neck should be included.
- Failure modes and effects analysis, and other robustness tools should be in the scope and their outcome provided as part of the project.

Project # PD-22: Fiber Reinforced Composite Pipelines
Thad Adams; Savannah River National Laboratory

Brief Summary of Project



The overall project scope is focused on the evaluation of fiber-reinforced composite piping (FRP) for hydrogen service applications, assessment of the structural integrity of the FRP piping, and development of a life management methodology. Challenges include 1) reduced installation costs for FRP that offer the potential to meet the long-range (2017) cost targets for installed hydrogen delivery pipeline, 2) development of a suite of standardized tests for assessment of hydrogen compatibility of FRP, and 3) development of a structural integrity/life management methodology similar to ASME code B31.8S. The project scope for the past year has been to complete leak testing of commercial FRP joining technologies and initiate life management methodology development.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project can potentially impact several aspects of the program by developing performance-based standards for FRPs and other similar pressure vessels.
- Pipelines are projected to be the low-cost option for hydrogen delivery, and non-metallic materials could offer the lowest installation cost.
- The success of this project would support the DOE objectives for the Hydrogen Program by assisting in the development of hydrogen delivery technologies.
- Fiber-reinforced pipeline can be a low-cost pathway to hydrogen distribution, so it is an essential element in the portfolio of approaches for hydrogen delivery.
- Reliability issues relating to leakage barriers are not realistic due to the cost of lost revenue.
- Piping costs for hydrogen delivery are very high and are a significant factor in delivery. An evaluation and consideration of FRP may offer lower cost piping.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- A generally good technical approach is used for this project, though it could become even more effective if the experimental approach is consolidated. Specifically, it is unclear whether the accelerated environmental tests are more effective than using actual samples. Optimally, both are needed to calibrate the work.
- A good approach is used for preparing the design and life management for FRPs for hydrogen service.
- The research approach for this project is logical and well thought out. The approach addresses the technical barriers. However, the actual progress did not meet what was proposed.
- The team uses a good approach to develop a life management plan to identify operations and maintenance issues and costs in addition to initial costs.
- Working directly with the American Society of Mechanical Engineers (ASME) is the right approach to establish codes and standards for FRP.
- The team incorporates a good background review of codes process, existing standards, and approaches.

- Using performance-based standards is the right objective.
- It appears that FRP will meet program objectives. Once long-term tests are completed, the team may need to perform a burst test to determine safety margins.
- The approach is good for evaluating vendors offerings, but the joints of FRP have not been addressed in this talk.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Considering the resources available, the project has shown good progress. The data presentation can be further improved in the supporting documents by providing more details on the methodology and why the technique is used.
- The reviewer questions whether future work on the joints will be conducted.
- The FRP pipe management plan has been completed.
- The fundamental work done for this project is a good basis. However, not enough progress appears to have been made in many areas. Some of these deficiencies include no testing for leakage via permeability, little or no consideration given to different piping materials, and a lack of applied testing.
- Solid progress has been made on establishing pH bounds and stress/strain characterization of glass fibers. Progress has also been made on other environmental factors, but a time factor is needed to assess durability for 25 years.
- Good characterization was completed of flaw impacts on different reinforcement designs. Again, a life of 25 years needs to be put into perspective.
- The project appears to have achieved progress towards meeting the program objectives in its flaw tolerance work with 40% flaw through fiber reinforcement layer.
- Technical analyses of FRP for hydrogen service are good, but it would be beneficial to show the effects of joints and couplings included in the program.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The project has a good mix of participation by national laboratories, industry, and ASME.
- Adding AGY to the project team brings fiber experience.
- The collaborations with manufacturers and ASME are excellent. The close work done with ASME is especially well-coordinated and beneficial.
- Collaboration with ASME is important.
- The key to switching to performance-based standards is integration into codes and standards.
- Collaboration with commercial partners and industry standards groups appears to be keeping this work on course. The team needs to combine environmental effects with flaw work. The development of modeling appears to be for FRP materials.
- The project demonstrated good collaboration with piping vendors and other national laboratory and university resources.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Future plans are not clear, with exception to the ASME review.
- There does not appear to be any future work proposed because the project is not proceeding.
- A good list is provided for implementing work into standards as an objective.
- Service degradation of material needs to be investigated.
- Future work was indicated, but effects of cyclic pressure were not included.

Strengths and weaknesses

Strengths
- This project has developed strong collaborations with manufacturers and ASME.
- The team uses a practical approach to solving the problem with current codes and standards with respect to FRP.
- Work completed to date supports the current findings.
- The fundamental approach for evaluating FRP as hydrogen piping should yield new insight for use of FRP substitute for hydrogen service.

Weaknesses
- Many unanswered questions remain for a project that has been in process for over four years .
- The team needs to include the cost of FRP as an estimate for determining commercial potential as compared to steel.
- The team needs to work closer with the construction industry to study the effects of trenchless installation. The team should also study the effects of temperature changes due to reduction of pressure in addition to the overall life of FRP.
- The team did not include cyclic pressure effects and joints.

Specific recommendations and additions or deletions to the work scope

- If not considered previously, it may be worthwhile to expand the project scope to include stationary storage vessels.
- It is unclear whether permeability of the FRP will be measured and if it is part of the less than 0.5% leak rate. The reviewer is unsure whether different composite pipe materials are being considered instead of just one type. Information on how/if the material properties change when exposed to hydrogen for long periods would be useful.
- The team should include the element of cost tracking/targets into the standards development process as a way to compare with the technology with steel pipelines.
- The team should define the comprehensive test metrics for future standards development and modification for FRPs with 25 years of durability timeframe in mind.
- The team should do modeling of failure mode at higher operating pressures and emergency venting of hydrogen.

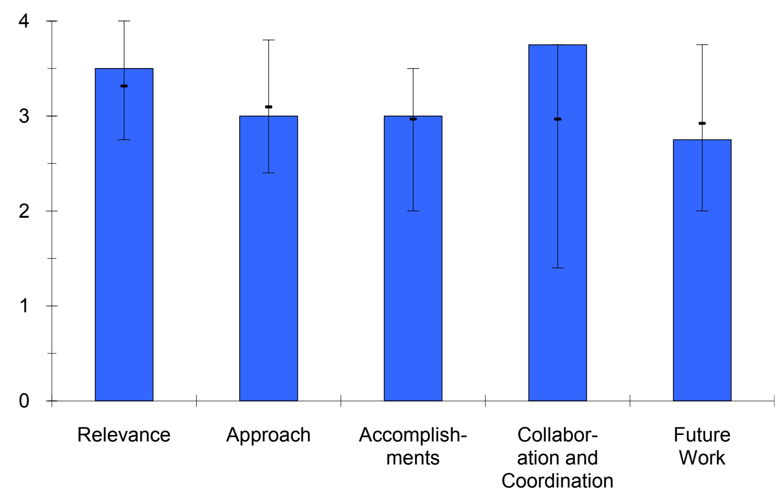
Project # PD-23: A Combined Materials Science/Mechanics Approach to the Study of Hydrogen Embrittlement of Pipeline Steels

Petros Sofronis; University of Illinois

Brief Summary of Project

The overall project objective is to come up with a mechanistic understanding of hydrogen embrittlement in pipeline steels in order to devise fracture criteria for safe and reliable pipeline operation under hydrogen pressures of at least 15 MPa and loading conditions, both static and cyclic, due to in-line compressors. Specific project objectives are to 1) study existing natural-gas network of pipeline steels or hydrogen pipelines, and 2) work with Oregon Steel Mills to propose steel microstructures with superior tolerance to hydrogen.

Overall Project Score: 3.2 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Understanding of hydrogen embrittlement is critical for safe use of hydrogen piping and is aligned with DOE objectives.
- Hydrogen embrittlement is one of the fears of failure mode for hydrogen pipelines and storage.
- The team still needs to define why this work is so important, given the miles of hydrogen pipelines currently in place.
- This topic may be more relevant to DOE's Office of Basic Energy Sciences.
- The project is 75% complete. The following barriers have been identified: high capital cost, understanding of mechanical hydrogen embrittlement, and how embrittlement relates as a function of time and microstructure.
- Steel pipelines are likely to be a key component of a hydrogen distribution network at high penetration. Safety will also be critical.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Developing an understanding of hydrogen embrittlement in pipelines will allow for safe operation with the cyclic pressures expected in future pipes.
- The team needs to establish a good connection between model and experimentation for validation of the theoretical approach. Carefully constructed experiments are good for validation, but they may not represent the real world situation and makes the model irrelevant for practical situations.
- The team needs to first define the problem in terms of exact range for cyclic pressure, frequency, and type of application for failure modes in actual pipelines due to hydrogen embrittlement.
- The presentation was a little too technical without actually bounding the problem and relating to actual failures in the field.
- Use of scanning electron microscope (SEM) to analyze the particles in the microstructure and characterize them is an excellent method to understand the issues.
- The approach appears to have a good mix of experimental and theoretical approaches.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Quasi-cleavage mechanism identification will allow improvement in the model.
- Development of theory is a good idea. The team needs to fully validate the theory with the field application and failures.
- Not much progress has been made in determining actual failure rates and recommending modifications to existing materials.
- To set the bounds on testing, the team needs to establish the magnitude of the problem of hydrogen embrittlement in existing or projected pipelines. This data on this issue was not shared.
- Excellent progress has been made toward understanding suitable steels for construction of pipelines to transport hydrogen. The work to determine if existing natural gas pipelines could, in fact, transport hydrogen is off track. The investigator is only looking at the base pipe materials and not the welds or other higher strength regions within the pipeline system.
- The project's efforts on quasi-cleavage appear to be a significant advance on technology.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- The team has an excellent mix of industry, university, national laboratories, and international cooperation.
- A good list of collaborators is provided, including the Pipeline Working Group.
- Participation in ASME is a must to get the improvements included in codes and standards.
- Seven industry partners, two national laboratories, and two codes and standards developing organizations are collaborating in this project.
- A good mix of industrial and national laboratory partners appear to be actively involved and engaged in this project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Experiments under cyclic pressure will provide needed knowledge regarding fatigue.
- The list of proposed future work is adequate to achieve the goals of the project.
- A focus on fracture testing is needed along with modeling of the fracture process.
- The team sets a good direction in its extension of modeling efforts to non-steady state and incorporation of new experimentally-observed effects.
- The proposed fatigue studies is a reasonable extension of the current work.

Strengths and weaknesses

Strengths

- This project provides the basic knowledge needed to understand the effect of embrittlement on future hydrogen infrastructure.
- The team's technical capabilities and theoretical competence are strong.
- The "go/no-go" decision approach for subcritical crack growth experiments is using sound engineering practices. This should assist with the development of a thermodynamic theory of de-cohesion with the use of ab-initio calculations.

Weaknesses

- Slow progress has been made on the actual determination of failure modes, failure rates, and the dependence on various operating conditions.
- Too much theory is presented without defining the real world problem and magnitude of the issues.

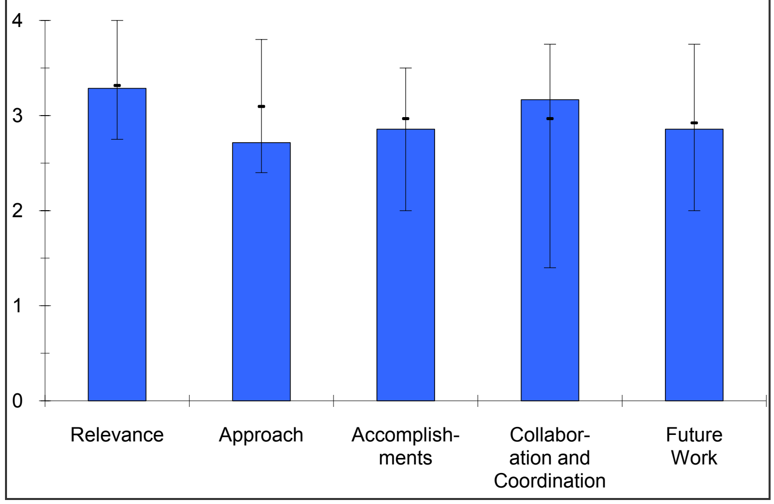
- Sawtooth marking at the top of SEM view is important on how the fracture and crack tip develop. However, this was not well expressed in the presentations or slides, and the reviewer is not sure why.

Specific recommendations and additions or deletions to the work scope

- The team should define an objective in terms of modification of codes and standards.
- The goal of the project should not be to find hydrogen embrittlement. Instead, it should be to define the relevance of embrittlement in the real world situation.
- The team should demonstrate intense dislocation activity for quasi-cleavage fracture. A single-edge notch tension test can be used as a comparison. The team needs to look at fatigue specimens and the characterization of the specimens to show the same mechanics as hydrogen embrittlement.

Project # PD-24: Composite Technology for Hydrogen Pipelines
Barton Smith; Oak Ridge National Laboratory

Overall Project Score: 2.9 (7 Reviews Received)



Brief Summary of Project

The objectives of this project are to 1) assess, primarily from a materials performance perspective, the compatibility of FRP and engineered plastics in high-pressure hydrogen environments, 2) to define research and development issues for adapting the technology for hydrogen use, and 3) to develop a path to commercialization for the technology. Milestones for 2010 are to 1) conduct hydrogen compatibility evaluations of the next group of composite pipeline materials and construction, 2) perform cyclic testing, and 3) complete the next round of polymer diffusivity and permeability measurements.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The main question confronting this project is whether any one of the composite technologies is appropriate for use in hydrogen pipeline and vessels.
- Non-ferrous piping may provide the lowest cost installation for hydrogen pipelines.
- The presentation material demonstrated that the project is responsive and clearly attuned to DOE objectives.
- Delivery costs dominate the current hydrogen fuel costs.
- Pipeline is the leading future commercial approach to delivery of hydrogen. Current pipeline costs are still too expensive, so this project is very relevant.
- Composite pipelines for hydrogen have the potential to reduce the cost of transporting hydrogen over long distances. The capital cost of composite pipelines is expected to be significantly lower than steel pipelines.
- This project is using existing technology solutions from other industries (e.g., oil and gas) for potential transportation of hydrogen. This project is looking to address long-term gaps that relate to the economics of hydrogen distribution. Over the past couple of years, the project made significant progress; however, it appears to have slowed or stopped over the past 6-9 months.
- Finding low-cost transmission and distribution piping for hydrogen is a necessary requirement for long-term hydrogen infrastructure.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The authors do not specifically address the approach, but it is evident that they are engaged in a series of empirical tests of various available composite technologies. However, it is unclear if the tests are actually addressing the "go/no-go" decision. For example, the reviewer is interested to know how the team plans to test or address the 25-30 year lifelong reliability of the material.
- The project approach is not covered in the presentation.
- The project is considerably outside of the reviewer's area of technical expertise, so the reviewer is deferring this question to more qualified reviewers. The approach appeared to be reasonable for addressing a core problem in hydrogen distribution, but this remains a difficult problem that will require substantial effort to resolve.
- A good approach was used to leverage existing oil and gas industry experience in composite pipelines.

- The project used a good list of composite pipeline manufacturers.
- The team used a good screening approach to identify issues and leading candidates.
- The overarching approach to the project was not communicated in the poster. No deficiencies in the testing approach were identified. A discussion of "go/no-go" decision points and project milestones would be helpful to better evaluate the approach.
- The original approach was to identify major barriers, which includes the material composition of the pipe. However, the team now needs to refocus and look at secondary issues with using these composite designs. The research team needs to consider the connector materials and other devices needed within the pipeline system and the effects of long-term exposure to hydrogen.
- Generally, the team presented a good approach and good collaboration with FRP pipe vendors. However, some testing should be done early on to examine the viability of pipe connections. This is almost always the weak point in piping systems.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- The limited resources provided in 2010 may have affected technical accomplishments and progress toward the project and DOE goals.
- The loss of tensile strength is an important issue that is not adequately addressed. The authors attribute this to shipping issues, but this is an inadequate analysis and it raises issues regarding the vulnerability of the fibers to defects and other external damages. The team should consider that this may not be a robust technology to be used in an uncontrolled environment.
- Delamination during the blow-down test highlights a potential weakness for lined pipe in cyclic pressure service.
- The project is considerably outside of the reviewer's area of technical expertise, so more qualified reviewers should be deferred to in assessing the technical accomplishments of this project.
- Overall, good progress has been made on the screening of leading composite pipeline concepts.
- The projected cost status is quite favorable in terms of costs.
- DOE needs to set proper metrics for hydrogen leak because less than 0.5% is not specific enough.
- The project has demonstrated that the composite pipelines perform better than predicted.
- The projected cost of the composite pipe materials appears to meet the estimates. However, a large-scale demonstration should be completed to look at the real world capability of the system transporting hydrogen. Test data on pressure cycling and variations in operating temperature is needed to verify the long-term reliability to the pipe system.
- In general, good progress has been made on evaluating Polyflow Thermoflex. The hydrogen leak rate looks good, although it was not done with fittings and couplings. Delamination of the inner sleeve is a problem, and the research and development team needs to define what constitutes a piping failure in specific terms.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- This project presents good coupling of a national laboratory with industry.
- Based on the presentation, a strong degree of collaboration appeared to be present for this project.
- A fair list of collaborators and pipeline manufacturers was presented.
- Collaborations appear to be adequate and appropriate, though proposed future work may require additional collaborators.
- The project investigator is working with six different pipeline system teams as they develop different compositions of composite pipe. Review of the operational conditions found in the ditch (e.g., operating pressure cycles, temperatures, and product loss due to line leakage) by existing hydrogen pipeline operators is suggested. This will provide insight into the testing conditions to which the new composite pipes would be subjected. Also, the projected loss due to leakage was estimated at 0.5%, but based on the cost of water, that is not much revenue. However, given the cost to produce hydrogen, that becomes a much larger dollar value

considering the total volume of hydrogen projected to be stored in that composite line. The team may need to rethink this.
- Great collaboration is taking place with piping vendors.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- Development of codes and standards is needed for hydrogen FRP service required for safe implementation.
- The proposed work seems like reasonable next steps to build upon the previous results.
- The proposed future work to finish the screening work is fair.
- The team needs to identify the next steps to successfully establish commercial readiness of this approach.
- The proposed future work builds on past progress and is focused on the barriers. However, decision points and barriers to realization of composite pipelines in hydrogen operation were not discussed.
- No real proposed future work was suggested by the presenter. The presenter was asked about reviewing currently operating hydrogen pipelines regarding pressure and temperature along with the connectors and system gages under consideration. The presenter agreed with this idea; however, funding limitations would hinder that work. The presenter did propose a real world demonstration test of recommended designs.
- Good plans were presented for 2010-2011, but the team needs to think about testing with couplings and defining criteria for piping failure besides leak rate.

Strengths and weaknesses

Strengths
- The team used a good and balanced approach of utilizing existing oil and gas industry experience in composite pipelines.
- The project has developed useful data for evaluating the status and potential of composite pipelines for hydrogen. Relevant materials and information for cost modeling are provided by project collaborators.
- The cost of projected composite pipe appears to have been met.
- The project addresses the need for cost reduction in transmission and distribution piping for hydrogen.

Weaknesses
- The scope is a little too narrow and should be expanded to something like "establish commercial readiness of composite pipelines for transportation hydrogen application."
- No significant weaknesses were identified.
- The team needs to look at long-term pressure and temperature cycling of pipe. They also need to investigate connectors and well health monitoring of system tools. The project investigator looked at the leakage rate of system and not the cost of lost product, which could be a much larger number than the projected 0.5% loss of product over a given length of pipe.
- The team should consider retesting the composite pipe at higher operating pressure and temperature along with the cycling of pressure and the effects on the long-term life reliability of the composite pipe.
- The team needs to define criteria for piping failure and include piping junctions and couplings in durability testing.

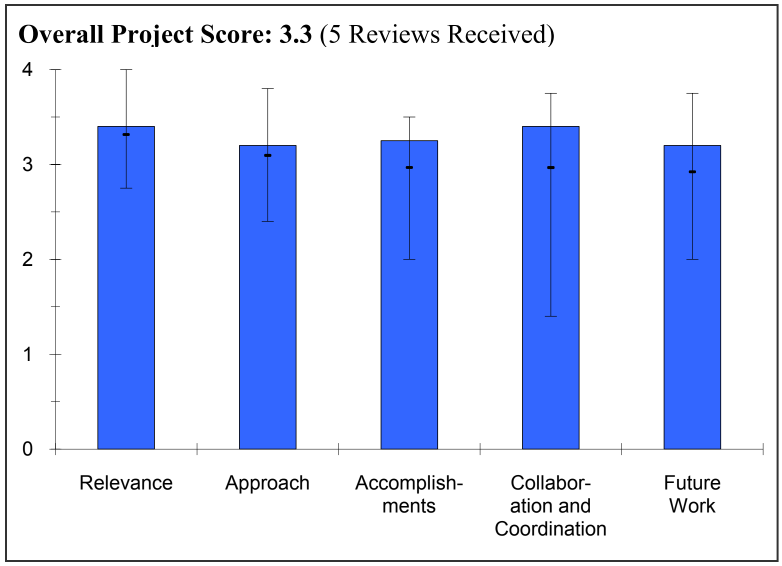
Specific recommendations and additions or deletions to the work scope

- The program needs to address the “go/no-go” decision promptly.
- The team should expand the scope to include the next step of establishing commercial readiness of the leading candidate from screening exercise.
- Testing of the composite materials under pressure cycling is an important next step in evaluating the potential for application in hydrogen delivery pipelines.
- The team should interface with existing hydrogen pipeline operators to better establish pressure cycling rates as well as operational conditions for temperature.

Project # PD-25: Hydrogen Embrittlement of Structural Steels
Brian Somerday; Sandia National Laboratories

Brief Summary of Project

The objectives of this project are to 1) demonstrate reliability and integrity of steel hydrogen pipelines for cyclic pressure by addressing potential fatigue crack growth aided by hydrogen embrittlement, and 2) enable pipeline design that accommodates hydrogen embrittlement by applying and optimizing hydrogen pipeline design code ASME B31.12 with an emphasis in FY 09-10 on measuring fracture thresholds and fatigue crack growth laws for X52 steel in hydrogen gas. The reasons for steel hydrogen pipelines are that the safety of steel pipelines is well established (e.g., third-party damage tolerance) and that hydrogen pipelines are safely operated under static pressure.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The embrittlement is an important issue, and while it is not in any of the critical pathways, considering the amount of resources, this seems to be an appropriate level of work.
- Pipelines are the expected low-cost delivery method. The project increases understanding of hydrogen embrittlement required for the application of steel pipe to a potential hydrogen infrastructure.
- There will likely be a need for steel pipelines to transport hydrogen. Therefore, research to understand the failure mechanisms for steel pipelines in hydrogen operation is warranted.
- The project is 40% complete.
- This project focuses on ASME code 831.12 and is attempting to address the static pressure of hydrogen and how the reduction of cyclic pressure will improve the integrity of the system.
- This project is highly relevant for the success of hydrogen infrastructure. The project goals support DOE research, development, and demonstration goals where the specific intent is to demonstrate the integrity and reliability of steel hydrogen pipelines for cyclic pressure, which is resistant to embrittlement.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Improved test methods are required to develop inputs to the ASME pipeline code.
- The testing follows standard ASME B31.12, supporting pipeline design, codes, standards and permitting efforts. The approach used in this project appears to be sufficient to provide data to support and optimize the test methods referenced in standard ASME B31.12, and to better understand fatigue-related failure mechanisms of steel pipelines in hydrogen operation. Testing at different frequencies has led to test method improvements and reduced testing times.
- The project is focused on demonstrating the improved reliability of steel pipelines transporting hydrogen by reduction of cyclic pressure.
- The approach was rational toward accomplishing goals, in particular using the specified ASME code to evaluate the fracture properties of various materials.

- Milestones were clearly laid out and results were to be provided to key standard development organizations. The team also presented a secondary goal of trying to reformulate relevant standards in order to improve testing efficiency.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- In general, good progress was made on the project as compared to last year.
- The project determined properties for X52, which was identified as a potential steel of interest.
- The project has developed data and test methods that substantially contribute to the assessment of fatigue crack growth laws and pressure cycling effects on steel pipelines in hydrogen operation.
- The team made excellent progress towards showing that the embrittlement barrier can be overcome by operating in a static range of operating pressure.
- It was great to see a round-robin approach for evaluation of consistency amongst groups.
- Determination of fatigue crack growth laws was useful and highlighted the time required by current ASME code. However, the overall group should shift focus to working towards their technical goals rather than making revisions to the ASME code.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- There is a good level of collaboration among various groups. The project would benefit if it could also tie
- in with the validation project and acquire samples of some of the steel storage cylinders.
- This is excellent work with the DOE Pipeline Working Group and builds on use of round-robin data.
- Interactions with the DOE Pipeline Working Group and standards development organizations are appropriate.
- The project has collaboration with industry, universities and two national laboratories.
- The project has solid participation with laboratories and universities via the Pipeline Working Group and is clearly in communication with the ASME organization.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- This was good work on planning the development of additional data on selected X52 steel.
- Testing of the fatigue growth rates of welds is an appropriate next step. Future testing should be focused toward understanding the effects of pressure cycling on steel pipelines in hydrogen operation.
- Plans are clear for the scope of work and the overall program objectives. There is a need to focus more on newer high-strength pipeline steels that are common today and not what was common 40 years ago. Focus on X-70 thru X-100 type steels.

Strengths and weaknesses

Strengths

- The collaborations are a strength in this project. Collaborations with the National Institute of Standards and Technology, component manufacturers, and ASME are particularly important for ensuring that sufficient and relevant data is collected to support test methods and standards, as well as to understand fatigue crack growth laws of steel pipelines in high-pressure hydrogen operation. Reviewer comments from the previous year appear to have been adequately addressed.
- The project developed a sound understanding for material effects of hydrogen toward the long-term safety and integrity of steels.

Weaknesses

- The amount of reliable data that can be obtained in this project is limited because there is only one test apparatus.
- The proposed future work is to develop test methods for measuring the fracture properties of pipeline steel girth welds in hydrogen. This work has already been completed and is in use by current pipeline operators of natural gas and hazardous liquids for the Department of Transportation Pipeline Safety Administration.

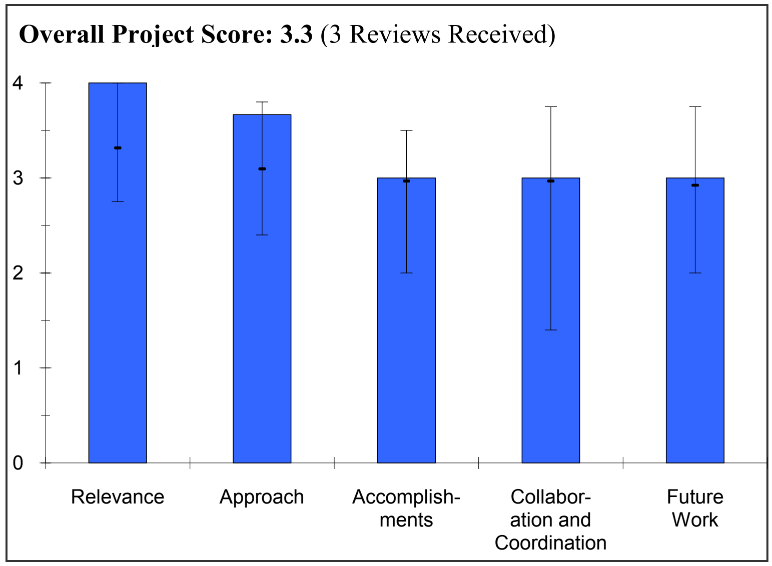
Specific recommendations and additions or deletions to the work scope

- The project should focus on the pressure cycling effects on steel pipelines in hydrogen operation. To the extent possible, determine the range of operating conditions likely to be experienced by steel pipelines in hydrogen operation, and verify that the testing parameters used are sufficient to understand the hydrogen embrittlement of these pipelines. The project should continue to generate data to determine how conservative the measurements at high frequency are and to optimize the balance between test efficiency and data reliability.
- The project investigator needs to coordinate his research and development efforts with members of the oil and gas pipeline industry.

Project # PD-26: Innovative Hydrogen Liquefaction Cycle
Martin Shimko; Gas Equipment Engineering Corp.

Brief Summary of Project

The objectives of this project are to 1) design a practical hydrogen liquefaction cycle that significantly increase efficiencies over existing technologies; 2) identify, design, and test the key component (i.e., continuous catalytic heat exchanger); 3) design a 50,000 kg/day plant using low/no risk development components; and 4) document a significant reduction in the total cost of hydrogen liquefaction at the 50,000 kg/day production level.



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- There is a good focus on making use of off-the-shelf components, which will help to build a reliable unit with a track record for available components used.
- If efficiency improvements are realized they would provide very important benefits to the near and mid-term hydrogen infrastructure.
- This technology is clearly relevant to establishing viable hydrogen infrastructures.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- This is a logical approach that transitions from modeling phase to testing.
- A very good approach, with reasonable progression from the modeling through the prototype testing stages.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- It is good to see that the prototype is in the process of being built.
- The reviewer is concerned that reliance on graduate students may cause potential delays.
- It appears that technical barriers have been addressed and should be overcome.
- Technical barriers are being adequately addressed, and good progress is being made.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The project investigator should connect with Argonne National Laboratory (Marianne Mintz) to verify the assumptions used for the DOE H2A model for liquid hydrogen and to verify that the system equipment costs are definitely approximately 40% of the H2A estimate.
- The work with partners was not outlined sufficiently. The team needs to work with hydrogen industry representatives.

- Seems to be working well with partners. Some additional attention to working with partners more closely in validating assumptions in H2A models could be of benefit.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The project should include work on the full-scale compressor.
- It was unclear if it will be tested on full scale with hydrogen.
- Future work plans seem adequate except for full scale testing, which is not included unless more funding is provided. Fiscal year 2011 work was not outlined.
- There is a logical progression in the work to date, and in the proposed future work. Full scale testing of this technology would be very interesting.

Strengths and weaknesses

Strengths

- It is valuable to use existing developed technologies (such as helium expanders) to bring the cost down and increase the reliability of the final product.
- The project was excellent to show progress in efficiency increases through comparison with current large-scale hydrogen liquefaction plants.
- There is a potential to use existing technology to dramatically reduce the energy for liquefaction.
- An interesting and novel approach to H2 liquefaction with potentially significant benefits to large scale hydrogen infrastructures.

Weaknesses

- The use of graduate students could cause delays in meeting the timeline for the final production full-scale unit.
- Continuous catalytic heat exchangers are a weakness, as the testing is not completed yet.
- The lack of an equipment test, which is scheduled, is a weakness. The project has no hydrogen providers on board.

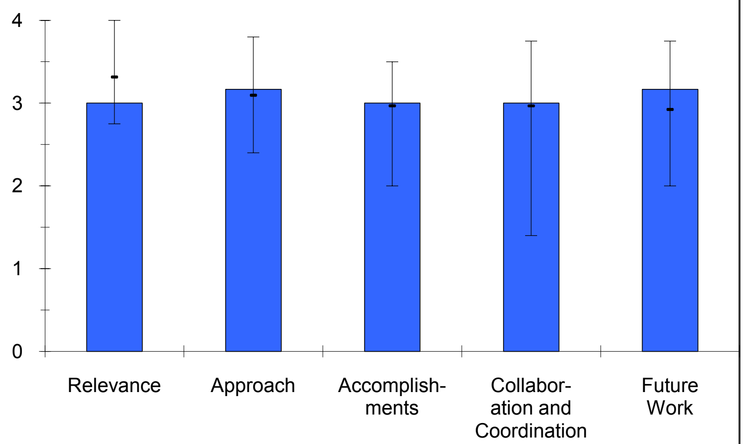
Specific recommendations and additions or deletions to the work scope

- The project should include numbers for both the capital cost of the system and the resulting liquid hydrogen cost/kg.

Project # PD-27: Solar High-Temperature Water Splitting Cycle with Quantum Boost
Robin Taylor; SAIC/FSEC

Brief Summary of Project

Overall Project Score: 3.1 (6 Reviews Received)



The overall objective of this project is to demonstrate the viability of a new and improved sulfur family thermochemical water-splitting cycle (i.e., sulfur-ammonia) for large-scale hydrogen production using solar energy. Project goals are to 1) evaluate sulfur-ammonia water-splitting cycles that employ photocatalytic or electrolytic hydrogen evolution steps and perform lab testing to demonstrate feasibility of the chemistry, 2) perform economic analyses of sulfur-ammonia cycles as they evolve, 3) select a cycle that has high potential for meeting the DOE 2017 cost target of $3/kg hydrogen and efficiency goal of more than 35%, 4) demonstrate technical feasibility of the selected sulfur-ammonia cycle in bench-scale, closed-loop tests, and 5) demonstrate pre-commercial feasibility by testing and evaluation of a fully-integrated pilot-scale closed-cycle solar hydrogen production.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- It is hard to picture who the customer would be for this technology. Presumably the customer is someone who has a lot of desert land for the heliostat, electricity available at reasonable cost, a way to get the hydrogen to customers, and would not have to worry much about safety and pollution. There are few customers like this.
- Thermochemical cycles remain one of the most promising approaches for large-scale hydrogen production by water splitting that could meet the DOE targets for hydrogen cost and efficiency.
- This project supports the DOE research, development, and demonstration objectives.
- This is definitely in line with the DOE goals and objectives.
- The project seeks to develop a water splitting process via the use of thermochemical cycles where most, if not all. of the energy comes from a solar thermal source.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The researchers are addressing issues, but more seem to pop up as the others are addressed.
- The evaluation of the sulfur-ammonia cycle is a good complement to the other thermochemical cycle programs in production and delivery. This project has an effective plan to evaluate and optimize the entire cycle.
- The team has changed their approach from last year and is now focusing on the more critical issues.
- The chemical cycle they have selected is much more complex than other thermochemical cycles. The complexity will substantially increase the capital and operations and maintenance costs, which is not reflected in their analysis. It is unclear that they will be able to lower the costs in reality.
- They do not include durability testing for any of the reactors in their approach. This is a very aggressive system and reactor life will be an issue. This is particularly important for the electrolysis stack since their approach to decrease the voltage is to raise temperature, which always results in shorter stack life.
- The project seems particularly well organized and presented.

- The project appears to be working on the correct technical barriers. However, start/stop cycling needs to be addressed in much more detail.
- The investigators have shown considerable creativity in changing their approach in response to emerging issues, -such as in the use of only a thermal solar input and in eliminating the zinc oxide/sulfate based cycle.
- The overall approach seems to take for granted the catalytic high temperature sulfur trioxide to sulfur dioxide + oxygen step. In a sense it is reasonable since this unit operation is being developed by others, but there should be at least a minimal engineering design for its integration into the overall process.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The main achievement seems to have been to get the electrolysis voltage down to 1.0 V from 1.6 V using a heliostat and a fairly complicated thermal splitting cycle that operates at temperatures up to 900° C. The reviewer expected better results than this using straight electrolysis. There are encouraging indications that the voltage can be reduced further to 0.6 V, but only for very low rate outputs. Even 0.6 V is not enough of an improvement over existing technology to justify the likely capital and safety costs of the cycle.
- There is good technical progress on improving the various sub-cycles. The modeling and economic analysis data is useful even at this early stage of the technology development.
- The lowering of the electrolytic cell potential is a good accomplishment.
- The H2A analysis underestimates the cost of water. This type of technology will be located in regions with limited water availability and water rights.
- The H2A analysis includes estimates for operation and maintenance. The project investigator should include the team's assumptions in the supplemental section. Their projected operation and maintenance costs seem low compared to the high complexity of the system and the chemicals in use.
- The project investigator is using current results in their H2A analysis, which is very good.
- More information on the oxygen evolution reactions is needed. The rate, kinetics, selectivity and catalyst durability should be described. The team obviously has much of the information (not including the durability) so it should be relatively easy to get this data.
- A hybrid cycle requires electricity that will need to be generated on-site or brought in from the grid. In order to achieve 24/7 operation, which the project investigator indicated was a goal, they will need to be connected to the grid. The assumption in the H2A models that they will be able to provide all of the electricity without purchasing from the grid is flawed since solar power is only available for a short time during each 24-hour cycle.
- The process design has come a long way since the last meeting. Bottoming cycle integration is a great idea and it would be great to see some of the process design details behind it.
- The team made excellent progress towards down-selection to an apparently more viable process cycle, using all liquid/gas systems with an electrochemical step for hydrogen generation. However, significant issues remain, including the low difference in temperature between ammonia and sulfur trioxide evolution, which could result in not only nitric oxide but also nitrogen. Ideally the temperature of ammonia release should be lowered, which may be possible by additives that increase the basicity of the melt. Secondly, the process has to be designed for continuous operation. No chemical plant, especially one operating at such high temperatures, can be efficiently turned on and off twice a day. Storage of the hot molten salt is suggested to address this issue, but keeping the sulfur trioxide to sulfur dioxide + oxygen reactor at process temperature needs to also be addressed.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Given the issues involved, they have chosen the right academic partners.
- The critical thermochemical cycle process development is occurring at a subcontractor facility (Florida Solar Energy Center). It is important that good collaboration continues to maximize the results of the process development.
- The project has a well balanced team of industry and academia.
- The number of publications is good.

- The project includes an excellent collaborative partner on the important electrochemistry step. However, at minimum a partnership with an active developer (possibly Sandia National Laboratories) of the high-temperature sulfur trioxide to sulfur dioxide + oxygen process is critically needed.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The investigators seem to be trying to improve the cycle in all the right ways. The problem with the cycle is that there is so much that needs improvement.
- The future work is well-planned and clearly focused on the key deliverables.
- The FY 11 activities seem reasonable.
- The major gap in the proposed future work would be looking at durability of the reactors, especially the electrolyzer. Lifetime testing of a minimum 1000 hours needs to be done.
- This process really needs to be a 24/7 operation. They need to include the assumptions used for the Aspen® analysis and H2A analysis for this requirement. This information should be included in the supplemental section of the presentation.
- This project is moving in a direction that has a good balance between system optimization and component development. However, more detail behind start/stop cycling as well as any thermal storage considerations would be beneficial. Steam cycles, for example, are very challenging to start/stop. A steam turbine may take most of a day just to start.
- The overall process must be designed for continuous operation beyond just a consideration of using a reservoir of molten salt. Maintaining the sulfur trioxide decomposition reactor at temperature during nighttime could (in theory) be accomplished by reversing the flow of reactants (i.e., sulfur dioxide+1/2 oxygen [air] now going to sulfur trioxide), which is an exothermic reaction. This concept is in the literature.
- A competent partner (possibly Sandia National Laboratories) for the sulfur trioxide decomposition technology should be included in the project.

Strengths and weaknesses

Strengths

- The project is unique and offers an attractive water splitting cycle.
- There is a good balance between the development of the improved thermochemical sub-cycles and the modeling/evaluation of the overall cycle.
- The project has a strong team.
- The team is focusing on the critical areas.
- This was a particularly well-constructed presentation.
- The apparent flexibility and creativity of the investigators in changing process pathways in response to technical and economic issues is a strength.
- The broad-based approach from the solar collector to the chemistry and electrochemistry of the overall process is a strength.

Weaknesses

- The cycle does not seem to work that well. It does not seem likely that this process will work well enough for practical use in the foreseeable future.
- There are still potential showstoppers in the thermochemical sub-cycles (e.g., cross contamination of ammonia and sulfur dioxide generation in the oxygen generation sub-cycle). A clear focus on solving these issues should be a priority.
- The efficiency of this process cannot be high since it can only be in the range of about 22%. The ammonia and sulfur cycle has been well studied for several decades and others have proven that it is a difficult process to optimize to get better efficiency.
- To achieve 24-hour operation in a hybrid cycle the system will need to be connected to the grid and purchase electricity from the grid.
- The chosen cycle is very complex and requires a large number of reactions and reagents. This will add capital cost and increase the operation and maintenance costs.

- The team is increasing the operating temperature to increase the electrolyzer performance. In water electrolyzers, this always results in decreased lifetime, and they are keeping the temperature above 80°C. The team needs to demonstrate that the electrolyzers will have a reasonable operation life at these higher temperatures.
- The diurnal nature of this project means there will be thermal cycling of the reactors and components. This needs to be addressed and is not discussed.
- The complexity of the conversion system is a weakness.
- The cost analysis is poorly described, which is a weakness.
- The project concludes that because electricity is a major contributor to hydrogen cost, voltage should be lowered to improve efficiency. The reviewer suggests doing an optimization on current density, as operation at a low(er) current density may lower electricity cost more than it raises capital cost.
- More H2A assumptions need to be specified, for example labor requirements and basis for capital costs.
- As noted above, the overall process has to be designed for continuous operation. To accomplish this, all of the steps need to be considered, not just the molten salt cycle.
- It would be helpful to know the theoretical minimum voltage required for the electrochemical step.

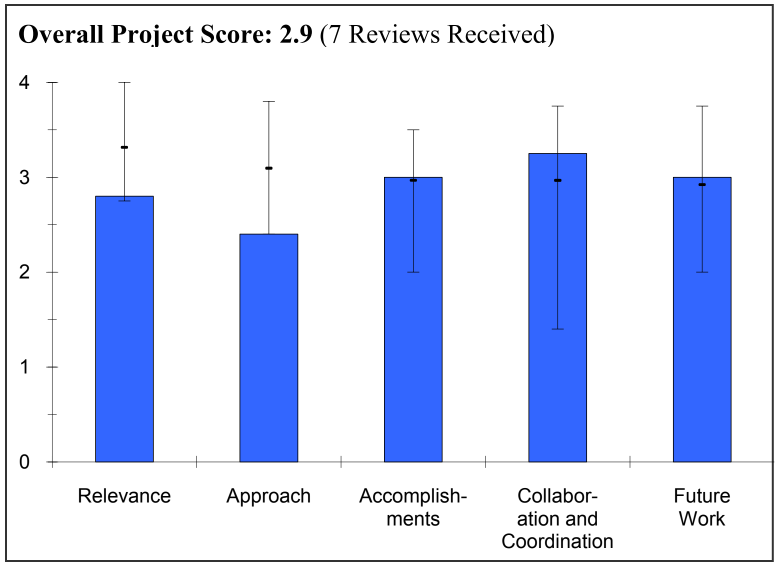
Specific recommendations and additions or deletions to the work scope

- They need to plan on durability testing and thermal cycling.
- It would be beneficial to provide a table showing subsystems, capacity, cost, and maintenance for each major subsystem including the solar field, solar collector, and steam cycle, to name a few.
- More attention should be given to the sulfur trioxide decomposition step, particularly on discerning the need for its integration into the overall process.

Project # PD-28: Solar-Thermal ALD Ferrite-Based Water Splitting Cycles
Al Weimer; University of Colorado

Brief Summary of Project

The objectives of this project are to 1) use the H2A analysis to provide guidance for a conceptual process design that is cost effective and determine feasibility, 2) conceptualize a scalable central solar reactor/receiver per H2A guidance on economics, 3) develop and demonstrate suitable materials for robust thermochemical redox cycling that will integrate easily into the solar reactor design, and 4) develop an overall plan to take the technology to the point of demonstration in five years, providing that market conditions warrant this step.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- While this program targets the key goal of cost of hydrogen, there are basic issues about the project's ability to serve a customer. The hydrogen produced is likely to be impure and at low temperature, and the location of the facility has to be in a desert. It is hard to picture a large customer base lining up to purchase this hydrogen.
- The solar-driven thermochemical water splitting process supports the DOE objectives.
- Low carbon hydrogen is a key component of a hydrogen economy. This technology is competitive against biohydrogen production, photoelectrochemical production, and SMR with carbon capture and storage.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- The group has targeted and demonstrated rapid-cycle hydrogen generation and good (not great) material durability. The high temperature and small temperature difference between the oxidation and reforming step suggests that the overall energy efficiency of the process is low. Similarly, the high temperature of operation suggests that the costs and materials issues will always be significant.
- Oxidizing a reducing iron is likely to have a strong, negative impact on the iron structure. The hydrogen purity and hydrogen pressure are likely to remain low for this process, which limits its applicability.
- The project investigator did a good job addressing many of the weaknesses identified in last year's AMR.
- The approach assumes the ferrite is directly heated by solar power, which limits the operation of the process to the relatively few hours when appropriate sunlight is available. This will also introduce large thermal cycles, which are not addressed. The project investigator only addresses cycles between X and 1500°C.
- The project uses a simple process; there is only one reactor and only one reagent.
- The reagent is non-toxic, unlike all the other chemistries being researched.
- The new design does not lend itself to 24/7 operation. For this approach to be viable, they must be able to operate 24/7.
- The critical path needs to be in demonstrating a long cycle life of the chemicals. The project needs have more focus on this.
- The project this past year has done a very good job focusing on the critical technological issues, for example those identified by last year's review panel.
- The chemical looping approach is well established and an extension to hydrogen production is reasonable.

- High temperatures (1200-1500°C) present a significant challenge for materials and energy storage.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- There are good cycling results on a five minute time scale. The small temperature difference between oxidation and reduction suggests that the hydrogen purity could not be high.
- The team was able to show that atomic layer deposition (ALD) $CoFe_2O_4$ can be cycled.
- It is unclear if the "ocene" cost analysis includes the price of processing to make the thin films. It seems to only include the raw material cost. ALD processing may substantially increase the cost, especially when such a large amount of the material is required.
- The number of cycle tests was not enough to demonstrate if the material is usable. The team needs to show hundreds (if not thousands) of cycles. The cycles should be between room temperature and operating temperature since they are not planning on operating 24/7.
- Operation and maintenance will be very expensive since the system is not operating 24/7. The reviewer would like to see the estimates for operation and maintenance used in the project's H2A analysis.
- The team seems to have made substantial progress towards demonstrating their goals.
- The project demonstrates a good comparison of bulk versus ALDC film performance and of considering the effect of the substrate on cycle reduction temperatures, which can lead to important conclusions.
- Temperature optimization is a significant development.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Given the project, it is hard to imagine better collaboration.
- The team had very good collaborations.
- No evidence of collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The presenter proposed to basically continue doing the current work .
- The project is ending.
- The team needs to show the cycle life.
- The team needs to modify the cycle to achieve 24/7 operation.

Strengths and weaknesses

Strengths
- This project has some interesting science.
- The project uses a simple process with non-toxic materials.
- The project has a strong team.
- The team has done a very good job of directing effort towards rectifying weaknesses identified previously.
- The team has done a very good job of experimental of testing material hydrogen flux rates at various conditions.
- It appears a tremendous amount of work has been conducted on this concept.
- The process appears to offer a promising solar-to-thermal and solar-to-hydrogen conversion efficiency.

Weaknesses
- There is a sense that this project is not likely to produce a practical option for hydrogen generation.
- It is not sufficient to use raw material cost for the ALD "ocenes" for the H2A analysis. The team needs to also include the processing cost to make the thin films.

- The system does not run 24/7; this means that there will be large thermal stresses as the system heats up and cools down each day.
- The team underestimates the operation and maintenance costs for a system operating for only a short time every day.
- The team did not address a key weakness from last year's review; i.e., high utilization of solar flux is critical for this project because the heliostat field is very expensive. The use of solar flux to heat the carrier material must be included in the economics. It is not acceptable for this project to propose high-carrier solids while doing economics on zero-carrier solids. This weakness should be addressed.
- Typically only five cycles are used to determine whether redox cycles are stable. While this is a good screening indicator, actual cycles will need to be over tens of thousands. Thus, we do not really know stability of cycling.
- While it is good of them to include an H2A analysis, no data is presented on capital costs or operating expenses. The team needs to list assumptions and corresponding basis.
- No fabrication costs of the materials seem to be included in the analysis.
- Cycle time is a critical factor for economics. However, there is no testing or assumptions listed to support the postulated five minute cycle time.
- A basic diagram of how the system would operate (in terms of the cycle) is not adequately detailed to lead the reviewer to have confidence that all cost components of the system have been included.
- The Aspen® model is not adequately labeled to make it explanatory or very useful.
- It is unclear whether or not labor is included in the analysis. Cost studies of similar systems reveal that labor can be a substantial cost contributor. Future presentations should list all key H2A assumptions.
- It is unclear how hydrogen and oxygen can be effectively separated in this solar-thermal process. It is also unclear if catalyst deterioration could be just an issue, or a serious problem.
- It is naïve to assume that ferrite is capital and will not be replaced. Material will have finite lifetime.
- The limited number of cycling experiments is a weakness. The process will undergo approximately 100,000 cycles per year. The team should perform 1,000 to 10,000 cycles at a minimum to evaluate ferrite stability.

Specific recommendations and additions or deletions to the work scope

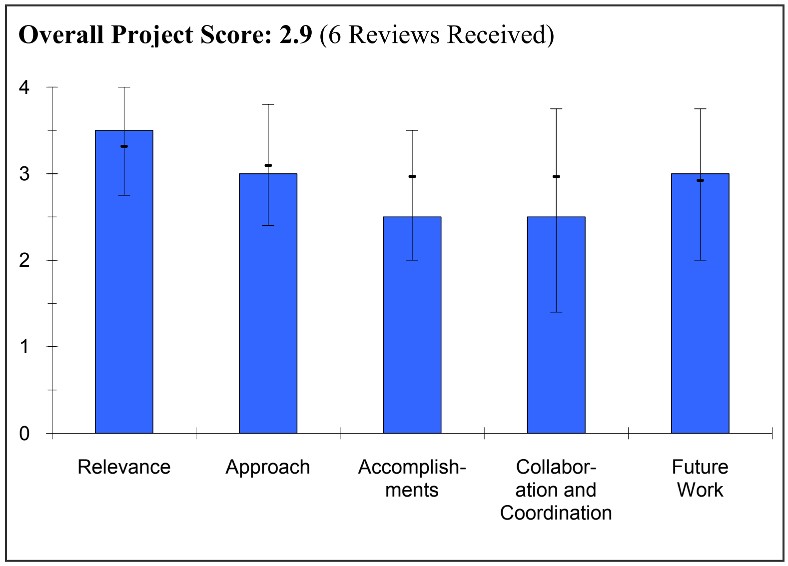
- The project should be continued as a basic science effort since laboratory studies are needed to increase the purity and the overall efficiency. Few, if any, solar tower tests should be done at this stage.
- The above-listed project weaknesses are central to the relevance and success of the effort and they should have been addressed, especially since this project started in 2005.
- Long-term cycling experiments should be added. The economics need to be extended to consider the possibility of gas firing the system at night and during cloudy periods. The produced hydrogen will not be as "green," but

- **Project # PD-29: High-Capacity, High Pressure Electrolysis System with Renewable Power Sources**

Martin Shimko; Avalence LLC

Brief Summary of Project

The electrolyzer development project goals are to 1) achieve at least a 15-fold increase in the gas production rate of a single high pressure production cell, 2) demonstrate the high pressure cell composite wrap that enables significant weight reduction, 3) build and test a 1/10th scale pilot plant, and 4) perform an economic assessment for a full scale plant (300 kg/day, 750 kW) that meets the DOE 2017 cost target of $3.00/gge.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The program did meet the DOE research, development, and demonstration intent in developing a high-pressure electrolysis system that produces high-pressure hydrogen at the cell level without using mechanical compression.
- It is not completely clear what the advantages of this approach are over other types of electrolysis. While noble metals may not be needed, there are many other aspects of the cost that are driving up the capital cost of this technology.
- To achieve the DOE targets for low-cost high-pressure hydrogen production, this project focuses on the development of a unique alkaline electrolyzer cell design with the goal of generating hydrogen at a pressure of 6500 psi, thus reducing the need for mechanical compression. An emphasis is also placed on a multi-cell electrolyzer design (pilot plant scale-up) powered by renewable energy sources.
- This high-pressure electrolysis development program is fully aligned with DOE research, development, and demonstration objectives. Generation of 6,500 psi hydrogen allows direct filling of vehicle hydrogen tanks.
- Electrolysis tied to renewable resources is an important part of the DOE Fuel Cell Technologies Program portfolio.
- This is definitely a relevant project.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Nested composite overwrap has shown promising results, but the team needs to further investigate the reactant mixing issue at each side of the cell.
- –The team needs a clearer pathway on where cost targets can go and how this can happen. Feasibility is also a major question, as the team is struggling to meet pressure at a safe purity level.
- The approach of using a cylindrical cell design and scaling up from a single-cell to a multi-cell “nested” design is feasible, but will require some creative engineering to complete.
- It would be interesting to see a cost study indicating the number of cells versus the length and diameter of cells during scale-up.
- The approach is very sound, with a logical progression of development. The initial work demonstrated 6,500 psi hydrogen production at a laboratory scale. The next logical steps being pursued are electrolyzer and balance-of-system scale-up. Problems identified during this scale-up work are being addressed. Success at resolving these problems will determine if continued development is warranted.

- The team is focusing on the major issue, which is whether high-pressure production can be achieved.
- The team is approaching this as an engineering problem (i.e., finding commercially available products to solve their problems). It is very likely that they will need to develop the materials themselves and it is not clear that they are prepared or planning on doing the material development themselves.
- The project is being executed with excellent efficiency given the resource level applied.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- Research did focus on a single path and understanding the design limitations, including its potential for further improvements. They are eliminating the need for a mechanical compression system by generating high-pressure reactant at the stack level.
- Significant issues were encountered in keeping oxygen purity at safe levels and it is not totally clear that these are going to be resolved. Nested cells will likely have even more issues than single cells.
- Progress demonstrating 6500 psi operation in single-cell testing has been made; however, more work towards completing the multi-cell (or nested-cell) design needs to be shown.
- A loss of cell efficiency is seen with the use of thicker membranes that are required for high pressure (6500 psi) operation. The effects of lost cell efficiency on the cost of hydrogen should be shown via the H2A model.
- Some technical and safety issues need to be resolved, including the possible accumulation of unsafe gas mixtures in the tubes exiting the electrolyzer cell.
- Significant progress has been made in terms of demonstrating the performance of the nested cell core at the laboratory scale and in demonstrating the ability to implement a composite fiber outer wrap. Reaching 6,500 psi pressure levels in larger cells remains the major obstacle, along with purity of the hydrogen and long-term operation. These obstacles are a significant challenge, and therefore progress will likely be slow. Modification of project goals should reflect this challenge.
- The progress seems modest for the time and funds spent up to this point.
- The team needs to show that they can operate at the higher pressures
- The electrolyzer efficiency is low and needs to be improved.
- The project demonstrated the membrane operation for a couple of hundred hours. The team needs to show the operation for over 1000 hours under real conditions.
- The team correctly acknowledges that their 6500 operation is not good enough. 96% oxygen purity should be end-of-life at worst, not beginning-of-life operation. The system clearly cannot last the 1000 hours of operation that is required. This was a single cell experiment and it is likely that the performance of nested cells will have higher impurities.
- The efficiency is very low and needs to be increased. The project is currently at approximately 50% efficiency, which is not close to the DOE targets (~69%).
- The project is demonstrating the high pressure operation requested by the DOE. Additionally, practical findings are being disseminated in the public forum. Full system integration up to 6250 psi is providing great practical learning for the program.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- The researcher can benefit from further collaboration with additional industry companies that specialize in sealing and hydrogen and oxygen diffusion.
- There was no real discussion of the progress of collaborators or any interaction between collaborators. The collaborators seem more like a supplier (of composite wrapping) and customer (using the National Renewable Energy Laboratory site), rather than much of a collaborative effort.
- In addition to last year's collaborations, Avalence is also working with Parker on the development of high pressure components (high-pressure hose assemblies) for the cell design.
- Some collaboration with HyperComp, Hydrogen Energy Center, and MaineOxy exists, but little information was presented to assess the degree of collaboration.

- The project has a good team, but the interactions are not clear. It seems that they will really begin collaboration later.
- More collaboration between electrolysis companies would benefit the technology. For example, Giner appears to have a higher expertise in materials, while Avalence has a stronger system architecture and system optimization.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Continuing on the path of developing larger cells and demonstrating scalability can support the 6500 psi reactant production without any mechanical compression.
- The project has some ideas to try, but did not really show any feasibility to date.
- A specific goal to scale-up the hardware for the pilot plant has been set. Avalence will need to demonstrate large diameter cell operation at 1000 psi, a go/no-go decision point for next year, and long-term 6500 psi operation.
- Barriers have been identified and are being addressed, predominantly in the areas of 6,500 psi operation with larger cells and reactant purity.
- The team needs to improve their membranes to increase the gas purity.
- The team needs to perform longer life testing.
- The team needs to increase their efficiency.

Strengths and weaknesses

Strengths

- Research did focus on a single path and understanding the design limitations, including its potential for further improvements. Eliminating the needs for a mechanical compression system by generating high-pressure reactant at the stack level is a strength.
- An alternative to acid-based electrolysis should allow for less expensive materials of construction, if the cost of high-pressure components does not exceed the benefit.
- Avalence has successfully demonstrated 6500 psi operation in single-cell testing. Although life-cycle testing is required, the short-term testing demonstrates the feasibility of the high-pressure cylindrical cell design.
- The core technology shows extreme promise at the laboratory scale. Some issues with respect to system scale-up have been successfully addressed, while other significant issues still remain. Avalence is very experienced and capable, with a sound project plan.
- It is an interesting concept to generate the hydrogen at 6500 psi.
- This seems to work for a single cell.

Weaknesses

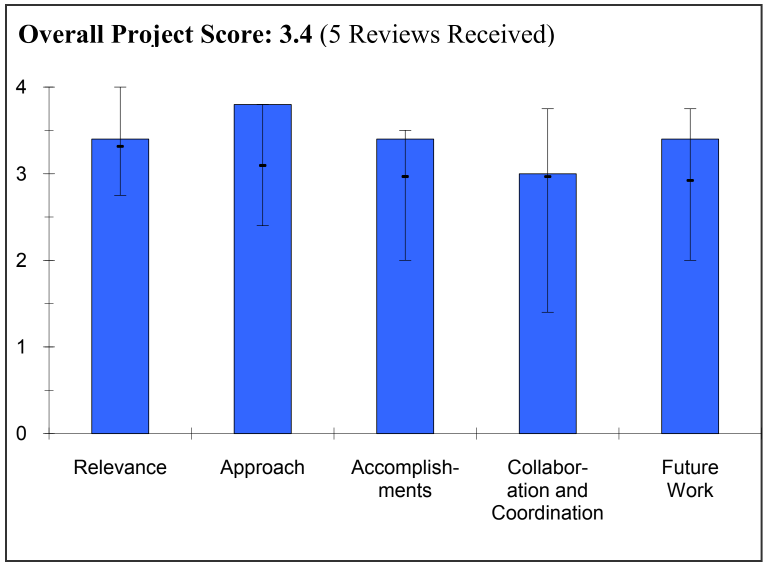
- Hydrogen and oxygen diffusion through the material was not clearly addressed. The study did demonstrate eliminating the diffusion to some level by adding complexity to the balance-of-system but it did not address potential overwrap material improvements.
- The high-pressure and complex design seem to be adding more cost than benefit to the project, and safety is a serious concern.
- Polarization scans (I-V performance curves) were not given for the testing that was conducted. The stated power consumption (kWh/kg of hydrogen) for 40 and 80-mil membranes needs to be quantified by stating the specific current density operating points.
- No major weaknesses exist. There are several challenging barriers which need to be overcome, and these are being addressed.
- The efficiency is very low.
- The project is having issues with materials.
- Safety is a key concern.

Specific recommendations and additions or deletions to the work scope

- The project would benefit from further study on reactant diffusion through the overwrap material and fabricating a larger stack, as well as demonstrating the technology scalability of any hydrogen generation at 6500 psi.
- If pressure targets cannot be reached, the team needs to look at this approach as a whole to see if there is still any benefit, including the efficiency and cost implications of compression and maintenance.
- An economic study of the system design should be conducted to demonstrate the feasibility of meeting the DOE cost targets for this program.
- Avalence should provide I-V performance curves in order to determine cell efficiency.
- No additions or deletions are recommended. The project should continue on its present path to address scale-up capable of demonstrating 6,500 psi hydrogen generation with high purity. Without this successful demonstration, further development is likely not warranted.
- The team needs to test the membranes at higher temperatures. When they operate in the nested-cell configuration, the internal temperatures will be higher. The team also needs to show that their bonding technique will work for a long period of time.
- Avalence would benefit from looking at higher reliability and efficiency compression technologies, such as the Linde ionic compressor. This may reduce their safety concerns and allow them to operate with lower-cost pressure vessels, while still capturing a high-efficiency operation. 3,000 psi may be a good "knee" in the optimum electrolysis pressure of operation. It would be beneficial to show diagrams of the system capital, operating, and feedstock costs compared with the various pressures in order to show what the optimal electrolyzer pressure might be.

Project # PD-30: PEM Electrolyzer Incorporating an Advanced Low Cost Membrane
Monjid Hamdan; Giner, Inc.

Brief Summary of Project



The overall project objectives are to develop and demonstrate an advanced, low-cost, moderate-pressure proton exchange membrane water electrolyzer system to meet DOE targets for distributed electrolysis by 1) developing a high-efficiency, low-cost membrane; 2) developing a long-life cell-separator; 3) developing a lower-cost prototype electrolyzer stack and system; and 4) demonstrating a prototype electrolyzer system at the National Renewable Energy Laboratory. Objectives for FY 09-10 are to 1) fabricate scaled-up stack components, 2) assemble and operate short stacks at Giner Electrochemical Systems for 1,000 hours, 3) complete system critical design review (CDR), and 4) begin fabrication of deliverable system.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The study did meet the DOE research, development, and demonstration project objectives, demonstrating an advanced, low-cost, 300-400 psi pressure proton exchange membrane water electrolyzer.
- This project contains a lot of good science that advances important principles. While the exact configurations may not be cost practical at this point, the underlying knowledge is important for setting design limits and advancing the field.
- The project is very relevant to DOE objectives in terms of cost and efficiency, but operating pressures of 300-400 psi will require a compressor to reach their ultimate goal of 6,500 psi, with its inherent cost and efficiency penalty. Stack optimization and system development exhibit a clear focus in meeting DOE goals.
- This project aligns with the DOE Hydrogen Program goals.
- This is indeed very relevant, as it provides renewable conversion to hydrogen.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- The dimensionally stable membrane (DSM) does show promising results and needs to be investigated further.
- All pertinent technical barriers are being addressed, often with multiple approaches. The approach includes a very feasible path toward demonstrating membrane reproducibility and durability, cell separator development, scaled-up stack design, and system development.
- The team has identified the critical path to develop a commercial product that meets the DOE targets.
- Giner is considering the complete system in optimization, including the DOE H2A values for compressor operation up to 6250 psi. However, it would be beneficial to explore the operation of all components up to ~3,000 psi or higher. This is in line with what was found by Avalence.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- A significant amount of work was spent on the characterization of the DSM and a couple of polymers, in house material, and an industry standard polymer (3M).
- Significant progress has been made in overall understanding. However, POCO graphite is very expensive and it is not likely to provide a cost savings. Some data was also somewhat misleading in that there are other failure mechanisms besides those being tested. It is not reasonable to claim 50,000-60,000 hours based on a 1000-hour test.
- Progress has been steady in all areas, with successful demonstration of DSM performance and lifetime. The remaining stack work is in the scale-up of membranes and separators, which should be challenging but not insurmountable. Successful completion of CDR will allow the initiation of system assembly and demonstration.
- The team has made very good progress.
- The stack efficiency is very impressive.
- Use of their high pressure dome increases safety and helps performance.
- The failure modes and effects analysis, as well as work on codes and standards, was included.
- The team decreased the cell assembly cost by decreasing the number of parts and changing manufacturing.
- Giner is showing good high-efficiency operation.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The program did not show strong collaboration with industry (i.e., 3M is only a supplier). If they were a collaborating company, the presentation did not state any information except for indicating 3M as a polymer supplier.
- There was a good description of systems interaction in the safety review. The Entegris collaboration is apparent in the parts made.
- The partners are full participants and the work appears to be well coordinated. Parker Hannifin is a large-volume commercial manufacturer working on system development, Virginia Tech is a leader in hydrocarbon membrane research, 3M is a major membrane manufacturer and catalyst developer, and Entegris produces carbon cell separators with improved resistance to hydrogen embrittlement.
- The team is working with Parker Hannifin for balance-of-plant cost reduction.
- It is not clear if the team is working with 3M or just buying their catalyst.
- It would be nice to see more publications.
- More collaboration with other electrolysis companies would be beneficial. Electrolysis companies are, to a large extent, in the same boat. They would benefit significantly by pooling research resources in a manner similar to the photoelectrochemical projects.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- The team is developing an end-to-end system with lead industry suppliers.
- The next steps seem logical based on the work performed to date.
- Their proposed plans are logical, building on past progress to overcome the barriers of scale-up. The success of their scale-up activities will be a determining factor to warrant continued development.
- The future plans clearly push their development.
- The team is focused on the barriers.
- Mechanical pressure cycle testing would be of interest.

Strengths and weaknesses

Strengths
- The project did demonstrate technology viability and low-cost hydrogen production.

- The project is aiming to develop a large-scale system and further evaluate the technology viability and durability at the system level.
- The project has a good fundamental science capability in terms of testing and approach.
- The project is very sound with demonstrated progress to date in the development of high-efficiency, low-cost membrane and a long-life cell separator. The barrier of scale-up of these components is being addressed. This barrier is challenging, but not insurmountable. The project has a highly effective and coordinated team approach.
- This is a strong team.
- The project has high efficiency and a low-cost stack.
- The titanium separator work is very good.
- The life test studies and accelerated testing experiments are well done. The team is using a good mix of experimental studies.

Weaknesses

- A continuous operating run above 500 hours is needed for a true technology viability evaluation. The system durability is not there yet.
- The team has little commercial experience, or units in the field on which to base cost projections or lifetimes. Their extrapolations are very long.
- No major project weaknesses exist. Scale-up of cell hardware is the major remaining barrier, which is being addressed.
- It is unclear how the team is going to commercialize the technology.

Specific recommendations and additions or deletions to the work scope

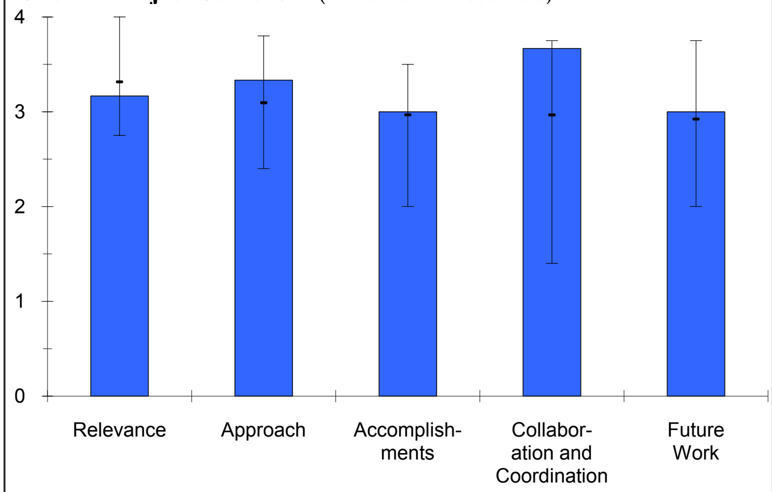
- The project should be continuing with the end-to-end system development to demonstrate overall system efficiency, viability, durability, and cost efficiency.
- The reviewer has no recommended additions or deletions.
- The project should continue.
- As the project is exploring the pressure vessel operation, it would be beneficial to explore high-pressure operation as well, for example to demonstrate 3,000 psi operation. While H2A-optimized hydrogen costs appear to be good with 300 psi operation, practical considerations of system reliability and distributed generation operation and maintenance would suggest that the elimination of compressors would be desirable. The Giner architecture also has a good opportunity for operating at elevated pressure. In addition, this would help them reduce parasitics associated with hydrogen drying.

Project # PD-31: Renewable Electrolysis Integrated System Development and Testing
Kevin Harrison; National Renewable Energy Laboratory

Brief Summary of Project

The objectives of this project are to 1) identify opportunities for system cost reduction and optimization as they pertain to electric utilities; 2) characterize, evaluate and model the integrated renewable energy systems; 3) characterize electrolyzer performance with variable input power; 4) design, build and test shared power electronics; 5) develop cost models for renewable electrolysis systems; 6) quantify capital cost and efficiency improvements for wind and solar-based electrolysis scenarios; 7) perform characterization and performance testing on electrolysis systems developed from DOE-awarded projects; and 8) test electrolyzer stack and system response with typical renewable power profiles.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The system performance analysis and evaluation fall within DOE research, development, and demonstration project objective.
- The project is small-scale and it is questionable if this applies to larger-scale systems.
- This project provides a real-world demonstration of integrating renewable power sources with electrolyzers for hydrogen production, and as such it is extremely relevant to the DOE Hydrogen Program and fully supports DOE objectives. Real-world demonstrations are invaluable in addressing capital cost, efficiency, and integration with renewables to reduce the costs of generating hydrogen.
- This project supports the DOE Fuel Cell Technologies Program.
- The work aims to address cost reduction to meet the hydrogen cost goal in the area of wind and solar electrolysis. Since DOE has already funded their Technology Validation Program, it is questionable what additional value can be gained from doing the refueling station portion of the work.
- Hydrogen for storage is likely to be an early stage application for hydrogen and fuel cells, independent of vehicle success. High costs limit its practicality in the absence of a carbon tax or other policies to enable low-carbon hydrogen.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The project is evaluating industry-developed end-to-end systems and/or components and technologies.
- The project is highly focused on demonstrating advanced controls, system-level improvements, and the integration of solar and wind renewable sources with electrolyzers. Their demonstrations are in the field and are real-world focused, generating valuable data.
- The energy storage cost analysis work approach is very interesting.
- The wind-to-hydrogen project approach is interesting, but the proposed integration does not seem correct. It is highly unlikely that wind towers will be dedicated to hydrogen production. It is more likely that they will do both hydrogen production and electricity production, so the power electronics would become redundant.

- The power electronics work on the solar side is very interesting.
- Providing independent verification for government-funded electrolyzer development projects is very important.
- In the effort to reduce hydrogen cost, the project investigator focused on the appropriate areas that have the most influence in the overall hydrogen cost. It is a good strategy to let the grid take extra variable power from the wind and have the stack continuously run on a stable output.
- Data gathering along with close coordination with electrolyzer manufacturers provides valuable analysis.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The project has shown excellent progress in several areas, including unattended long-duration operation of both proton exchange membrane (PEM) and alkaline electrolysis systems, electronics comparison of direct coupling with maximum power point tracking, analysis and benchmarking of hydrogen-based energy storage, and the commissioning and operation of a 350-bar refueling station. Barriers to the acquisition of more long-term test data are being addressed.
- The hydrogen station, shuttle bus, and the independent analysis are not part of this project, so it is unclear why they are discussing them. There is very limited time so the team should focus on the accomplishments that pertain to their project.
- The team finally began long duration testing, which is good.
- The power electronics for optimized solar photovoltaic (PV) electrolyzer operation was interesting, and the fact that the power electronics do not need tuning at each site is good.
- The data showed that unattended operation appears to be just a few days long. The project investigator needs to consider longer hours of unattended operation of both PEM and alkaline systems under variable wind conditions to test both stacks and power electronics under real-world conditions.
- The finding of the value of power electronics versus irradiance is significant.
- The drying energy consumption work has value.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- Numerous participants are involved in project and all seem to be fairly well coordinated. Participants include electrolysis and renewable power manufacturers, utilities, a university, and a research center.
- The work with partners and feedback to electrolyzer manufacturers is good.
- The international work is excellent. The development of international standards for electrolyzer testing is important.
- The team has excellent collaborations with various electrolyzer companies and various agencies to share lessons learned.
- The project has close ties to electrolyzer manufacturers.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The team is evaluating the system performance of several vendors that are currently supported by the DOE research, development, and demonstration program.
- It is unclear if the assumptions for the cost of hydrogen produced using PEM units are realistic. Generally it is not realistic to assume PEM hydrogen cost based on a unit production number of several thousands.
- The project should try to include the cost of hydrogen for near term as well, when the number of PEM hydrogen production units is in the tens.
- The proposed future work includes comparison testing of PEM and alkaline electrolysis systems, side-by-side electrolysis stack testing, and investigation of the dynamic response differences between fuel cell, photovoltaic, and wind energy sources.
- The independent analysis and validation of the electrolyzer systems is good.

- Integration of the fuel cell is a good step to demonstrate the use of hydrogen for energy storage.
- If the intent is to compare PEM systems to alkaline systems, the project investigator needs to compare them on the same basis and have a list of appropriate evaluation criteria.

Strengths and weaknesses

Strengths
- The independent assessment of current DOE technology suppliers end-to-end systems is a strength.
- The project has a good overall systems evaluation approach.
- Renewable hydrogen production is a strength.
- The inclusion of several different technologies is a strength.
- The most significant project strength is the generation of real-world test data showing integration of renewable power sources with electrolysis systems. This test data is invaluable.
- DOE has spent a lot of money building the infrastructure for this project.
- The partners and feedback to the industry is important.

Weaknesses
- The lack of benchmark numbers from other projects is a weakness.
- It would be beneficial if there is more demand for hydrogen at hydrogen vehicle refueling stations because this would make the work more realistic due to having consistent demand.
- There are no weaknesses of note.
- The project is not focused. It wandered between wind-to-hydrogen and comparative testing, and then to activities that are not part of the project. They need to be careful that this does not turn into a “make work” project. DOE does not have the funds for a “make work” project and the project investigator is too good of a researcher to be used in this way.
- There is very little new technology, which is a weakness. The project seems to be more about the evaluation and optimization of commercialized technologies.

Specific recommendations and additions or deletions to the work scope

- Independent assessment of electrolysis systems helps to further understanding of this technology shortfall. The project should verify the operation of a similar, large-scale project in Stolpe, Germany. The Clean Energy Partnership (Total, Vattenfal, etc.) is involved in this project that uses a large windmill (330 kW) and electrolyzer to do similar work to this project. The presenter stated this concept was the only project of its kind in the world but it may not be. See the following resources: http://www.hynor.no/total (p.30-33), www.iphe.net/docs/Meetings/Norway_3-09/post-meeting/Germany/CEP.pdf (p.17), and www.netinform.net/H2/H2Stations/H2StationsDetail.aspx?ID=352.
- The team should make sure to include the production-level assumption when presenting cost targets because it was unclear what level they are based on (e.g., 1000 units produced or 1000s of units produced).
- The project should continue to expand the generation of real-world test data with even more electrolysis manufacturers and system concepts.
- As always, the team needs to continue long duration testing. They also need to show the integration with the wind.
- In the future, focus the presentation on work and accomplishments that are directly funded for this activity. The filling station and the bus, although interesting, are not part of the project and too much time was spent on them. It would have been better to spend more time on the long duration testing, the comparative testing, and the power electronics, which are key to this project. A lot of good work was done on these areas and more detail would be interesting.
- This comment is for both the project investigator and the DOE. Providing feedback to the manufacturers is very important work. NREL should continue doing this as part of the DOE electrolyzer programs (i.e., for currently active DOE electrolyzer projects) and provide independent verification of electrolyzer stack development, paid for by the DOE. It is unclear whether it is appropriate for the government to pay NREL to provide feedback to private companies to improve their technologies that are not part of a research and development project. For example, the HOGEN units being tested are not part of the development activity. This may be easily misinterpreted as consulting work paid for by the government. Perhaps some cost share from industry receiving

the feedback would be appropriate when the units tested are not directly part of a DOE-funded development activity. The concern is in the optics, not the execution.

- It is a good approach to let the grid take the extra "wild" power to keep the electrolyzer running on a steady level of power. The project investigator should consider other integration and operation options such as, (1) letting the electrolyzer run entirely on wind power to make hydrogen for point-of-use or storage (this is more feasible at remote locations without power transmission lines), or (2) sell all power to the grid at a higher price for renewable power and pull a little bit of electricity from the grid to make hydrogen. This might make more sense with limited and unpredictable hydrogen demand.
- The project should be expanded to consider hybrid electricity, i.e., wind, solar, and grid, to maximize return and minimize produced electricity costs.
- The project needs to consider the economic tradeoffs of constant utilization using grid electricity to complement wind or solar production. It is unclear whether using grid electricity offsets the capital cost to the extent that it might significantly lower the cost of peak power from stored hydrogen.

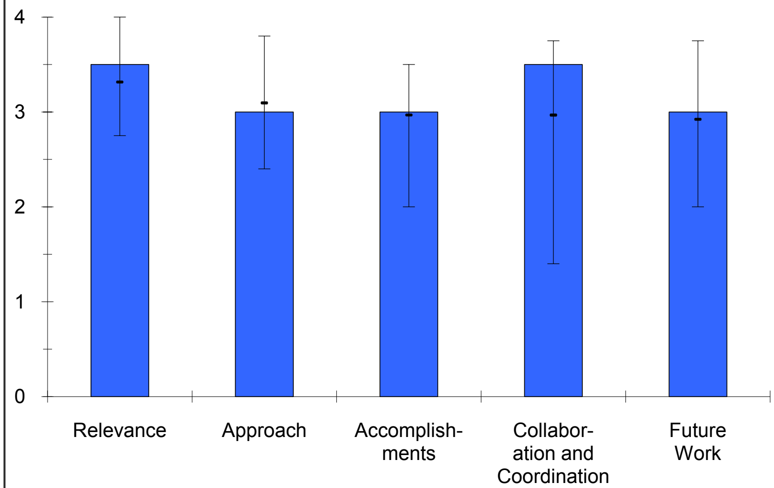
Project # PD-32: Photoelectrochemical Hydrogen Production: DOE PEC Working Group Overview

Eric Miller; University of Hawaii at Manoa

Brief Summary of Project

The U.S. DOE Photoelectrochemical (PEC) Working Group brings together academic, industry and national laboratory leaders in the research and development of practical PEC semiconductor systems to produce hydrogen via solar water-splitting. The DOE PEC Working Group's primary objective is to develop practical solar hydrogen-production technology, using innovative semiconductor materials and devices research and development to foster the needed scientific breakthroughs for meeting DOE Hydrogen Program goals.

Overall Project Score: 3.2 (2 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is at a very low technology readiness level (TRL). Each of the collaborating members is focusing on different synthesis materials, and there are many more materials for consideration.
- There are a number of good projects, especially those attempting to incorporate nanoscopic structures into their systems.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Further planning of work and management of tradeoffs is still needed. More materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- A three-pronged approach involving theory, synthesis, and analysis is promoted, which seems like a good strategy, but it needs more balance. There were only two theory projects (at approximately $100k each?) and only one analysis (at $100k?). Everything else is synthesis, accompanied by its own internal analysis.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- There are many other materials and experimental methods that need to be explored and evaluated, but the question is whether any of them would pan out.
- There were a few impressive reports and everyone seemed to have made some progress.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Collaboration is demonstrated among the team, in that each member is working on a different material for the same technology and sharing the results.
- A lot of good synergies have been set up. It was difficult to grade the projects, as it seemed like any given group's progress was significantly reliant on data acquired at other collaborating institutions.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- There are many other materials and experimental methods that need to be explored and evaluated, but the question is whether any of them would pan out.
- There is a good understanding of the technological barriers, but some of the projects are still basically hit and miss, with trying a material based mainly on chemical intuition and the presence of a decent band gap.

Strengths and weaknesses

Strengths

- Focusing on materials and evaluating their potential and viability is a strength.
- This is some really exciting work in the nanoscopic structures area. New computational power has the promise of discovering new semiconductor electrode materials.

Weaknesses

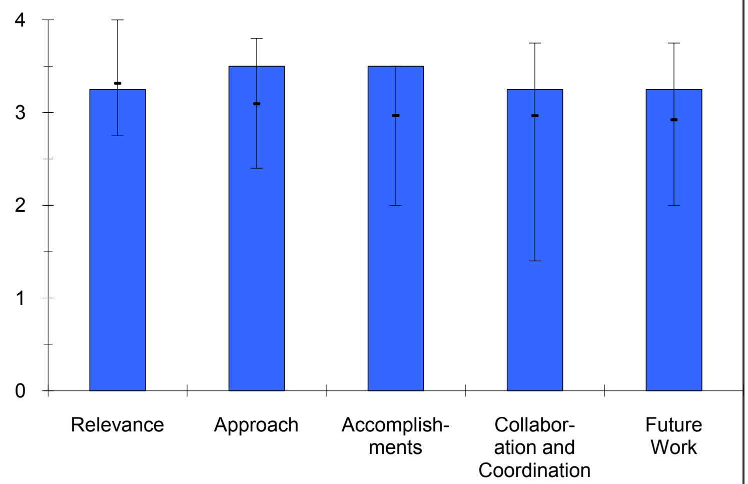
- Technology viability, scalability, and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- On the basis of simply delegating the projects among the three corners of the triangle, the program seems heavily weighted on analysis, and less on theory and synthesis. The presentation seemed to emphasize new predictive power, but the reviewer is not sure that any new compounds have been identified to date. It is unclear whether someone is going to investigate the compounds they identify. The need for large-scale manufacturing methods is recognized, but it still appears that most systems are small and require a vacuum chamber to for fabrication.

Specific recommendations and additions or deletions to the work scope

- The team should consider reevaluating technical viability and feasibility.
- The two theory groups are both at national laboratories. DOE should issue a request for proposals soliciting help from academic institutions.

Project # PD-33: Nanostructured MoS2 and WS2 for the Solar Production of Hydrogen
Thomas Jaramillo; Stanford University

Overall Project Score: 3.4 (4 Reviews Received)



Brief Summary of Project

The main objective of the project is to develop new photoelectrode materials with new properties that can potentially meet DOE targets (2013 and 2018) for usable semiconductor bandgap, chemical conversion process efficiency, and durability. To date, there are no known materials that simultaneously meet these DOE targets.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of the proposed technology is at a very low TRL. Each of the collaborating members is focusing on a different synthesis material and there are many more materials for consideration.
- This is not a specific criticism of this talk versus the others, but at some point there needs to be a better cost analysis from the PEC Working Group on the entire system design. Even in PEM electrolysis the catalyst currently represents less than 10% of the entire cost. This will become more significant as the balance of plant cost is reduced, but at less than 10% of the current density for PEC the team will need a lot more benefits besides reducing the cost of platinum group metals to move forward.
- The project has a solid focus on PEC hydrogen.
- The team has a good understanding of the technical challenges to meeting targets for PEC water splitting.
- The project is investigating low-cost synthesis routes.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- There is still planning of work and tradeoff needs to be done.
- The team has a very well laid out experimental approach and grounding in the necessary advancements.
- The team understands the properties of nanoparticles and how they can be exploited.
- The team is applying concepts from nanomaterial literature to PEC applications.
- The team has a good understanding of materials properties and system needs.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- There is still planning of work and tradeoff needs to be done. More materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- The proof of principle looks good, but there is a lot of work left to do. However, the approach allows a focus on demonstration of the right aspects.

- It is difficult to say what has been done singularly in the last year, but what was shown looked like technical progress to me.
- The project achieved a wider bandgap, as designed.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaboration is shown among the team, in that each member is working on different material for the same technology and sharing the results.
- The PEC Working Group has a lot of synergy, but there could be a little more clarity on how the work from Heske and Yan has helped guide the next steps.
- The project is fully integrated into the PEC Working Group.
- The team is working well with collaborators.
- The project is actively participating in the PEC Working Group.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The team is evaluating different materials and their potentials.
- The project would benefit from input from some of the theory people.
- The project has an appropriate work plan moving forward.

Strengths and weaknesses

Strengths
- Evaluating materials viability is a strength.
- There is a good fundamental science and approach in general with a single focus, which allows for better understanding.
- The project has an excellent application of nanoscience. This may be DOE's best bang for the buck.

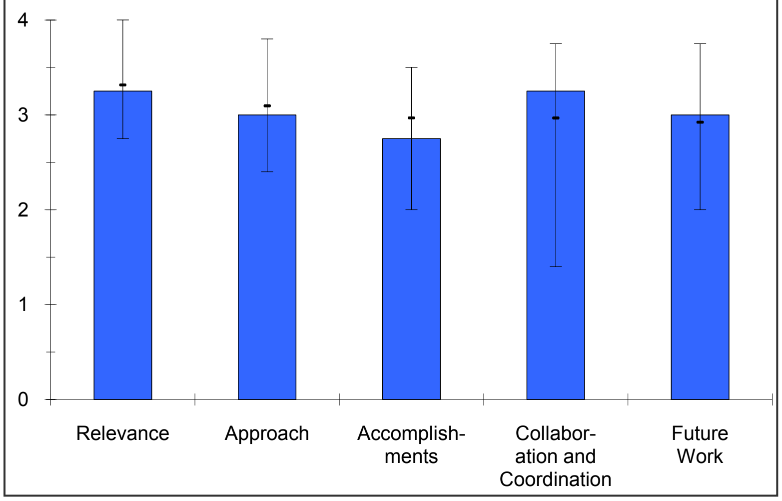
Weaknesses
- Technology viability, scalability and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- A practical device is a long way off, but the project could use some basic analysis to show that a system is cost feasible.

Specific recommendations and additions or deletions to the work scope

- The team should consider reevaluating technical viability and feasibility.

Project # PD-34: Development and Optimization of Cost Effective Material Systems for Photoelectrochemical Hydrogen Production
Eric McFarland; University of California Santa Barbara

Overall Project Score: 3.0 (4 Reviews Received)



__Brief Summary of Project__

The objectives for this project are to 1) develop improved materials for solar photon absorption using high throughput methods and new syntheses, with a focus on abundant and non-toxic elements; 2) use high-throughput screening to identify candidate materials with a threshold efficiency and stability that, with optimization, might meet the DOE performance and stability targets; 3) explore the effects of morphology on the PEC material system efficiency making use of nanostructures to minimize charge carrier path lengths and maximize reactive surface area; 4) explore processing and synthesis parameters to optimize efficiency through increased conductivity and minimized charge trapping and surface recombination of selected materials; 5) identify and minimize electrokinetic limits by synthesis of appropriate electrocatalysts compatible with the host, electrolyte, and reactant/product properties; 6) develop a complete "PEC unit," combining material absorption, charge transport, stability, and electrokinetic design features; and 7) evaluate conceptual model reactor systems, theoretical and practical economic potential of alternative redox reactions, and estimate hydrogen production costs.

__Question 1: Relevance to overall DOE objectives__

This project earned a score of **3.3** for its relevance to DOE objectives.

- Perhaps the technology dose meets the DOE research, development, and demonstration intent but the current state of proposed technology is in a very low TRL. The question is, compared to current PV technology, how superior are new materials going to be and would they improve the current PV efficiency by 50% or another level.
- As part of the PEC Working Group collection of projects, this project has the same benefits and drawbacks. It offers an interesting area for study but it is likely a long way from practical system development.
- For the hydrogen program to be a success, both cost and scale are vital components. This project keeps in mind that the use of expensive or rare materials will drive the final cost up, specifically by looking at abundant, low cost elements.
- The team is addressing barriers to efficient and cost-effective PEC production of hydrogen.

__Question 2: Approach to performing the research and development__

This project was rated **3.0** on its approach.

- The research does focus on evaluating different syntheses, and laboratory evaluations of small samples do show some promising results.
- The project investigator did not attend the poster or have a delegate, so it was impossible to ask questions on the approach. Normally indium phosphide is not stable in water. The use of alternative oxidants seems to get away from the goal of direct water splitting.
- The project shows good focus, not just on good basic science, but also on incorporation into a scaled system.
- They are using a systematic approach with go/no-go decision points.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- There is still planning of work and tradeoff needs to be done. The current technology readiness level is very low, about TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new proposed technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- The budget seems large for the amount of progress made.
- Progress has been achieved in two major areas. The proposed coupling of the PEC hydrogen process with the biomass/methane coupling process gives a compelling, if complex, system to examine. This innovation in thinking has been proposed by the investigator to have side-stepped one of the more difficult challenges of PEC, oxygen oxidation, and replace it with the more facile bromide oxidation.
- The project has made good progress, especially considering there has been no funding in the past two years.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Other members of the working group are working very well together and the reviewer did not have any comments to the contrary regarding this project investigator.
- In a project that is a confederation, rather than a fully coordinated multi-institution project, the collaborations and discussions are very good.
- The team is collaborating with key players and institutes.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- There are many other materials and experimental methods, which need to be explored and evaluated. The question is whether any of them would pan out.
- The proposed future work clearly builds on the successes and failures of previous work, while keeping an eye on the overall program targets.
- DOE should fund this work if they are expecting additional results.

Strengths and weaknesses

Strengths
- The project did well in out-of-the-box technical approaches, collaboration with other research labs, and focusing on the stability of materials.
- The project had strong characterization abilities.
- The synthetic capabilities to create new materials are strong, as well as the ability to formulate target compositions and then create them. The high-throughput screening capabilities enable rapid screening of compositional variations. The continued search for how to overcome barriers yields innovative proposed solutions worthy of further investigation.

Weaknesses
- Technology viability, scalability, feasibility and manufacturing in large scale are not addressed or recognized.
- No cost projections are provided, and there has been very little output from Tasks 7-9.
- It is not fully clear if the proposed particle passivation schemes will be able to keep the particles unagglomerated in the reactor without diminishing photo activity.
- The reviewer presumes there is a good understanding by the investigator, though it is not clear if there is strong understanding/collaboration regarding the engineering challenges that may be present with integration of the proposed CH_4/Br_2 cycle step.

Specific recommendations and additions or deletions to the work scope

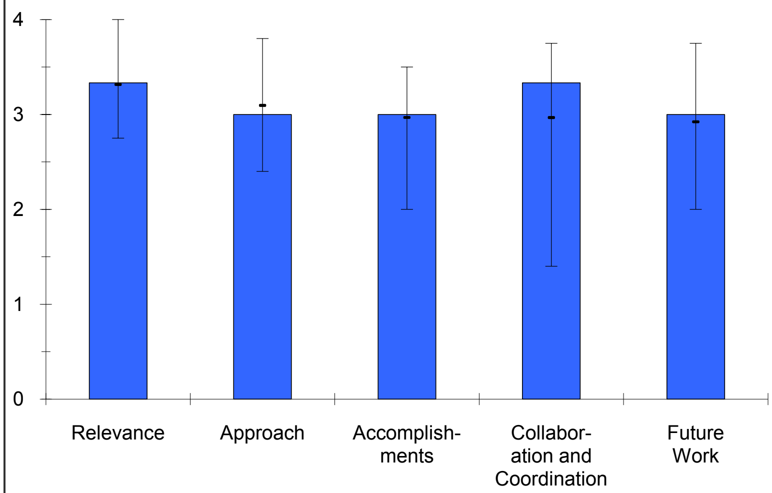
- The project should produce larger active area cells.
- The project appears to be ending this year.

Project # PD-35: Semiconductor Materials for Photoelectrolysis
John Turner; National Renewable Energy Laboratory

Brief Summary of Project

The objective of this work is to discover and characterize a semiconductor material set or device configuration that, 1) splits water into hydrogen and oxygen spontaneously upon illumination, 2) has a solar-to-hydrogen efficiency of at least 5% with a clear pathway to a 10% water splitting system, 3) exhibits the possibility of 1,000 hours stability under solar conditions, and 4) can be adapted to volume manufacturing techniques. The main focus of our work this past year has been to develop and optimize state-of-the-art materials that we have identified as promising for meeting DOE's near-term efficiency and durability targets.

Overall Project Score: 3.1 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is in a very low TRL. Each of the collaborating members are focusing on different synthesis materials, and there are many more materials for consideration.
- Materials are the best to date, in terms of direct water splitting. The reviewer would recommend specifying the project dates for this phase versus the entire field. If we have really been working on this same project since 1991, the progress is not impressive.
- This is a vital link between the storage of solar energy and hydrogen energy technology.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The research does focus on evaluating different syntheses, and laboratory evaluation of small samples does show some promising results.
- The experimental approach is reasonable for the short term, although RuO_2 as a counter-electrode does not eliminate the platinum group metal (PGM) issue. It is also misleading to say that $5.50/kg is "quite possible" based on the current status. This would require a 20% efficiency, 20-year lifetime and cost of $\$80/m^2$ of material, all simultaneously.
- The project investigator understands the technical barriers, but the project is so big it is hard to define a central theme or focus.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- There is still planning of work and tradeoff needs to be done. More materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.

- It is not totally clear what work is occurring under the project investigator versus other partners. A lot of the work presented also seemed to be in specific groups' posters. However, stability mitigation strategies are an important part of the strategy, especially if they can be applied beyond the level III-V systems.
- The team tried to show technical progress by reporting projects moving ahead and others being laid aside. It was difficult to decipher whether the technical progress was occurring at NREL or elsewhere, or whether the work was funded by money flowing directly from DOE or via an NREL subcontract. In other words, a lot of results were shown, but it is unclear how much of it should have been in NREL's presentation.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material for the same technology and sharing the results.
- The PEC Working Group seems to be very well connected with each other and working in synergy.
- The team has excellent PEC Working Group interaction. They are clearly taking a leadership position.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- I would like to see the computational predictive power put to more use.

Strengths and weaknesses

Strengths
- Focusing on a single material and evaluating its potential and viability is a strength.
- This is essential work as part of the PEC Working Group, in developing reporting standards and common testing. Stabilization strategies are an important part of the project and this is the one group discussing such strategies. A long history and deep knowledge in the area of direct water splitting should provide good direction for evaluation.
- The project brings leaders in the PEC community into the DOE program. It was a good idea to establish efficiency standards.

Weaknesses
- Technology viability, scalability, and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- The project investigator should focus on making the project stand on its own merits instead of stating very broad and misleading statements about other approaches (e.g., combinatorial synthesis and electrolysis in general). The system capital cost and utilization factor is not clear, raw materials are expensive, and the system will be complex. Someone, whether this PEC Working Group or DTI, should be disclosing more of the assumptions that went into the cost model because the system design is not going to be simple in terms of compressing the hydrogen.
- The anticipated synergism between theorist and experimentalist has not quite happened yet. The reviewer is waiting for the experimentalist to try something that the theorist points to. Low-cost synthesis was listed as a secondary concern after materials, but the reviewer did not see anything on it.

Specific recommendations and additions or deletions to the work scope

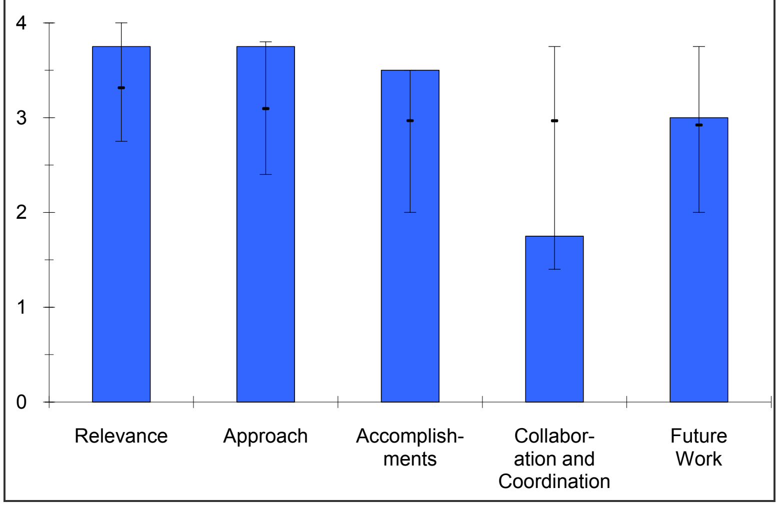
- The team should consider reevaluating technical viability and feasibility.

Project # PD-36: Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures
Tasios Melis; University of California Berkeley

Brief Summary of Project

The objective of the project is to minimize the chlorophyll antenna size of photosynthesis to maximize solar conversion efficiency in green algae. The project will identify and characterize genes that regulate the Chlorophyll antenna size in the model green alga, Chlamydomonas reinhardtii, and apply these genes to other green algae, as needed.

Overall Project Score: 3.4 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Improving the light-to-hydrogen conversion efficiency by reducing antenna size fits well with the DOE Hydrogen Program mission.
- The project has a very clear relevance to the stated DOE Hydrogen Program goals for the improvement of light utilization efficiency in photobiological hydrogen production.
- The reduction of photosystem antenna size is potentially a very helpful element so that photobiological hydrogen production can work well in the long term.
- The issue of adapting the antenna size to optimize growth is key to the large-scale, inexpensive production of algal biofuels. This has been a very systematic and productive study, making clear progress each year at decreasing the antenna size and increasing the effective amount of photosynthetic electron transfer at high light.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- The genetic approach to antenna reduction is sensible as a start, but the project investigator did not indicate or is unaware of the limitations of this approach in terms of practical solutions. The consequences on slower growth rate were not mentioned, let alone discussed.
- This is a well-thought-out, highly targeted molecular approach that has yielded very clear results. Given the project's focus on regulatory elements, genome-enabled approaches (especially transcriptomics) could be more effectively leveraged to examine the timing of expression and relevant regulatory networks. However, this may be beyond the scope of the current project.
- The investigator used the right approach, which has been quite successful so far.
- The project involves the discovery and characterization of mutants that regulate antenna size. Three mutants have been discovered: one is completely characterized (at least in terms of physiology, though more mechanistic studies could be performed), another is partially characterized and the third awaits complete characterization. All three appear to be functionally relevant.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- The project investigator ran into some technical problems that diverted attention from the main focus. The actual data showing the relative change in rates for oxygen evolution for mutants (2) were minimal and had large errors. No data showing a comparison of rates to optical absorption were discussed, even though this was

the primary motive of the work. No hydrogen data was shown, so the effectiveness to hydrogen production was not apparent.
- The project has made impressive progress and significantly exceeds many key DOE targets in terms of the overall improvement of light use efficiency. Although there have been some issues regarding specificity of the antibodies raised against the tla1 protein, problems of this nature are not uncommon, and the investigators appear to have successfully resolved the issue. The project has generated two publications thus far (one review and one on primary results) and, based on the work presented, several more are likely to follow.
- The investigator did excellent work in a progressive manner from mutant tla1 to mutants tla2 and tlaR, moving toward the DOE project goal.
- Progress in the project is excellent. The antenna size has been decreased, mutation by mutation, from over 600 chlorophylls to close to the theoretical size limit of just under 150 chlorophylls. Astoundingly, this has had almost exactly the predicted effect on both the rate of photosynthetic electron transfer at high light intensity and the resistance of the organisms to photoinhibition. This project is on schedule to meet all of its goals. The mutations are being used now by others both in industry and at NREL for DOE-funded research in hydrogen production.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- The team does not have any collaboration; the project is working independently. The absence of data, as noted above, is a shortcoming. The project investigator needs to provide data demonstrating the effectiveness of the mutants and find collaborators, which should be easy since he is working at the largest bioenergy laboratory in the country.
- As stated by the lead investigator, this is a single-source project with no significant outside collaborations. If collaboration was not a requirement at the time of the project's original selection for funding, it seems somewhat unfair to give it an evaluation of "poor", but this is the only available option in the grading system though the reviewer would prefer the option of giving a "not applicable" evaluation in this case.
- If possible, it would be better to make the other antenna mutants, such as tla2 and tlaR, available to the scientific community. In this way, the question of whether any of these antenna mutants can help photobiological hydrogen production could be tested by independent laboratories, which is essential as in any other scientific research.
- While it is clear that the specific goals of this work are contained within the project investigator's laboratory, it would be good to see more formal collaboration in the characterization of the mutants. Characterization could be tested both in terms of mechanism of the proteins involved and in terms of the efficacy of the mutations in a real-world application. Some of this is happening without formal collaboration, which is great, but the rate of implementation could be increased with a more concerted effort in this regard.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The project has a minimal description of priorities. It is difficult to know what the project investigator will do next or what his understanding is of the problems that need to be overcome.
- Since the project is nearing the end of its funding period, it is somewhat difficult to assess future work. Having said that, the investigators have identified a promising new avenue of research with tlaR mutants and would no doubt apply many of their previously developed techniques to characterize the functional properties of this protein and its role in antennae formation.
- The proposed future work is mainly to finish up with mutants tla2 and tlaR, which looks reasonable.
- The remaining work is clear in terms of characterization of the mutants and is well laid-out.

Strengths and weaknesses

Strengths
- The project did well in the creation and screening of antenna mutants.

- There has been impressive progress from a highly productive research group. In addition to its relevance in photobiological hydrogen production, the results of this work will have very clear relevance to considerations of algal systems for bioenergy production, regardless of the target fuel compound.
- This reviewer in general agrees with the comments made by last year's reviewers: the investigator has demonstrated superior progress toward well-defined goals; the investigator has a collection of mutant organisms with truncated antenna sizes and, based on these mutants, novel genes were identified; the outcomes reveal how chlorophyll antenna sizes are regulated for the first time in Chlamydomonas; the investigator's laboratory is well equipped to determine chlorophyll antenna size; the team has a good project investigator and demonstrated accomplishments; this project has a well-defined problem to solve that is directly applicable to large scale growth; the project team has demonstrated that mutations that result in decreased antenna size can be found and can also improve photosynthetic efficiency and stability to high light levels; and the investigator is an expert in photosynthetic systems of Chlamydomonas.
- The greatest strength of the work is the systematic manner in which mutants have been developed, characterized and shown to behave in predictable ways.

Weaknesses

- The project had weaknesses with its limited characterization of mutants, old recycled data, a lack of independent corroboration of data, and no established collaborations established.
- This reviewer in general agrees with the comments made by last year's reviewers: even though mutants with truncated antenna sizes displayed higher photoconversion efficiency (oxygen evolution), it is not yet determined if hydrogen production is similarly improved; the project has insufficient data and descriptions on advanced mutants; ultimately, this project needs to be coupled to hydrogen production values in order to be critically evaluated; no data was presented to show increased hydrogen production in the tla1, tla2, and tlaR mutants; and the investigator should have made the tla1, tla2, and tlaR mutants fully available to the scientific community for independent tests.
- There are no serious weaknesses in this project. As stated above, more formal collaborations with industry and other laboratories could now be formed to accelerate the pace of implementation.

Specific recommendations and additions or deletions to the work scope

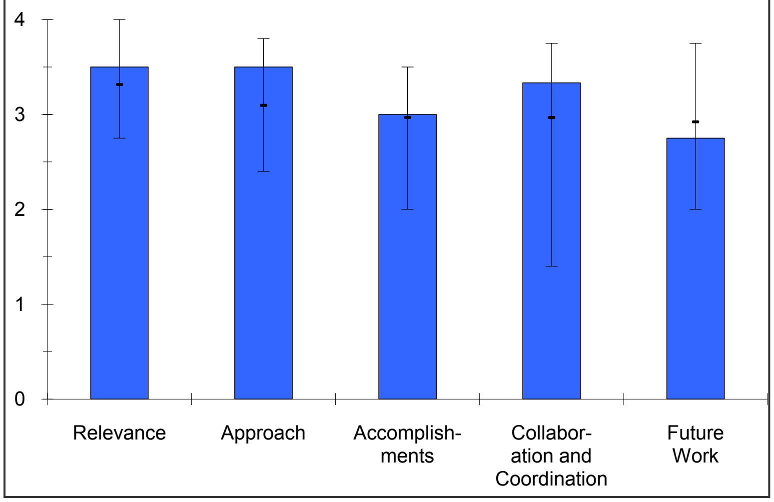
- If the project were to receive renewed funding, it would be significantly strengthened by establishing partnerships with groups more focused on photobioreactor design and evaluation. These partnerships would provide an important test bed for generated strains and allow the investigators to examine potential scaling issues.
- As stated above, expansion of the genome-enabled approaches would benefit this approach.
- One general concern is that, based on growth rate and other properties, these mutants would presumably be at a significant competitive disadvantage relative to wild-type algae and other phototrophes. Although it may be beyond the scope of this project, the question of whether photobioreactors can be maintained over time, with engineered algal strains remaining the dominant population, would seem to be an important consideration in scaling and eventual deployment.
- Overall, the investigator has made excellent progress with a quite reasonable project scope. This project effort should be funded when possible if the investigator could agree to make the antenna mutants such as tla2 and tlaR available to the scientific community.

Project # PD-37: Biological Systems for Hydrogen Photoproduction
Maria Ghirardi; National Renewable Energy Laboratory

Brief Summary of Project

The overall objective of project is to develop photobiological and integrated photobiological/fermentative systems for large-scale hydrogen production. Task objectives are to 1) address the oxygen-sensitivity of hydrogenases, which prevents continuity of hydrogen photoproduction under aerobic, high solar-to-hydrogen (STH) conditions; 2) utilize a limited STH hydrogen-producing method (sulfur deprivation) as a platform to address other factors limiting commercial algal hydrogen photoproduction; and 3) integrate photobiological and fermentative systems in different configurations for less costly hydrogen production in the short term.

Overall Project Score: 3.2 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- All tasks have high relevance to the DOE Hydrogen Program mission.
- The project's objectives are highly attuned to the DOE Hydrogen Program objectives and this was clearly described in the presentation materials.
- The main goal of the NREL photobiological hydrogen project is to create an oxygen-tolerant hydrogenase mutant, which (if successful) could be a very helpful component for photobiological hydrogen production to work well in the long term.
- The three tasks address important elements of both continuity of biological hydrogen production and feedstock costs. The first task is the oxygen sensitivity of the hydrogenase. The second task is working with the sulfur depravation approach to increase product yield in immobilized systems, in particular looking at ATPase mutants that all proton leakage and reduced antenna size mutants. The final task is integrating photobiological and fermentative organisms to improve the total yield of hydrogen. Each task has shown progress towards reducing risk though some have more than others, as described below.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- The project approach uses a combination of the development of previous research and development and some very clever new ideas.
- The research approaches (discussed in more detail below) are clearly focused on addressing key barriers in the development of scalable hydrogen production using phototrophic organisms.
- The project uses a rational scientific approach of using computer simulation of hydrogenase coupled with site-directed mutagenesis, although there has been no luck so far in the team's attempts to create a desirable oxygen-tolerant hydrogenase mutant. The expression of an oxygen-tolerant bacterial hydrogenase in Chlamydomonas is a reasonable alternative approach. The team also has a good approach of creating fusion between hydrogenase and ferredoxin to improve reductant flux to the hydrogenase.
- The first task met some difficulty. The hydrogenase mutants designed to block oxygen from entering the active site actually had the opposite effect, presumably due to folding issues. This aspect of the project now awaits further modeling to determine if it is possible to design mutants that can both block oxygen and avoid protein

destabilization. The reviewer wonders, in fact, if design mutants to stabilize the protein might have the desired effect, simply because it may be the protein dynamics that best lends itself to oxygen entry. The focus of task 1 has turned to the expression of a bacterial oxygen-tolerant hydrogenase in Chlamy. The constructions have been made and inserted, but it is not clear if this has resulted in protein expression or activity at this point and additional work is ongoing to understand the limits to expression. Finally, progress has been made towards creating a system for random mutagenesis, including an expression system for hydrogenase in E. coli that has been developed. This system will be used to screen libraries of random mutants for oxygen tolerance. A special high-throughput assay has been developed. While there were no home runs in task 1, each of the subtasks has shown progress or answered an important question about feasibility and therefore risk assessment.

- Task 2 has shown more progress. In particular, the ATPase mutants designed to allow some leakage of protons in the chloroplast, and thus keep the gradient from limiting the rate of photosynthetic electron flow, appear to have had a positive effect on overall hydrogen production on the order of a third greater. This is both an interesting scientific observation, as it suggests that the rate of the photosynthetic electron transport chain really is rate limiting as expected, and it provides some hope for further improvement of the hydrogen production rate by increasing photosynthetic rates. They also took advantage of the mutants being prepared by Mellis (also part of this program) that decrease antenna size and thus allow denser cultures at higher light levels. Again, the rate of hydrogen production was clearly much higher in these mutants, particularly in the early time after sulfur depravation. The fact that this works as predicted suggests that we are really beginning to get a handle on what the limiting issues are, which bodes well for further improvement. Overall, these are probably the most interesting and potentially useful results in this project.
- Task 3 also showed some promising results. In this task, the concept is to couple biomass utilization with algal hydrogen production. By fermenting biomass to acetate and related materials, the biomass is available for uptake by photosynthetic organisms that can use this as an energy source, along with light, for making hydrogen. Apparently the two separate reactions, acetate with the photosynthetic organisms and fermentation of potato wastes to form organic acids (and to make hydrogen), have both worked. It remains unclear whether there is really any great advantage of using photosynthetic organisms rather than just making the hydrogen anaerobically, given the complications of having to illuminate the cultures and the oxygen made by the cultures interfering with hydrogenase action, but the concept is interesting.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Multiple types of data were presented, which indicates a high degree of characterization. One issue could be the limited value in some proof of concept ideas like hydrogenase-ferrodoxin fusion that would be unlikely to be applicable in vivo or lead to a practical in vitro system, in the reviewer's opinion.
- For Task 1, it is difficult to assess progress due to recent shifts in objectives. Expression of alternate hydrogenases, such as Chlamydomonas, is at a very early stage of development, and although the goal of developing hydrogenase-ferredoxin fusions seems interesting and relevant to Hydrogen Program goals, it is unclear how this will be pursued further at NREL. If funding is not available, will this be handed off to the Massachusetts Institute of Technology? Other subtasks appear to be at transitional stages, and further study is needed to assess potential impacts.
- For Task 2, the studies on immobilized algal films have yielded interesting results on the tradeoff between optimal hydrogen production rates versus oxygen protection. These studies should inform the development of future production platforms as the work moves forward. The construction of ATP synthase is at too early a stage to meaningfully assess progress.
- For Task 3, the project has made good progress in terms of optimizing conditions for hydrogen production by bacterial consortia and determining whether growth on algal biomass would be feasible (the presentation implies that it is feasible). However, the real test of this system will be integrating the fermentative subsystem with the photobioreactor. Although it appears that connections have been completed, no further information was presented, but it would have been helpful to see even preliminary data. I am less clear on how the potato waste fermenter fits into this project; is this a necessary sub-element?
- Although there is clear progress on the various subtasks, there are many points listed where project elements are either a transitional stage or have been just been initiated, making it somewhat difficult to assess the progress of the project as a whole.

- Overall, this project is making quite reasonable progress, although the project team so far (for nearly 10 years, since FY 00) seems unable to create a desirable oxygen-tolerant hydrogenase mutant. This probably just reflects the challenging nature of the research project. This reviewer believes the project investigator and the team are quite talented and are working hard with decent effort. Therefore, their effort should be appreciated. They are making an effort also in somewhat different areas, such as the sulfur-deprivation platform and other research aspects.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The team has multiple effective collaborations.
- Although there are extensive collaborations with other institutions, it is not always clear how these fully integrate into the project's overarching objectives or future goals, which may contribute to the somewhat scattered sense of objectives (discussed in more detail below).
- The collaboration of the NREL team with other institutions is excellent. It seems intended to reach out to others in the field and rightly so.
- The team has significant and viable collaborations on each task. It was particularly nice to see the collaboration between this group and Mellis on the antenna mutants, as it points to the importance of interactions between DOE-funded researchers in this area.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Multiple tasks are planned with a clear justification for most, though one issue is how the priorities are established. There was limited justification in terms of quantitative expected improvements for each task. The reviewer questions, if fewer funds were available, which tasks would be prioritized first and why.
- Several of the sub-tasks are at critical points where viability will need to be demonstrated in the coming year. Although the majority of the specific research objectives are appropriate, the reviewer would have liked to hear more discussion on how effort will be directed if a given element does not perform to expectations.
- The proposed future work looks reasonable.
- For task 1, some hard decisions need to be made, particularly about moving forward with the site-directed work, but also about the likelihood that a truly random mutagenesis approach is going to achieve the kind of results desired. This task could simply go on forever trying to determine what might be a better approach without the ability to make some sort of clear prediction based on structure/function understanding. While the transfer of the oxygen-tolerant bacterial gene is a possibility, there are many complexities in such a system that go well beyond just the hydrogenase and it is difficult to see how the different factors are going to be sorted out.
- As described above, task 2 is making better progress and the proposed studies seem more logical.
- Task 3 is confusing to this reviewer. The team needs to determine what value is added from using photosynthetic organisms if the only real objective is to go from potato starch to hydrogen. It seems that other non-photosynthetic organisms would be better suited for this purpose and would be much easier to grow.

Strengths and weaknesses

Strengths

- The team has a strong record of accomplishments, publications, invitations, and scholarship. This is an interactive group that educates the community and shares information to advance the field.
- The project is clearly attuned to the goals of DOE's Hydrogen Program and nicely leverages resources available at NREL and the collaborating institutions.
- This reviewer in general agrees with the comments made by last year's reviewers: this is a long-term project that is relatively well funded and has many partners; the project team is exploring the fundamental understanding of hydrogenase structure and function, particularly with regard to oxygen tolerance; and the team is also characterizing the conditions that allow hydrogen production, particularly when some oxygen is present.

- The major strengths of the project have been in better defining some of the parameters that can be productively changed. It appears that decreasing oxygen sensitivity by blocking oxygen pathways through design is more complex than expected. On the other hand, making it possible for the photosynthetic system to run faster does seem to increase hydrogen evolution rates, as does decreasing the antenna size, and this is valuable knowledge.

Weaknesses

- The status of attaining hydrogen production benchmarks was not made clear. It was also unclear how dead-ends are identified (i.e., go/no-go decisions). It seems that some projects get carried forward with no closure.
- The project's tasks and subtasks, although individually interesting and appropriate, in many cases seem scattered across a wide range of goals and objectives. It is sometimes unclear how these subtasks form an integrated project where the whole is greater than the sum of the parts. This should not be read as a criticism of the newly developed project elements, which are interesting and appropriate (and were apparently requested in a previous review). However, as the project moves forward, it would be appropriate to evaluate the performance of various subtasks (both new and old) and redirect effort and resources to a more streamlined set of well-integrated goals.
- This reviewer in general agrees with some of the comments made by last year's reviewers: progress in this project seems slow or at least unclear. For example, the team has been working on developing high-volume throughput screening tests for several years and it is not clear what progress has been made. Other comments from last year include the following: the challenges and progress should be better identified; there is no indication, at this point, how this work could be scaled and no real understanding of how to increase the hydrogen production to a point that will be useful at scale; and this project is early stage exploratory work, which is not a weakness, but it makes extrapolation to any kind of useful system essentially impossible.
- The logic associated with task 3 is a bit unclear. They need to quickly answer the question about whether involving photosynthetic organisms in this case really makes sense.

Specific recommendations and additions or deletions to the work scope

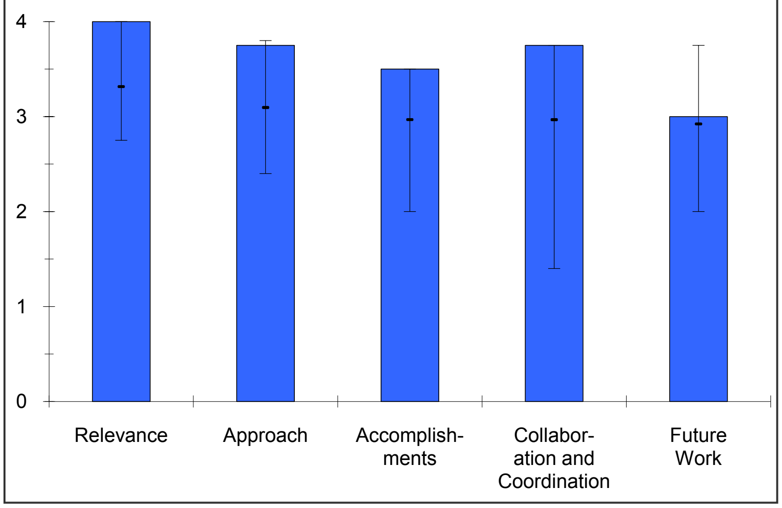
- Overall, the project team has made a reasonable effort towards the project goal and should receive continued funding support if possible. The NREL team effort should continue to focus on achieving the main goal of the project (i.e., to create a desirable oxygen-tolerant hydrogenase mutant), which is really what they were funded to do. Although the team's approach of computer simulation with site-directed mutagenesis seems to not be working, expression of an oxygen-tolernant bacterial hydrogenase in Chlamydomonas is a likely alternative pathway to achieve the goal (especially since the NREL team has already worked out some of the required accessory genes to express a functional hydrogenase, so this is likely doable). It could be viewed as a sign of failure to jump on something else without achieving the major project goal. Therefore, the project team should make sure to achieve the main goal of the project before spending time on something else.
- It is worthwhile to note that photobiological hydrogen production research is a quite challenging and long-term research and development area, which may or may not fit the time scale of a typical EERE program. Program-wise, the currently EERE-funded projects are not sufficient to address the (at least) six known problems (technical barriers) in algal hydrogen production. As reported in 2010 U.S. Patent Number 7,642,405 and PCT application number WO 2007/143354, these six problems are proton gradient accumulation, carbon dioxide inhibition, bicarbonate binding requirement, electron-drainage by oxygen, hydrogenase oxygen sensitivity, and oxygen/hydrogen separation and safety issue. Without solving these six problems, the photobiological hydrogen production technology will not work. The EERE photobiological hydrogen program seems to need additional innovative project teams to overcome these technical barriers. Perhaps this type of project effort is better to be supported through DOE's Office of Basic Energy Sciences program, where the program funding is relatively more stable, so that the investigators such as the NREL team could focus on their research rather than worrying about funding year by year.
- It is time to take a hard look at task 1 and consider reallocating personnel and resources to the other tasks, particularly to task 2.

Project # PD-38: Fermentation and Electrohydrogenic Approaches to Hydrogen Production
Pin-Ching Maness; National Renewable Energy Laboratory

Brief Summary of Project

The overall objective of the project is to develop direct fermentation technologies to convert renewable, lignocellulosic biomass resources to hydrogen. Task goals are to 1) determine effects of substrate loading on rates and yields, 2) develop genetic tools to improve hydrogen molar yield, and 3) develop a continuous flow microbial electrolysis cell reactor to improve hydrogen molar yield.

Overall Project Score: 3.6 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- All tasks have high relevance to the DOE Hydrogen Program mission.
- The project's objectives are clearly aligned with specific technical goals of DOE's Hydrogen Program. The emphasis on conversion of lignocellulosic plant material lends additional relevance to current DOE efforts in the development of biomass feedstocks for bioenergy production. The collaborative development of fuels cells for the conversion of organic acids produced during primary fermentation addresses one of the key barriers of fermentative hydrogen production (i.e. comparatively low hydrogen yield).
- The objective of the project is to develop direct fermentation technologies to convert renewable, lignocellulosic biomass resources to hydrogen, which addresses feedstock cost and hydrogen molar yield barriers to improve the techno-economic feasibility.
- The work is highly relevant to hydrogen yield, waste accumulation, and feedstock costs (particularly the first objective). The ability to directly process lignocellulose sources with a high hydrogen yield is very valuable.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- The project has a technically sound approach that complements other work going on elsewhere.
- At each step, the approach to accomplish the stated objectives of the project is well thought-out and clearly justified. In some cases, more foundational work (such as the development of better genetic tools for relevant hydrogen producing microorganisms) is being pursued, but this is reasonable and necessary to achieve the long-term goal of increasing hydrogen production. The project has an appropriate focus on mixed culture hydrogen production systems operating under high temperatures given that these systems should provide greater flexibility in the conversion of a range of substrates. The production systems should also be more robust in maintaining hydrogen-producing community structure during growth on necessarily non-sterile feedstocks.
- One of the approaches is to use corn-stover lignocellulose and cellulose-degrading bacteria to address feedstock cost. The second approach is to redirect metabolic pathways to maximize hydrogen production via the development of genetic methods. Another approach is to improve the hydrogen molar yield (mole hydrogen/mole hexose) by integrating dark fermentation with the microbial electrolysis cell (MEC) reactor to convert waste biomass to additional hydrogen.
- The combined approach of enhancing the fermentation process and coupling this with MEC technology is novel and has been very productive. Task 1 focused on adapting the bioreactor technologies for the direct fermentation of corn stover and Avicel, which are both available at NREL. In particular, Clostridium thermocellum has been employed in this work with significant success. Task 2 has focused on improving the

Clostridium metabolism of cellulose through genetic engineering. They have started by investigating the effects of known inhibitors on the hydrogen production. In this way, they have identified blocking acetaldehyde production as a major contributor to increased production. Inhibiting formate production from pyruvate also gave a significant improvement. The development of a genetic system is in progress. They have managed to find appropriate growth conditions on plates and locate an appropriate plasmid for expression. Initial conjugation attempts from E. coli were unsuccessful, and they are now using S-17-1 E. coli to attempt conjugation again. This strain has worked well with related photosynthetic organisms like rhodobacter and thus is a promising avenue. Task 3 is being run in Pennsylvania State University (PSU) laboratory and involves processing the material left over after fermentation using an MEC. In this case, a voltage is applied to provide a driving force and the hydrogen is evolved at the cathode of the cell. Bacteria at the anode serve to metabolize the material in the waste stream and provide the electrons to the anode. The integration of these methods appears very promising.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- Multiple types of data were presented, which indicates a high degree of characterization. The project investigator exhibits high versatility in working on both biomass conversion and molecular biological approaches. Co-culturing work has some practical limitations, which the investigator did not make clear, and the results indicated limited improvement in hydrogen yields thus far. It is still worth pursuing, but predictions of the expected gains to be realized with co-culturing remain unclear and prior literature results were not made apparent. There appears to be some good progress on the CBS expression.
- There has been very good progress on Task 1. It will be important to further development of the system to make a determination of hydrogen production characteristics under different substrate loading conditions and with varying microbial community compositions (small caveat on the latter, see below).
- Task 2 presents some challenges in terms of developing improved transformation systems for C. thermocellum. The electroporation approach comes with its own difficulties (especially for anaerobes). This task appears to be at a transitional phase, and more results will be needed to meaningfully assess the progress. The integration of this task with the work in tasks 1 and 3 is a bit more tenuous.
- Overall, this project seems to be making very good progress, including the following: determined the effects of substrate loading on hydrogen molar yield and rates; established a co-culture (C. thermocellum and a Clostridium consortium) and improved substrate utilization (both hemicellulose and cellulose); performed a hydraulic test and achieved steady hydrogen performance in the reactor using a continuous flow system; and achieved up to 0.53 m^3/m^3-d at a cathode surface area of 0.15 m^2/m^3.
- Task 1 has met its objective of measuring the molar stochiometry between hydrogen evolved and hexose units consumed. Molar yields in the 1.5 region were generally detected. Task 2 has also met its twin objectives of first using known metabolic inhibitors to determine which pathways to genetically engineer and then developing and evaluating some of the basic tools required for genetic engineering. That said, conjugation has not yet been demonstrated, although the team seems to be taking a reasonable approach to developing a conjugation system in that the plamids and E. coli strains being used have worked in similar cases previously. Task 3 has shown considerable progress in that the PSU group has been able to demonstrate that, using the effluent left over from lignocellulose processing at NREL, they can obtain almost seven moles of hydrogen per mole of hexose, bringing the total to over eight. What is not clear in what was presented is exactly how scalable this is. During questions at the presentation, it became clear that it takes roughly a day to clear one of their reactors, which could ultimately be the rate limiting aspect of the system.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- The team has multiple effective collaborations. The project investigator seeks input from other experts when needed to advance the project.

- The collaborative aspects of this project, including those being conducted with other groups supported by the DOE Hydrogen Program, appear to be well coordinated and are consistent with the overarching goals of the work.
- The collaboration of the NREL team with other institutions is excellent. They seem to intend to reach out to others in the field, and rightly so.
- Obviously, there is an interaction between NREL and PSU. In addition, there are collaborations within NREL to provide the biomass sources, and there is a collaborator in Canada helping on the development of the genetic system.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- One issue is how the priorities are established. There was limited justification in terms of quantitative expected improvements for each task. The team should determine which tasks would be prioritized first if fewer funds were available.
- The future work that was presented represents logical next steps that build on current efforts. The reviewer has some concerns regarding the lack of plans to perform any characterizations on the microbial consortia being used in this project; this is more fully described below.
- The proposed future work looks quite good.
- For each of the tasks, the proposed work seems generally appropriate. Task 1 appears to be mostly complete. For task 2 the big issue is whether they will be able to generate a working conjugation system for Clostridium, and the plans for doing this seem reasonable. Task 3 is also essentially complete. This was a bit confusing as the downloaded presentation did not specifically call out future work to be performed.

Strengths and weaknesses

Strengths

- The project investigator is highly collaborative, interactive, and technically versatile.
- This is a generally well considered approach that addresses many of the major challenges in the development of fermentative hydrogen production.
- This reviewer in general agrees with the comments made by last year's reviewers: the team has meaningful milestones, which indicate substantial thought being put into the process and, as a result, the team has developed a plan to achieve its goals; the project's integrated process better utilizes the feedstock; the project has a very good combination of novel fermentation with MEC technology; and the project results in terms of molar yields are quite impressive.
- The project demonstrates good integration of fermentation and MEC approaches and strong collaboration. Good progress has been made towards improving hydrogen per hexose ratios.

Weaknesses

- Fewer reviews and more primary research publications are needed, particularly on the expression work.
- This reviewer pretty much agrees with some of the comments made by last year's reviewers: this project requires a relatively expensive feedstock; maintaining the feed mixtures will be a significant challenge for this team; hydrogen gas produced could be consumed by methanogens, which could be a show stopper; MEC requires external supply of electricity and the energy efficiency of the MEC system is not very clear; and it would be good for the team to start working on predicting the effects of scaling output and considering the roadblocks involved in the scaling process.
- Future work plans were not very clear.

Specific recommendations and additions or deletions to the work scope

- I find the undefined nature of the microbial consortium being used here somewhat problematic. Given the amount of effort being put into developing this as an inoculum source for biomass conversion bioreactors, having some information on community composition would give a better sense of metabolic potential, likelihood of ability to convert different carbon substrates, presence of strict versus facultative anaerobes, and ability to determine if organisms performing competing processes (e.g., spore-forming acetogens, heat resistant methanogens) remain in the culture. Conducting a 16S-based phylogenetic characterization of the consortium should be relatively straight forward, especially given the limited microbial diversity likely to present in the sample after the heating enrichment step to select for spore-forming organisms.

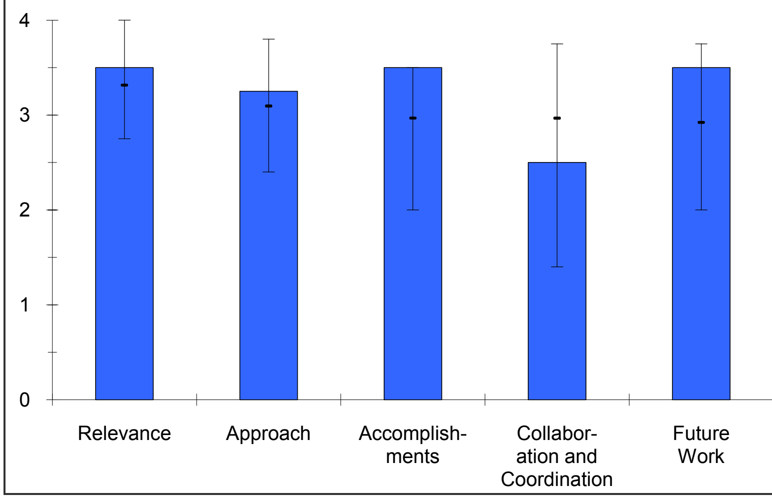
- Given the recent emphasis on development of dedicated biomass feedstocks such as switchgrass and poplar, it may be appropriate to consider these as potential carbon sources for future testing.
- Overall, the project team seems have made some very good progress towards the project goal and should continue to receive funding support to explore the work if possible. The MEC part is quite interesting, but it needs to provide clear and objective analysis on the energy efficiency since it consumes electricity. Economic assessment of the proposed process is another issue. They need to also pay attention to the issue of methanogens that could eat hydrogen because sterilization of large amounts of waste biomass for energy production would unlikely be feasible.

Project # PD-39: Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacterial System
Qing Xu; .J. Craig Venter Institute.

Brief Summary of Project

The overall objective of project is to develop an oxygen-tolerant cyanobacterial system for continuous light-driven hydrogen production from water. The target for 2011 is to produce one cyanobacterial recombinant evolving hydrogen through an oxygen-tolerant nickel-iron-hydrogenase. The target for 2018 is to demonstrate hydrogen production in air in a cyanobacterial recombinant.

Overall Project Score: 3.4 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Most tasks in this project are relevant to the DOE Hydrogen Program mission.
- The project effectively addresses a key barrier to biological hydrogen production in microbial phototrophs, namely the oxygen sensitivity of hydrogenase enzymes.
- The goal of this photobiological hydrogen project is to develop an oxygen-tolerant cyanobacterial system for continuous light-driven hydrogen production from water.
- Finding new oxygen-tolerant hydrogenases and transferring them into cyanobacteria is highly relevant to DOE goals in terms of the continuity of hydrogen production.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The project approach involves a heterologous expression of an oxygen-tolerant hydrogenase in a model host. The same approach is underway in many laboratories with limited success, which leads to the conclusion that new ideas are needed.
- This project attempts to address two barriers to hydrogen production by the cyanobacterium Synechococcus sp PCC 7942 through elimination of the putative uptake hydrogenase present in the wild type and provision of more oxygen tolerant hydrogenase derived from a different organism. The selection of Synechococcus sp PCC 7942 as a platform is reasonable, given the sequenced genome and availability of genetic tools for this organism (with a caveat noted below). The nickel-iron hydrogenase of Thiocapsa roseopersicina has similarly been well characterized in previous studies. A parallel project with similar experimental logic is being performed by collaborators at NREL working on Synechocystis sp. PCC6803 and a hydrogenase from Rubrivivax gelatinosus.
- Although this approach is appropriate and reasonable from a research perspective, there is not a great deal of justification as to why these organisms in particular should be developed for hydrogen production or how they might perform in anything other than bench-scale conditions.
- The approach is to transfer a known oxygen-tolerant nickel-iron-hydrogenase from T. roseopersicinainto cyanobacterium Synechococcus sp PCC79423, to identify novel oxygen-tolerant hydrogenases through metagenomic analysis of marine microbes in the global ocean, and to transfer the hydrogenases into cyanobacteria.
- There are two tasks. The first is a parallel effort by J. Craig Venter Institute (JCVI) and NREL to transfer known oxygen tolerant hydrogenases into cyanobacteria. JCVI is transferring the hydrogenase of T. roseopersicina into a synechococcus strain and NREL is transferring the hydrogenase of Rubrivivax gelatinosis

into a synechocystis strain. Both efforts involve the expression of the gene followed by isolation and characterization of the hydrogenase and determination of the activity in vivo. All of these efforts seem like solid approaches.
- The second task is performed entirely by JCVI and involves discovering new oxygen tolerant hydrogenases from a broad sampling of oceanic species, as well as the cloning and expression of this system in cyanobacteria. This is a very ambitious but potentially high payoff approach to solving the problem and again involves the same basic types of processes as described above, with the addition of the initial screening to find likely candidates. Again, this all seems very solid.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- The evidence given was not always convincing. Qualitative data on hydrogen activity was reported and only in vitro assay is currently reported. The team could improve the quantitative determination of their activity.
- The team at JCVI has successfully introduced the necessary genes for hydrogenase expression and assembly into Synechococcus and is able to detect low-levels of activity in cell extracts. This has been accomplished for the nickel-iron hydrogenase gene from R. gelatinosus, as well as a hydrogenase derived from a marine metagenomic database. Although this is an impressive accomplishment (especially the latter), the reviewer remains concerned that expression has not yet been demonstrated in vivo. This may reflect improper assembly of the enzyme, incorrect presentation of the enzyme in the native cell environment, or any number of other factors that could prove extremely challenging to correct.
- Although the current results may represent 80% progress in terms of milestones, the success of the project very much hinges on introducing detectable activity into intact cells, and the reviewer would reserve judgment until this can be unambiguously demonstrated.
- The presentation of enzyme assay data in crude extracts was somewhat confusing and difficult to interpret; this should be presented in a much clearer fashion in the future.
- The project has thus far generated two publications of primary data (one released, one accepted) and has several others in preparation.
- Overall, this project seems to have made very good progresses including the following: at JCVI the genes of the Thiocapsa oxygen-tolerant hydrogenase were transferred into S. PCC7942 and activity from the heterologously-expressed hydrogenase was detected; a novel nickel-iron-hydrogenase was cloned from the Sargasso Sea environmental DNA, expressed in T. roseopersicina, and showed activity in the presence of low levels of 1% oxygen; and the genes of this novel hydrogenase were transferred into E. coli and Synechococcus, and activity from the heterologously-expressed hydrogenase was detected.
- The NREL team has developed two different expression systems, expressed at least three CBS hydrogenase subunits and one maturation subunit in Synechocystis, and purified a CBS native hydrogenase.
- The JVCI group first characterized the isolated hydrogenase, then cloned the required hydrogenase genes in cyanobacterium, expressed it, and demonstrated activity in the expressed strain. Similarly, the NREL group has cloned the genes, expressed them, and isolated the hydrogenase from cyanobacteria.
- The JVCI group has also identified a new oxygen-tolerant hydrogenase from their broad sampling of the ocean, which is a nickel-iron hydrogenase. They have cloned the needed genes and expressed them in roseopersicina. The genes were very heat tolerant and moderately oxygen tolerant. The entire gene cluster required was then cloned into cyanobacterium lacking hydrogenase, and weak but definite hydrogenase activity was demonstrated.
- This project has had excellent progress.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- It seems like the work at NREL and JCVI is done independently, rather than collaboratively. Different organisms are done at different sites, and evidence of true interaction is not so apparent. JVCI is not using NREL's extensive facilities.
- No results from collaboration with Kovaks at Szeged given.

- Although the collaboration with researchers at NREL was discussed in detail, the presented material suggested that this exists as a largely parallel project, and it was unclear whether there was a significant degree of integration and collaborative work being conducted between the two groups. Minimal information was provided on collaborations with the three other institutions, other than that they share a similar approach.
- The collaboration of the JCVI and NREL seems very good.
- The collaboration with NREL adds substantially to the activity with two opportunities for expression and comparison of oxygen-tolerant hydrogenase. In addition, they are collaborating with two other laboratories involved in the expression of hydrogenase.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- The arrival of a new person, Dr. Weyman, to replace Dr. Xu in leading this effort indicates a change in personnel that was not explained at the outset, could indicate a good future for new developments.
- As was mentioned above, demonstrating in vivo expression of hydrogenases in the non-native hosts remains the most critical milestone that this project must meet. The proposed future work is appropriately focused on this goal.
- The proposed future work looks reasonable.
- The future work revolves around both characterizing the strains with heterologous hydrogenases and improving them by adding additional genes and manipulating the metabolic pathways. These are sound approaches with significant promise.

Strengths and weaknesses

Strengths

- The project has strong expertise for metabolic engineering of this type, and the ability to effectively leverage metagenomic libraries is a significant advantage.
- The project has a well-defined work plan.
- JCVI has very good microbial (cyanobacterial) molecular genetics and genomics expertise to contribute.
- JCVI is making an excellent effort to identify novel oxygen-tolerant hydrogenases through metagenomic analysis of marine microbes from the global ocean.
- The project has made consistent progress in transferring oxygen-tolerant hydrogenases from several sources into cyanobacteria and demonstrating activity.

Weaknesses

- The strong genomics/bioinformatics capabilities at JCVI are not apparent in the results presented. The heterologous expression approach that is used is redundant with other efforts elsewhere that also have not yet been successful.
- The parallel nature of the collaborations seems to be a weakness, and the reviewer is unclear on why the project resources would be divided in this way. A clearer demonstration of integrative effort is needed.
- Expression of an oxygen-tolerant hydrogenase is unlikely to result in a practical cyanobacterial system for continuous light-driven hydrogen production from water, because the oxygen sensitivity of hydrogenase is merely one of the (at least) six known problems (technical barriers) in photobiological hydrogen production. For example, there is a very different type of oxygen sensitivity that has nothing to do with the hydrogenase, but sucks electrons away from the hydrogen-production pathways even at an oxygen concentration as low as 1000 ppm, whereas the in-vivo hydrogenase is completely active. Therefore, this electron-draining oxygen sensitivity (reported in 2003 Applied Biochemistry and Biotechnology, Vol. 105-108, pg 303-313) is a far more serious issue that needs to be addressed in photobiological hydrogen production than the oxygen sensitivity of hydrogenase per se.
- There are no significant weaknesses.

Specific recommendations and additions or deletions to the work scope

- If possible, localization of introduced hydrogenases within the cell could prove very informative.

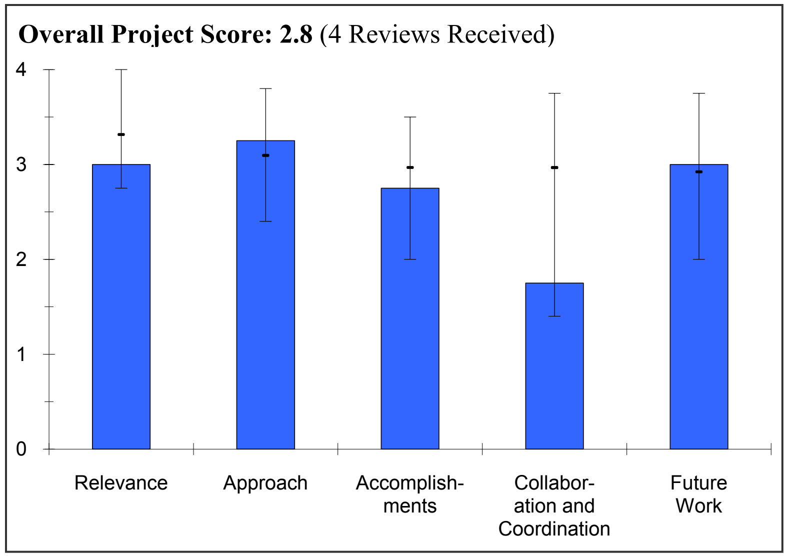
- Overall, the project team has made some very good progress toward the project goal and should receive continued funding support to explore the efforts, if possible. The involvement of JCVI could benefit the DOE photobiological hydrogen program by contributing to its expertise on microbial (cyanobacterial) molecular genetics and genomics. JCVI's effort on searching for novel oxygen-tolerant hydrogenases through metagenomic analysis of marine microbes from the global ocean is also potentially valuable.
- One must also understand that photobiological hydrogen production research is a quite challenging and long-term research and development area. Program-wise, the current EERE-funded projects are not sufficient to address the (at least) six known problems (proton gradient accumulation, carbon dioxide inhibition, bicarbonate binding requirement, electron-drainage by oxygen, hydrogenase oxygen sensitivity, and oxygen/hydrogen separation and safety issue) in algal hydrogen production. Expression of an oxygen-tolerant hydrogenase by the JCVI-NREL project would help solve only one of the six technical problems that are reported in 2010 U.S. Patent Number 7,642,405 and PCT application number WO 2007/143354 A2. Without solving the remaining five problems, the photobiological hydrogen production technology will not work. That is, the EERE photobiological hydrogen program seems to need additional innovative project teams to overcome these technical barriers. Perhaps this type of project effort is better to be supported through DOE's Office of Basic Energy Sciences program, where the program funding is relatively more stable so that the investigators, such as the JCVI-NREL team, could focus on their research rather than worrying about funding year by year.

Project # PD-42: Catalytic Solubilization and Conversion of Lignocellulosic Feedstocks

Troy Semelsberger; Los Alamos National Laboratory

Brief Summary of Project

The overall objective of the project is to develop novel low-temperature chemical routes and catalysts to produce hydrogen/syngas from lignocellulosic feedstocks. The 2012 target is to reduce the cost of hydrogen produced from biomass gasification to $1.60/gge at the plant gate (less than $3.30/gge delivered). By 2017, the target is to reduce the cost of hydrogen produced from biomass gasification to $1.10/gge at the plant gate ($2.10/gge delivered).



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The project aims to develop a low-temperature catalytic aqueous phase reforming (APR) process to produce syngas from lignocellulosic feedstocks, which is the type of biomass that is most available. The project is very challenging, but it may have an important impact in the future production of renewable hydrogen from the most abundant biomass and wastes.
- Hydrogen production does not have a very high yield from this process. However, it appears that they may be getting an insoluble phase of hydrocarbons, which could be a useful fuel. The researcher did not investigate this potential, and it may be worth revisiting some key tests. Lignin is a difficult feedstock to digest, and the concept that is proposed would be useful for integrating with a traditional digester.
- This project supports the programmatic goals to produce hydrogen from renewable sources at costs competitive with fossil sources.
- This project was not sufficiently funded to move the technology forward to address the barriers.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The low-temperature catalytic APR process has a number of advantages over the pyrolysis-based processes. The proposed research is directed towards solving the fundamental problems associated with the solubilization of lignocellulosics and hydrogen-selective APR.
- The approach could have been more thorough in analyzing yields of various species. The mass and energy balance was not pursued to a sufficient extent to track the full conversion of potential fuel species.
- The work accomplished is of high quality and the approach is well defined.
- The team has not been able to demonstrate if this technology can accomplish the stated goals, but the problem is due to funding constraints. The results look interesting and the chemistry appears to be feasible, but the results are very limited and the economic analysis is at too high of a level because there is not enough data to accurately represent the technology.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The project only had one year of work during which the team established an experimental apparatus and performed initial APR and solid phase biomass conversion with interesting observations in the chemistries of these systems.
- It is my understanding that the technical results shown are from previous years. It is not surprising that they have not made recent progress since they have not had any funding for 2009 and 2010.
- \Again, the reviewer would like to have seen a full spectrum of product species and any hydrophobic phases.
- For the work performed, the progress is good and the results are encouraging that this technology may be practical for certain applications.
- The issue with this project is that there is no indication of how much funding was provided against the original project budget. Only $500k was made available from 2007 to the present. The results of the work presented should have allowed it to proceed, but since the budget was cut it was not continued.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- The team does not have any partners.
- The reviewer did not see much, if any, collaboration.
- This program was done without significant collaboration.
- The team has is no collaboration, but it may have been because the budget was cut in 2009.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The team's system concepts and explanations of future work are very good.
- The future work looks interesting and logical. However, I am not sure if this is the right presenter for the work. The future work would propose to augment traditional digestion.
- The proposed work plan is reasonable based on the accomplishments. However, more bench-scale work should be carried out on actual wood, both hard and soft, to validate if the cellobiose is a good model compound.
- Due to the high cost of the catalysts in the preliminary economic model, the additional work should validate the rare earth options to cut the $19 million cost more quickly, and if it is not possible, then the project should not be continued.

Strengths and weaknesses

Strengths

- The project tackles one of the most important challenges in developing technologies for hydrogen production from renewable and low-cost biomass (i.e., the utilization of lignocellulosic feedstocks). The APR system proposed here is extremely challenging but, if successful, it would be a viable alternative to the traditional pyrolysis or high-temperature gasification methods.
- The team has a good vision of what they want to achieve.
- The project has a good research and development team at Los Alamos National Lab.
- The project uses a good technical approach that has not been attempted in the past.

Weaknesses

- The plans are reasonable, but currently there is not enough data to evaluate the technical approaches.
- While the team has a good vision of what they want to achieve, they have had very limited good results.
- The project does not have any teaming with universities or companies that may have more expertise on catalyst systems for aqueous phase reactions.

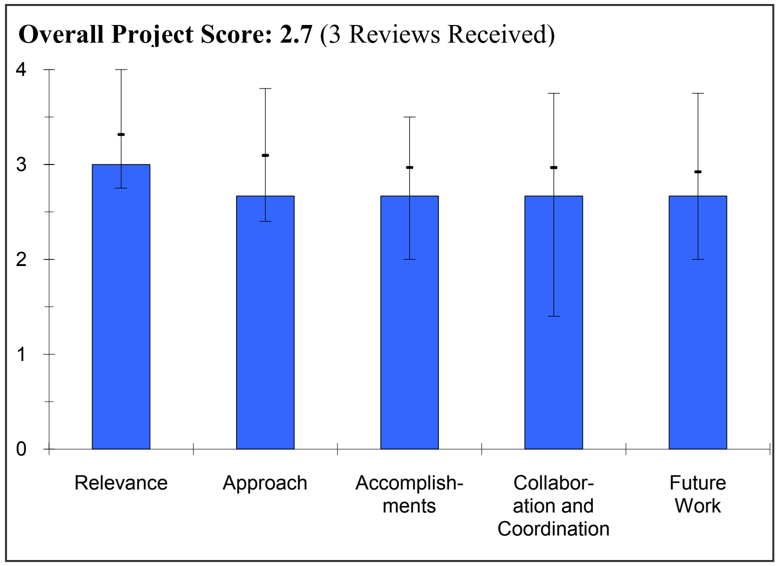
Specific recommendations and additions or deletions to the work scope

- It would be valuable to revisit some of the original tests and check for an insoluble phase in the product. The off-gassing of significant amounts of carbon dioxide and a little hydrogen would suggest that there could be hydrocarbon chains remaining after lignin digestion, which could be a useful fuel or chemical feedstock.
- DOE should continue the funding and establish a stage gate review once the project has achieved more data.

Project # PD-45: Distributed Reforming of Renewable Liquids Using Oxygen Transport Membranes
Balu Balachandran; Argonne National Laboratory

Brief Summary of Project

The overall objective for this project is to develop a compact, dense, ceramic membrane reactor that meets the DOE 2017 cost target of less than $3.00/gge for producing hydrogen by reforming renewable liquids. The reactor would use oxygen transport membrane (OTM) to supply pure oxygen for reforming renewable liquids. The initial focus on reforming natural gas was changed to ethanol reforming in FY 08. Objectives during the past year were to use OTM to reform ethanol at less than or equal to 700°C and generate data for detailed analysis to identify benefits of the approach.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The success of this project would support the DOE objectives for the DOE Hydrogen Program by reducing the costs and energy requirements for reforming of ethanol.
- Technology is a contributing factor to enabling a hydrogen economy, but not crucial.
- The project objectives are aligned with the DOE Hydrogen Program goals, and the barriers listed are also appropriate. However, considering the budget and the objectives, it appears to be an overly ambitious project.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The research approach for this project is logical and well thought-out. The approach addresses the technical barriers to reduce the capital costs for ethanol reformer technology, which increases efficiency, selectivity, and durability by using an oxygen transport membrane.
- The project plan is good.
- The approach is poorly defined. Other than stating the obvious, there is no clear definition of how the objectives will be accomplished.
- Oxygen transport membranes are well known, and there have been significant efforts in this area with multi-million dollar projects led by Air Products and Praxair. In light of this, it is not clear what is unique about the approach.
- The milestone description is very weak and– there are no quantitative measures that define the milestones. Simply stating "perform ethanol reforming studies..." does not constitute a milestone. It would be useful to have some targets in terms of conversion, efficiency, cost, and other similar metrics.
- On slide 8, no criteria are established for a go/no-go decision, e.g., a go/no-go decision is not possible just based on the "testing of catalysts."

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The accomplishments to date have been limited by the lack of funding. Note that this should not be considered a shortcoming of the researchers. It was not clear if the air stream will be pressurized to improve oxygen flux rates. The cost of increased air pressure verses the benefits may need to be detailed.
- The team needs to address steam effects because, in SMR processes, the steam ratio is one of the most important variables in determining costs and energy efficiency.
- The team's progress appears to be reasonable given the low funding level. However, several plots comparing OTM versus blank are unnecessary, as these are expected results and not novel. Also, the use of rather low ethanol concentrations (7-13%) does not serve any practical purpose.
- It would be helpful to present the data in the form of commonly used parameters that can be compared with other studies, such as conversion, instead of a production rate in cm^3/min, without mentioning the feed rates of ethanol and air.
- The comparison of different material compositions on slide 13 is meaningful. However, these flux results are not verified with the use of these membranes in ethanol reforming.
- The results of economic analysis are abruptly presented in slides 14-15 and cannot be appreciated or judged in the absence of any mention of assumptions with respect to membrane performance and cost.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- This project has well-developed collaborations with other institutions, including a potential end-user.
- Investigators should consider collaborations with commercial catalyst providers, as well as practitioners of steam methane reforming.
- The collaborations are reasonable at this stage. However, the interactions among them are not apparent and it is not clear from the results if any collaboration is leveraged.
- A key problem appears to be the absence of an industrial partner who can provide direction with respect to commercial aspects.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The proposed future work is clearly laid out and will address the technical barriers necessary to make this project a success.
- The proposed plan should work to acquire kinetic data and develop an engineering model of the process, which will allow the investigation of parameter space including space velocities (reactor size and cost), steam ratio, etc.
- The future work plan is stated in rather vague terms. It is first necessary to establish that the proposed approach can significantly reduce the ethanol reforming cost. This would help to establish specific performance targets and corresponding go/no-go decision points. At present the project seems to have been poorly structured.

Strengths and weaknesses

Strengths
- This project is very focused on improving the ethanol reforming process, and the experimental design clearly addresses the technical barriers.
- The project builds on strong OTM efforts and the team has good expertise in membrane fabrication.
- The team has good knowledge of OTM materials and past experience with synthesis and testing of such materials.

Weaknesses
- There has been a lack of consistent funding to date, which is not a fault of the researcher.
- The project does not address sealing issues. This is always a major issue in OTM and other high-temperature membranes.

- The team needs to clearly show the advantage of oxidative reforming over steam reforming. The investigator claims OTM advantages in methane reforming, but it has not been commercialized.
- The team has a lack of understanding of commercial aspects.
- The project has poorly defined objectives, targets, and milestones.
- There is an insufficient basis for the proposed approach and benefits.

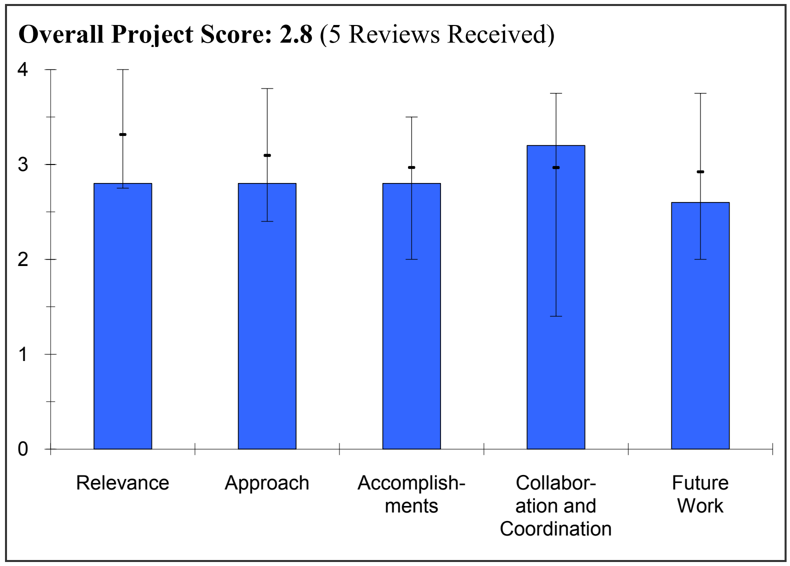
Specific recommendations and additions or deletions to the work scope

- The team should perform an engineering analysis (and supporting experiments) to show how much better OTM-assisted reforming must be in order to be economically superior to steam reforming. This may be a function of scale.
- As stated above, the project needs to first establish a clear baseline, improvement potential, specific targets, and a corresponding clear pathway.
- The work seems to emphasize material compositions to improve performance. Based on the prior work in this area, it is known that the real issues with this approach are with respect to fabrication, integrity, and durability, which are not addressed in the project.

Project # PD-46: Reversible Liquid Carriers for an Integrated Production, Storage and Delivery of Hydrogen
Alan Cooper; Air Products

Brief Summary of Project

The overall objective of this project is to develop a conceptual design and fabricate an initial 0.1 to 1-kW prototype of a dehydrogenation reactor/heat exchange system to deliver hydrogen via liquid carrier materials. The project is divided into three tasks: 1) liquid phase hydrogen carrier raw materials sourcing and processing, 2) develop a conceptual design and fabricate an initial 0.1 to 1-kW prototype of a dehydrogenation reactor/heat exchange system to deliver hydrogen, and 3) conduct an economic evaluation of the delivery and storage system for the liquid carrier hydrogen delivery concept.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- This program evaluates a novel approach where hydrogen is absorbed and desorbed from an organic liquid. The concept is similar to an oil change; change the system with fresh liquid, extract the hydrogen from the liquid, drain the spent liquid, then recharge with the fresh liquid. The concept, if viable, is intriguing. The process and the status of the activity is very clear.
- The reviewer is not sure this pathway shows the potential to achieve much lower delivery cost and widespread adoption.
- The project seems highly attuned to DOE Hydrogen Program objectives.
- This is very relevant, as delivery cost dominates the overall hydrogen fuel cost.
- The liquid organic hydrogen carriers (LOHC) concept can be a game-changer for the transportation hydrogen economy and infrastructure requirements.
- This process is important to enabling a hydrogen economy, but not critical.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The approach to this task is focused. It is unclear which activities are occurring on the to-be-determined working fluid. The presentation did not clarify if it is to be Perhydro-nitrogen-ethylcarbazole or if this fluid is being used as an example for the modeling.
- The reviewer found the approach to be interesting, novel, and highly focused on the development of solutions to long-term needs in hydrogen fuel distribution.
- The approach is fair, it demonstrates reaction productivity to meet the system volume targets.
- It would be better if the focus first is on identifying the LOHC that is cost effective and safe, as the key limitation for this concept is to identify a working LOHC. A model LOHC does not have benefit.
- It is better to directly collaborate with the hydrogen original equipment manufacturer (OEM), BMW, on a timeline and criteria for the demonstration and acceptance of this concept.
- The forecourt dehydrogenation solves only half of the problem and onboard hydrogen storage remains one of the key bottlenecks. It is better to focus on onboard dehydrogenation.

- Advanced reactor concepts are required to achieve high conversions with the high rate of gas generation. The novel reactors, such as the suspended slurry being investigated, have the potential to raise conversion.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The program seems to be adequately focused and directed.
- It appears that the testing and results have improved, though long-term benefits from the method may need to be evaluated.
- It seems that significant progress had been made on a challenging set of barriers.
- The team has not made much progress since last time. The only progress is test reactors with measurements of productivity.
- The work should be directed to focus on identifying a practical LOHC carrier as all of the reactor and other development applies to the current carrier, which is not practical.
- The projected delivery cost range is too wide. It should be more specific at this point.
- The team's progress is very good considering the short time the project has actually been funded in 2010.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The collaboration with other parties appears to be adequate.
- OEMs are critical to the adoption of this process and yet are missing from project.
- The project appears to be an effective collaboration between industry, academic researchers, and DOE national laboratories.
- The project has a good number of relevant partners.
- The reviewer would like to see a timeline by the OEM (BMW in this case) to deploy a test version in one of their vehicles.
- The team has a good list of collaborators. Pacific Northwest National Laboratory's experience in microreactors is a good complement to the investigator's expertise.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- The future work plan, while detailed, might consider additional ancillary issues that would need a plan to institute the concept.
- The exact future work for the project was not clear. The team needs a more developed plan of next steps and potential improvements.
- The reviewer found the logic of the proposed future work somewhat difficult to follow, but did not have an engineering background and would defer to more qualified reviewers.
- The project has a fair list of items for future work
- As mentioned before, future work should include deployment in the vehicle (through BMW).

Strengths and weaknesses

Strengths
- The concept appears sound, and the effort is working towards a set of functional requirements for the reactors.
- The project has good safety aspects and delivery simplification of hydrogen-to-vehicle from a production facility.
- The team has good partners with the right relevance and technical strength.
- Air Products and chemicals are the right choice for the lead in this work area.

Weaknesses

- The definition on the infrastructure details required to implement this concept appears to be lacking.
- It is unclear if there is an assurance of hydrogen quality for fuel cell vehicles. Critical project partners are absent, such as large OEMs (besides BMW).
- The project has a poor approach. They need to first identify and establish the candidate LOHC material, look at its economics, and then do the reactor development.
- The project needs better co-ordination with an OEM on vehicle deployment plans.

Specific recommendations and additions or deletions to the work scope

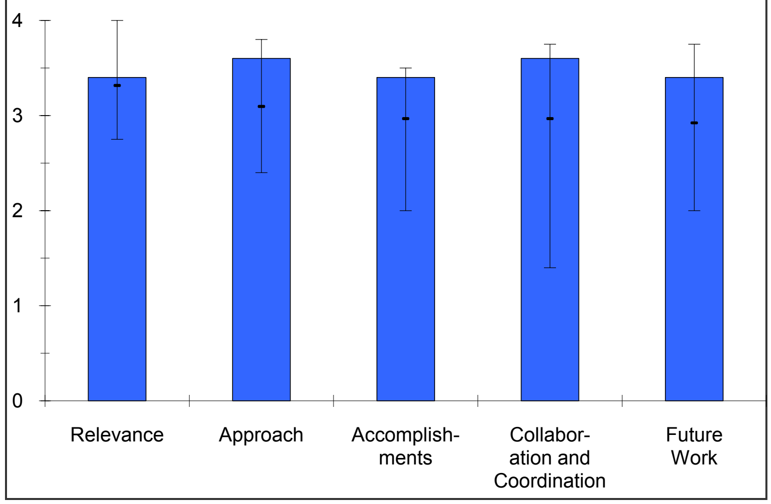
- The project should define what other things need to happen to institute this concept, for example, how to distribute, collect and reprocess the material.
- The project should examine the allowable toxicity of the working fluid and the personal protection equipment required to transfer the charged and depleted fluids.
- It is unclear if the general public will be doing the fueling, or if certified technicians will be required.
- The project needs to either bring on board more vehicle manufacturers, or consider working on a fleet application for this method. The project needs to have a more detailed analysis of the future cost advantages versus conventional (or proposed) hydrogen delivery methods.
- The project should search for a practical, safe and cost effective LOHC material.
- The team needs to establish the protocols and approach for material recycling at the station.
- Forecourt dehydrogenation should be deleted from the scope because it only solves half the problem.

Project # PD-47: Materials Solutions for Hydrogen Delivery in Pipelines
Doug Stalheim; Sacramento Emergency Clean Air & Transportation Grant Program

Brief Summary of Project

The overall objective of this project is to develop materials technologies to minimize the embrittlement of steels used for high-pressure transport of hydrogen. The objectives are to 1) identify steel compositions and processes suitable for construction of a new pipeline infrastructure or potential use of the existing steel pipeline infrastructure and 2) understand the economics of implementing new technologies. The objective to develop barrier coatings for minimizing hydrogen permeation in pipelines and associated processes is on hold per DOE.

Overall Project Score: 3.5 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Hydrogen pipeline materials are very relevant to the delivery of hydrogen over long distances. The reviewer would like to see work done on weldments, and not just unwelded material.
- The need to evaluate the effects of hydrogen embrittlement for existing pipeline steels is very important.
- The project seemed well attuned to DOE objectives.
- This project demonstrates important work on a metallurgical testing of steels for hydrogen pipeline service.
- This is a project whose objective is to deliver material microstructures that are hydrogen compatible. The reviewer thinks it is the only program in the DOE that explores the metallurgical aspects of the material microstructure relative to hydrogen embrittlement. Amongst four originally proposed microstructures, the project has down-selected two low-alloy steels that were found to exhibit the best performance in gaseous hydrogen tensile testing. One of these alloys, designated as steel B, is an X70/X80 polygonal ferrite with coarse acicular ferrite at 10%. The other steel microstructure, designated as steel D, is an X52/X60 polygonal ferrite. Fracture toughness results for these microstructures indicate that they are extremely resistant to hydrogen degradation up to pressures of 3000 psi with fracture toughness values greater than 80 $MPa*m^{0.5}$. The fatigue properties of these steels were also studied. The da/DN curves indicate that hydrogen increases the growth rates for both steels B and D in the intermediate stress intensity factor range. On the other hand, it seems that the near-threshold fatigue crack growth rates are similar to those in air. It remains to be seen how these hydrogen-induced crack growth rates are affecting the life of a pipeline. In summary, the project is showing outstanding progress toward ascertaining the hydrogen resistance of two promising material microstructures.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- The project is doing good, standard work.
- The project uses a tried and true industry approach to evaluating materials.
- Although the project is considerably outside of my area of technical expertise, the need to evaluate the potential of existing pipeline infrastructure to serve in hydrogen distribution seemed reasonable and was convincingly presented.

- The project is relating steel microstructures, as characterized by microscopy, to performance at high hydrogen pressures using American Society for Testing and Materials (ASTM) test procedures for tensile strength and fracture stress (e.g., the appearance of cracks).
- The project has been asking the right questions and, as a result, it concluded that the polygonal ferrite/coarse acicular ferrite (steel B) provides the best performance in high-pressure hydrogen environments. The latest tests on fracture toughness in gaseous hydrogen and fatigue crack growth are precisely the tests needed to ascertain the compatibility of these microstructures (steels B and D). The project is also planning to explore the relationship between performance and the fraction of acicular ferrite. This is indeed the right approach, as it will provide further insight into how we should tailor the microstructure to achieve better resistance.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- The project has some nice results. The reviewer would have preferred more metals to be included and some weldment tests performed. Without these additional data, one might carry out pipeline design based on limited test results.
- The progress to date is excellent.
- From admittedly a very limited number of samples, alloys of two microstructure types were selected as potentially the best performers.
- This work has provided an important knowledge base on the relationship between the pipeline steel's microstructure and its compatibility with hydrogen.
- The project has identified that the presence of acicular ferrite in low-alloy steels provides increased resistance to hydrogen degradation. In fact, the measured fracture toughness is very high (greater than 100 $MPa*m^{0.5}$ for alloy B) and this is an excellent result. The project also found that the fatigue resistance is independent of the fraction of the acicular ferrite (steels B and D). This is a result that needs further investigation given the fact that both steels B and D were found to exhibit increased crack growth rates in fatigue at the intermediate stress intensity factor range regime. The growth rates near the threshold stress intensity factor range are not different from those in air, which is an intriguing result that holds great promise for future developments.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- The team has some collaboration but nothing special, either good or bad.
- The collaboration to date is good. The team should discuss this project with the Department of Transportation (DOT) Pipeline and Hazardous Materials Safety Administration. There may be a synergy there that would identify additional alloys of interest and potential funding sources.
- The project exists as part of a well-integrated consortium of industrial, academic, and national laboratory personnel.
- The project has very close collaboration with a number of industry partners and academia for obtaining characterization data.
- The project's collaborations with the Sandia National Laboratories are outstanding. The project managed to obtain fracture toughness data and fatigue crack growth rates, which is a direct result of this collaboration.
- Collaboration with Oak Ridge National Laboratory (ORNL) is good, as long as it aims at addressing alloy development for enhanced hydrogen resistance, such as the effect of the acicular ferrite content. The in-situ tensile testing at ORNL, although a screening procedure, is not reliable for assessing materials against hydrogen embrittlement. By way of example, it is reported on slide #24 that for pressures greater than 800 psi alloy D is more resistant than alloy B under strain rates of 10^{-5}. This contradicts the fracture toughness results shown in slide #29 whereby alloy B is more resistant than D. The point is that hydrogen-induced loss of reduction in area in tensile tests is not a reliable index of embrittlement. In fact, for the same two alloys and pressures greater than 800 psi, the ONRL result at a strain rate of 10^{-4} is exactly opposite to the result at a strain rate of 10^{-5}.
- Collaborations with the American Society of Mechanical Engineers Codes and Standards division for the development of B31.12 is also good for the project because it helps place the project's results under a proper industrial perspective for pipeline design.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- They have a plan to fill in the needs, but unfortunately they plan to do this after the money runs out.
- The proposed work is very good. It would have been nice if the scope was larger, but it is likely limited by funding.
- The presenter mentioned that the project is in its final stage of DOE funding and is unlikely to be renewed. The future work presented seems a logical next step in securing future support from relevant private sources.
- The team proposes the examination of a greater number of samples. It is not clear what is meant by in-situ testing of the steels under hydrogen.
- The project's future work plan to investigate the relationship between embrittlement and the fraction of acicular ferrite is a very well-thought-out approach.
- It is an intriguing result that the fatigue crack growth rates at near-threshold stress intensity factor changes for steels B and D are the same as that in air. Its validity needs to be understood at various pressures.

Strengths and weaknesses

Strengths

- This is a good, solid project and well done. The team has decent collaboration and the project is relevant.
- This project has a clear, direct, applied science approach.
- The project is providing a valuable knowledge base on the effect of hydrogen on pipeline steels, which is important for any development of a hydrogen economy that will require the conveyance of hydrogen at much higher pressures than are currently employed.
- The strength of the project is the collaboration between Sandia National Laboratories and DGS Metallurgical Solutions. They both provide complementary expertise for the success of the project.

Weaknesses

- No effect on welds measured, and these are often the weak points. The steels that were looked at are fairly old school.
- The project had a lack of breadth of the testing. The reviewer would have liked to see a dozen or so alloys evaluated.
- The number of evaluated samples and microstructure types is still very limited for gaining a desirably broader knowledge base of the effect of hydrogen on pipeline steels.
- It is not productive to focus resources on tensile testing. The project should continue with fracture toughness testing and fatigue crack growth testing. The reviewer does not believe that reduction in area can be used to evaluate suitability for service, as mentioned in slide #43. In fact, as the project has found, and the reviewer discussed above, reduction in area frequently yields contradictory results to those from fracture toughness.

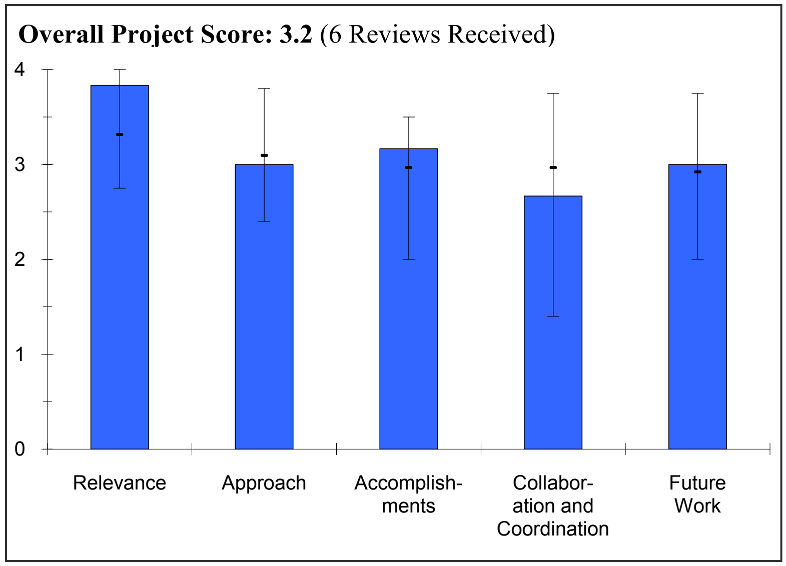
Specific recommendations and additions or deletions to the work scope

- The project should look at welds as soon as possible.
- There are a number of questions posed on other alloys currently used for hydrogen storage. It would be interesting if the scope of the task were extended to include the evaluation of existing DOT cylinders steel alloys. These alloys are being considered for fuel storage on mobile material-handling equipment like fork trucks.
- Given that hydrogen-induced corrosion of steels depends on a dissociation of the hydrogen molecule into H atoms, the team should consider if a hydrogen and deuterium isotopic exchange rate into HD (as can be readily followed by mass spectroscopy) could possibly be utilized as a diagnostic for the onset of such corrosion.
- The project should focus on 1) quantifying the effect of the acicular ferrite content on fracture toughness, and 2) why the presence of acicular ferrite is not influential on decreasing fatigue crack growths.
- The project should explore other possible microstructural details that can be tailored to enhance fracture toughness and fatigue resistance.

Project # PD-48: Development of Highly Efficient Solid State Electrochemical Hydrogen Compressor (EHC)
Ludwig Lipp; FuelCell Energy, Inc.

Brief Summary of Project

The objective of this project is to 1) develop designs and materials to increase EHC pressure capability from 2,000 to 6,000 psi, 2) improve the cell performance to reduce power consumption, 3) reduce the EHC cell cost by increasing operating current density, and 4) study thermal and water management options to increase system reliability and life. When compared to current mechanical compressors, an EHC will increase reliability and availability, prevent the possibility of lubricant contamination,, increase compression efficiency , and potentially reduces hydrogen delivery cost to less than $1/gge.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Using the "hydrogen pump" as a compression option is novel and promising.
- This project has the potential to demonstrate great results and .the potential for improving the energy efficiency of hydrogen compression that is so critical to a hydrogen roll out
- Achieving hydrogen compression pressures between 5,000 and 10,000 psi is critical to successful on-board hydrogen storage for fuel cells (or internal combustion engines) used in transportation.
- High-pressure hydrogen compression, using an electrochemical system (with no moving parts) is very attractive.
- The project objectives involve improving performance and durability and reducing costs for EHCs, which are goals that support the DOE research, development, and demonstration mission.
- The project is clearly focused on developing this EHC technology to the point where it is competitive with state-of-the-art compressors with further improved reliability, availability, and efficiency.
- The project is aimed at developing a novel hydrogen compression technology, and it is addressing critical issues such as reliability, efficiency, and cost. Hydrogen compression is an important aspect of hydrogen fueling under various scenarios.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach being taken is sound and focused.
- The approach has been on the safe side should push the boundaries faster. For example, it was stated that the team completed 20 cycles to 3000 psi in a 10-cell stack, but they did not test beyond that. It would be helpful to understand the number of cycles before failure.
- The project uses a unique approach to compress hydrogen that does not require the conventional mechanical compression dependence on moving components. The approach comes close to achieving its near-term goals of 5,000 psi.
- Current scoping research has seemed to demonstrate the concept, but it remains focused on improving technology for higher pressure. Equally important, if not more so, is increasing capacity beyond two pounds of hydrogen per day.

- The approach appears reasonable at the broadest level, but lacks detail. Project milestones and progress towards them were highlighted and demonstrated tracking to the requisite metrics.
- The approach is well defined with appropriate key parameters such as current density, catalyst loading, and cell design.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The technical accomplishments to date are very good.
- Accomplishments such as compression ratio and hydrogen recovery are good, but the energy consumption of the current prototype was unclear.. For example if the heating value for EHC 100A-1-9 is approximately 4.1 kWh/kg lower, what is it being compared to?
- There is good progress in achieving the compression goals of 5,000 psi.
- Good technical review of accomplishments and progress on demonstrating capability to deliver higher pressures. One key item missing is cost estimates for a fully operational commercial system. It is unclear whether this system will be competitive with conventional compression systems.
- Project milestones, including increasing pressure (i.e., to 5,600 psi), efficiency, and stack scale-up, appeared to be demonstrated. However, the efficiency and cycling data that were highlighted were at lower pressures than those of interest. The overall progress has been good. The capacity of a single cell is approaching the target of 6,000 psi, although the same needs to be accomplished with a multiple-cell stack.
- The energy consumption, which is a critical parameter, has been improved significantly. However, it is not clear if the values reported are for a single cell or a cell stack.
- It appears from the milestones table that hydrogen recovery is reduced when the number of cells in a stack is increased. This reduction could be a drawback when this system is compared to a mechanical compressor, where there is no loss of hydrogen.
- Current density and catalyst loading are mentioned as critical parameters to cost and efficiency targets, however no improvements are reported with respect to these parameters.
- The team needs to conduct a cost estimation to demonstrate that the cost target can be met. Also, the capacity needs to be improved significantly to be of practical value; 2 lb/day is too small.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- The collaboration with others is good. If the concept, or rather the industry needs, had been discussed with the OEMs it would have been better. The usefulness of a 6,000 psi compressor is limited when considering hydrogen-fueled commercial and personal vehicles.
- While collaboration partners (FuelCell Energy, Inc. and Sustainable Innovations, LLC) and academia partners (University of Connecticut), were included , the overall collaborations were not sufficiently described.
- Collaboration with Sustainable Innovations and University of Connecticut were noted, but there was little acknowledgement of their contributions. It would be helpful to have more detail than one line on one slide to determine the extent of collaboration.
- Two subcontractors are listed, but their specific contributions and the extent to which they participate in this project is unclear. The reviewer would encourage additional collaborations with other entities focused on novel compressor technologies and/or electrochemical processes, particularly for validation.
- The team has appropriate partners and the team members are well qualified to conduct the proposed work. Nevertheless it would be helpful to add an end-user as a partner as the product development gets closer to real-life situations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The proposed work is focused for the 6,000 psi application, though the present 5,600 psi may be adequate for existing needs assuming the efficiencies are improved to a point nearing the goal levels.
- Future work should push the boundaries to determine competitiveness with current technology, such as demonstrating 2 lb/day at 6,000 psi, increasing the pressure capability of a single stage EHC cell from 5,600 psi to 12,000 psi, and reducing the power consumption to that of mechanical compressors (or lower).
- The goals to increase pressure capacity (5,600 to 6,000 psi), improve long-term durability, and increase output (at 3,000 psi) will be addressed and are a logical progression to achieving long-term goals.
- They have an acceptable target for future work, but the program ends in two months. The reviewer would like to have seen economic analyses of expected costs for this system at full scale deployment and comparison to conventional systems, including both CAPEX and OPEX.
- The team is focused on moving to a higher-pressure capability, yet durability and efficiency work is at lower pressures. There seems to be a disconnect in conditions and operation between these tasks.
- The plans are in line with the goals. However, it would be more desirable to achieve the lifetime and cycling performance, as well as hydrogen recovery at the maximum target pressure of 6,000 psi and not 3,000 psi. Also, it should include at least conceptual design for a higher capacity, for example to match a fueling station.
- The plan mentions "Update estimates of capital and operating costs," but there is no mention of any cost results to date.

Strengths and weaknesses

Strengths
- The strengths of the project are the focused applied science and the near-term goal of 6,000 psi. This pressure level is adequate for the fueling of 250 bar systems. (The 250 bar systems are often used with hydrogen-powered material handling systems (e.g., fork trucks)).
- A strength of the project is the ability to use this technique to separate non-reactive compounds like nitrogen, argon, and helium.
- The project has a clear, concise development process and outline of goals. The project lead's experience is also a strength.
- The novel approach to compress hydrogen without the use of mechanical/moving components is a strength.
- The project is novel and technologically elegant.
- Direct conversion of electrical energy to hydrogen compression is an appealing approach and the team members have the technical capabilities required for the tasks. With no moving parts, there is the potential for a higher efficiency machine with lower maintenance costs than a mechanical compressor.

Weaknesses
- The current scope limits the use of this equipment. It is suggested that if the goal were extended to 7,200 psi, it would cover 350-bar fueling (for buses and trucks). If the goal could be extended to 14,000 psi, it would cover 700-bar fueling, which is the current DOE car fleet goal.
- The water content of the compressed hydrogen is a weakness. The present car fleet fuel requirements limit water to approximately 5 ppm to avoid sublimation of ice on the valve and instrument operating surfaces.
- The goals need to be higher, and future cost estimates are not provided. The presentation made no mention of failure modes or development barriers, but these need to be known in order to facilitate collaborations (e.g., materials failure and transport reduction).
- While this concept is successful in avoiding mechanical devices and issues regarding parasitic mechanical energy losses and gradual wear of components, it does not deal with potential parasitic losses arising from internal resistance and degradation/poisoning of membrane properties.
- The concept goal of 2 lbs/day of hydrogen seems rather low. The team needs to address and discuss concepts to scale up the system to a larger throughput.
- The team did not present any economics to show if this would be viable as compared to conventional compression technology.
- Practical system designs are challenging and have not yet been addressed.
- With a futuristic view, cost and performance should be compared with emerging technology options (e.g., ionic liquid and metal hydride-based compressors) as well and not just with conventional mechanical compression.

- The milestones table is confusing with different pressure levels, different cell structures (number of cells), lifetime and cycling targeted. The progression from a single-cell to a multiple-cell stack, the number of cycles, and lifetime testing should be better aligned with the final product configuration in mind.
-

Specific recommendations and additions or deletions to the work scope

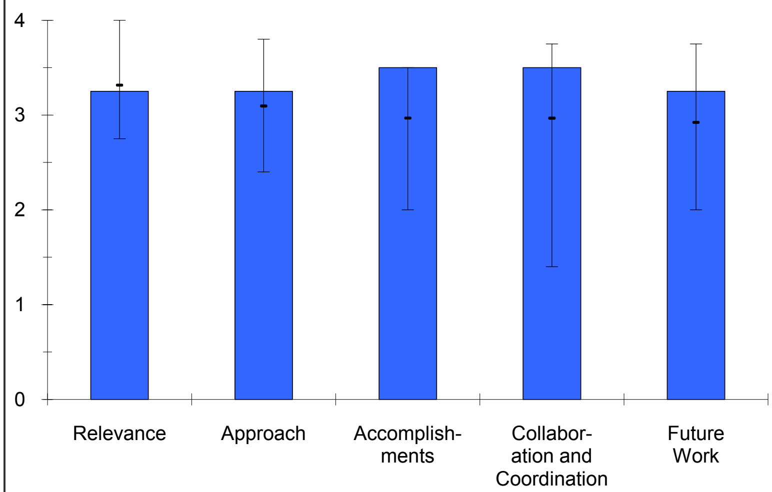
- This is an excellent project. The team should consider a post-compression drier and pressure level (possibly by staging cells) approximating the needs of 350 and/or 700-bar systems to open market potential. It may be useful to have discussions with either a fuel provider or specialty gas company.
- The team should increase the project goals to allow the determination of future competitiveness with current methods of compression.
- The project should add a task to address the impact of internal resistance on system efficiency and provide some level of characterization of performance degradation as a function of compression cycles.
- The team should do the scoping economics and a comparison to conventional compression systems.
- The project should include a detailed cost estimation to show that targets can be met at practical flow rate capacities and under practical operating conditions.
- Key barriers to scale up should be identified up front and addressed to make sure that they will not be showstoppers.
- The final report should include a discussion about key parameters, such as cell density and catalyst loading, and their impact on cost and performance.

Project # PD-51: Characterization of Materials for Photoelectrochemical Hydrogen Production (PEC)
Clemens Heske; University of Nevada Las Vegas

Brief Summary of Project

The overall objective of this project is to compile experimental information about the electronic and chemical properties of the candidate materials produced within the PEC Working Group to determine status-quo, find unexpected results, propose modifications to partners, and monitor the impact of implemented modifications. The project objectives are to 1) use a world-wide unique "tool chest" of experimental techniques; and 2) address all technical barriers related to electronic and chemical properties of the various candidate materials, in particular, bulk and surface band gaps, energy-level alignment, chemical stability, and impact of alloying/doping.

Overall Project Score: 3.4 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is at a low technology readiness level.
- This sub-project supplements the family of investigators on this joint project by providing the characterization of materials and a fundamental understanding of the materials being studied, both before and after electrolysis testing. This understanding is essential for providing much needed direction on the mechanisms and limitations of these photoactive materials for the design of improved materials at the molecular level.
- The work performed in this project is vital to understand the mechanisms of in-situ energy-level alignments, as well as the subtle changes that occur to the materials over time.
- The project is addressing the pertinent barriers to PEC production of hydrogen.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The research focuses on evaluating and characterizing the synthesized iron oxide behavior.
- As a stand-alone project, this project does not have a lot of strategic direction. The sub-team seems to be at the mercy of the other investigators in terms of what materials are being studied. However, the materials characterization approach is excellent and provides good direction back to the team.
- The work uses state-of-the-art techniques and provides quantitative insight into areas which used to be primarily speculative or qualitative.
- The project is applying characterization skills to a number of materials.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- There is still planning of work and tradeoff needs to be done. The current technology readiness level (TRL) is very low, about TRL1. The major issues with any new proposed technology, such as this, include market

variability, manufacturing in a large scale, reproducibility, reliability, and durability. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- Work has been completed on all of the materials of interest from the other PEC Working Group teams, and feedback was given with guidance on mechanisms.
- This project does not directly address the barriers outlined in the DOE program. Instead, the progress here is graded as excellent for the insight that this project provides for the variety of materials systems examined. The work here can greatly accelerate the success of materials and systems iterations.
- The project has characterized a number of materials for a variety of collaborators.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Collaboration is apparent among the team in that each member is working on a different material for the same technology and sharing the results.
- It is not clear how much the other investigators are leveraging this work to set their next direction. It would be helpful to have one of the team members describe the overall roadmap and interactions.
- All of the work is collaborative in nature; the investigator provides analytic services to other DOE-funded PEC projects.
- The team has excellent interaction with key players in the PEC community.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The team is evaluating different materials and studying the impact of material modifications.
- The team is continuing to provide the same type of analytical results for new systems and newer collaborators will continue to provide important inputs for guiding the next iterative steps.
- Work should continue as long as there is a need for these characterizations within the PEC community.

Strengths and weaknesses

Strengths
- The project has good out of the box technical approaches, collaboration with other research labs, and focus on the stability of material.
- The team has excellent materials characterization capability and thoroughness in the experimental approach.
- The project provides important data to guide the design of the next materials iteration, in both synthetic and theoretical approaches. This helps the collaborators understand better what works and what does not work.

Weaknesses
- Technology viability, scalability and manufacturing in a large scale are not addressed or recognized.
- The team is somewhat at the mercy of other teams, in terms of the materials to be evaluated.

Specific recommendations and additions or deletions to the work scope

- The team should produce larger active area cells and evaluate the viability and feasibility of the technology.

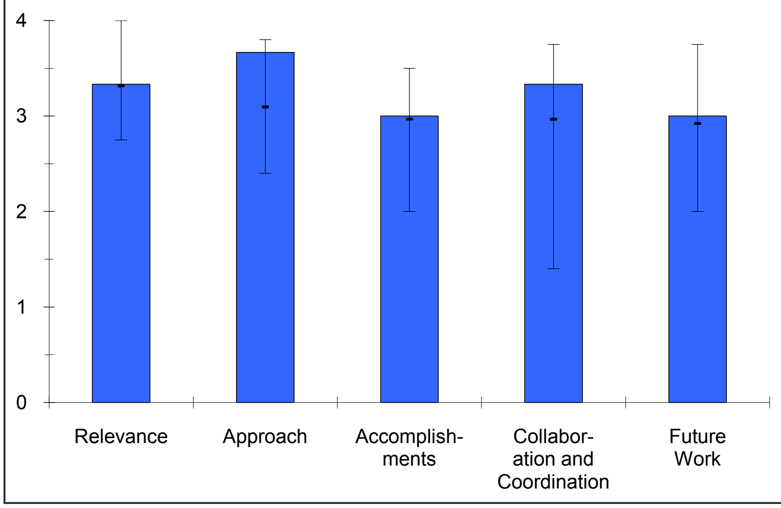
Project # PD-52: PEC Materials: Theory and Modeling

Yanfa Yan; National Renewable Energy Laboratory

Brief Summary of Project

The main focus of the project is to 1) understand the performance of current PEC materials, 2) provide guidance and solution for performance improvement, 3) design and discover new materials, and 4) provide theoretical basis for go/no-go decisions to DOE PEC hydrogen projects.

Overall Project Score: 3.2 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of the proposed technology is at a very low technology readiness level. Each of the collaborating members is focusing on different synthesis materials, and there are many more materials for consideration.
- The project has a very good theoretical approach, which is needed for this area.
- A fundamental understanding of how the various properties of PEC materials can be tuned is important. Additionally, a quick way to screen various doped compositions without risking false negatives from non-optimally processed materials is a valuable tool.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The research focuses on evaluating different syntheses, and the laboratory evaluation of small samples show some promising results.
- The approach can help to determine the limitations and advantages of different material structures if it is used by the rest of the team.
- The approach slide (slide 5) outlines the strategy of using theory to investigate band structure, optical absorption, defect and doping effects, surface chemistry, and structural stability. However, the last two items are not addressed in the presentation, though the contributions of understanding the first three on the list are very valuable.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- There is still planning of work and tradeoff needs to be done and more materials need to be evaluated. The current technology readiness level is very low, below TRL1. The major issues with any new proposed technology, such as this, include market variability, manufacturing in a large scale, reproducibility, reliability, and durability. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- The team as a whole has a long way to go to prove the feasibility of direct PEC. All of the systems to date require voltage assist to produce photocurrent in water. This sub-team, however, is providing good progress.

- It is not clear whether the delafossite optical absorption coefficients shown on slide 15 are calculated or measured. Slides 8, 10 and 11 underscore one of the primary pitfalls of theory work, which is that the theory only holds if the proposed structure is actually formed. It is refreshing to see that pitfall called out explicitly.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material for the same technology and sharing the results.
- This sub-team is somewhat dependent on the other team members in the PEC Working Group, but less so than Heske since they are modeling instead of measuring. The reviewer would like to see continued direction setting.
- The collaborations listed are very useful, including synthesis groups for materials creation and properties and an analytical group (Nevada) for the confirmation of band structures. During the reporting period, it appears that most of the collaboration was with University of California, Santa Barbara.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Many other materials and experimental methods need to be explored and evaluated, but it is unclear whether any of them would provide desired results.
- It would be helpful to discuss why the next generation materials were chosen.
- Continuing to support the PEC efforts through the theory work is an important component to the overall discovery process. There appears to be a good mix of stages of materials (i.e., early-stage work, such as the $BiVO_4$, and work that is further along such as the iron oxide and the delafossites). The stability of nitrides and carbides is interesting, and it will be interesting to see if theory can really help identify materials' stability.

Strengths and weaknesses

Strengths
- The project is focusing on a single material and evaluating its potential and viability.
- The project has strong theory and calculation competency, thus providing a good explanation of data.
- The theory component is important to help understand the results observed and to help guide promising areas of research, especially in light of limited resources. The project seems to be taking "physical realities" into account, such as the formation of different phases and the possibility for a dopant to dope into different sites.

Weaknesses
- Technology viability, scalability, and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- The project needs to take a stronger direction-setting role on the synthesis of materials from other members.
- There is no indication that the theory is developed enough that it can give any predictions regarding the final phase of the materials or which of the multiple sites a dopant will prefer. Such capabilities would enhance the screening value of the simulation work.

Specific recommendations and additions or deletions to the work scope

- The team should consider reevaluating technical viability and feasibility.

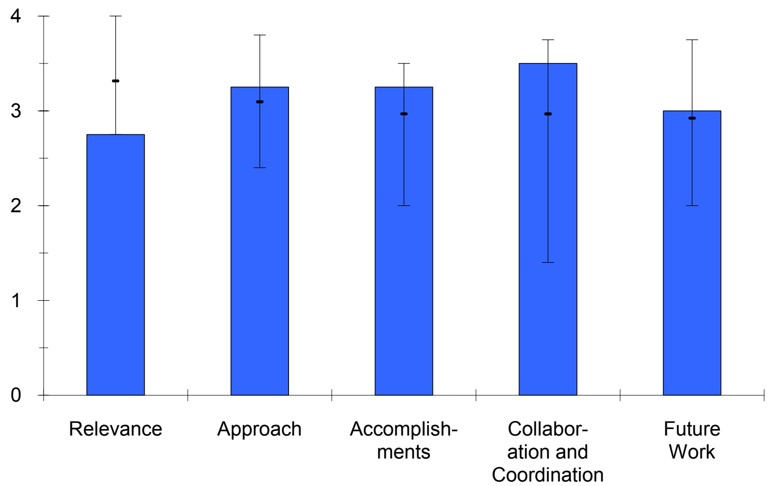
Project # PD-53: Photoelectrochemical Hydrogen Production: Progress in the Study of Amorphous Silicon Carbide as a Photoelectrode in Photoelectrochemical Cells

Arun Madan; MVSystems

Brief Summary of Project

The overall objective of this project is, by December 2010, to fabricate a hybrid a-Si tandem solar cell/amorphous silicon carbide photoelectrode device which exhibits a photocurrent greater than or equal to 4 mA/cm2 and durability in electrolyte of at least 200 hours. Advantages of amorphous silicon carbide photoelectrode are: 1) lower bandgap (Eg) in comparison with tungsten oxide, produces more photocurrent; 2) bandgap can be increased/tuned with carbon inclusion into amorphous silicon material; and 3) amorphous silicon carbide uses same plasma-enhanced chemical vapor deposition technique (PECVD) as amorphous silicon photovoltaic (PV) cells.

Overall Project Score: 3.2 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is at a very low technology readiness level. Each of the collaborating members is focusing on different synthesis materials, and there are many more materials for consideration.
- It is not clear why one would want to degrade the efficiency of a solid state device by burying it with an over layer and using it to split water. While this is an elegant solution to the problem of not having good materials for splitting water directly from the sun, it seems that this approach adds significant complexity and cost, making the comparison to PV plus electrolysis even less attractive.
- The project is in line with the DOE goals and targets.
- The project effort is aligned with the DOE program objectives.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The research focuses on evaluating different syntheses, and laboratory evaluation of small samples shows some promising results.
- It is not clear how these devices are going to get to the 10% target, but even the 5%-7% that has already been reached is fairly impressive.
- The approach of leveraging PV technology is highly valuable.
- The team has a systematic approach, which is making good use of analytical equipment and evaluation.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- There is still planning of work and tradeoff needs to be done and more materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale,

reproducibility, reliability, and durability are major issues with any new proposed technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- Good progress has been made against the milestones laid out in the project.
- The analysis of work function and differing surface modification/catalytic sites was well done and looks to have been fruitful. The analytical work in collaboration with Professor Heske helps to point out potential degradation pathways, which will relate to durability of the systems.
- Good progress has been made in several key areas.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material for the same technology and sharing the results.
- The project is well integrated with the PEC Working Group.
- Sample exchange for testing and analysis has led to very useful information regarding both performance and durability.
- This is a collaborative project with appropriate partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Many other materials and experimental methods need to be explored and evaluated, but it is unclear whether any of them would yield desirable results.
- There seems to be a lot of future work for a project with a go/no-go decision later this year. A better plan may be to focus on how to get the information that is needed for the decision.
- The results seem to indicate that elimination of the silicon oxide (via Hydrofluoric Acid [HF] etch) improves performance, but that silicon oxide forms again during PEC testing. If this affects the durability significantly, prevention of the formation of silicon oxide should be considered. Incorporation of nitride materials looks like a good avenue to pursue from a materials stability standpoint.
- A go/no-go decision is to be made by the end of 2010.

Strengths and weaknesses

Strengths
- Focusing on a single material and evaluating its potential and viability is a strength.
- The project has an elegant solution to the problem of band mismatch for water splitting. The partners have good materials characterization capability.
- The project leverages an existing knowledge base of PV very well and supplements the PEC knowledge base. The project leverages collaborations for testing and analysis.

Weaknesses
- Technology viability, scalability, and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- The cost of this approach compared to other options is a concern and should be addressed. The project should determine what counter electrode will be looked at long term and how much that will hurt efficiency, how the system will be designed, and if these electrodes can be made in a large active area.
- The project has a PV approach to PEC. However, the collaborations bridge the knowledge gap, just as the investigator's knowledge of PV bridges some PEC knowledge gaps.

Specific recommendations and additions or deletions to the work scope

- The team should reevaluate technical viability and feasibility.

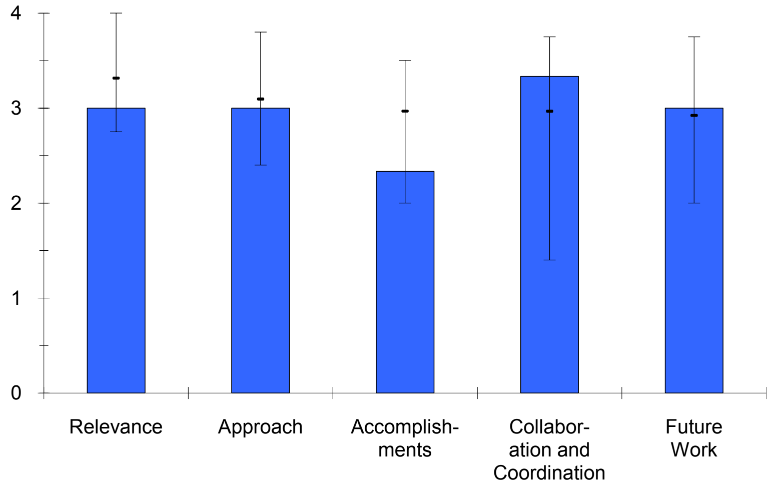
Project # PD-54: Progress in the Study of Tungsten Oxide Compounds as Photoelectrodes in Photoelectrochemical Cells

Nicolas Gaillard; Hawaii Natural Energy Inst.

Brief Summary of Project

The overall objective of this project is to study tungsten oxide in PEC cells. Advantages of tungsten oxide include 1) good performance has been demonstrated in several applications, 2) film can be deposited using low-cost processes, and 3) tungsten oxide satisfies the main criteria for water splitting. A tungsten oxide PEC device has shown 3.1% solar-to-hydrogen efficiency demonstrated in a mechanical stack configuration (using MVSystems' solar cell). However, this material suffers from 1) its bandgap value (2.6 eV) that limits light absorption and 2) the position of the valence band versus oxygen half-reaction potential.

Overall Project Score: 2.8 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The technology meets the DOE research, development, and demonstration intent, but the current state of proposed technology is at a very low technology readiness level. Each of the collaborating members is focusing on different synthesis materials, and there are many more materials for consideration.
- The project uses an interesting idea, but the reviewer does not completely see the advantages compared with other options. The reviewer would like to see a more detailed cost and efficiency comparison.
- The project is aligned with DOE objectives.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The research focuses on evaluating cost effective fabrication methods and different processes; laboratory evaluation of small samples shows some promising results.
- The team's testing approach is generally sound and the project is well-designed for the goals laid out.
- The project addresses DOE-appropriate barriers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- There is still planning of work and tradeoff needs to be done and more materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new proposed technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- Significant data from the last year was in the poster. The progress on milestones seems to be at the same point in 2010 as it was in 2009.
- The project research is progressing towards the performance objectives.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material for the same technology and sharing the results.
- The project is very well coordinated with other related posters. The PEC Working Group in general seems to be collaborating well. The team could potentially use characterization and modeling more effectively.
- The team is a participant in the PEC Working Group and is collaborating with appropriate technical partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- There are many other materials and experimental methods that need to be explored and evaluated. The question is whether any of them would produce desired results.
- There is a good plan focused on addressing material issues.

Strengths and weaknesses

Strengths
- Focusing on a single material and evaluating its potential and viability is a strength. The project also does well in economic viability studies.
- The team has good science and a good approach, with collaboration across many groups.

Weaknesses
- Technology viability, scalability and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- The cost of this approach compared to other options is a concern and should be addressed. It is unclear what counter electrode will be looked at long term and how much it will that hurt efficiency, how will the system be designed, and if these electrodes can be made in a large active area. The project seems to be at a lower point than the other teams in terms of efficiency.

Specific recommendations and additions or deletions to the work scope

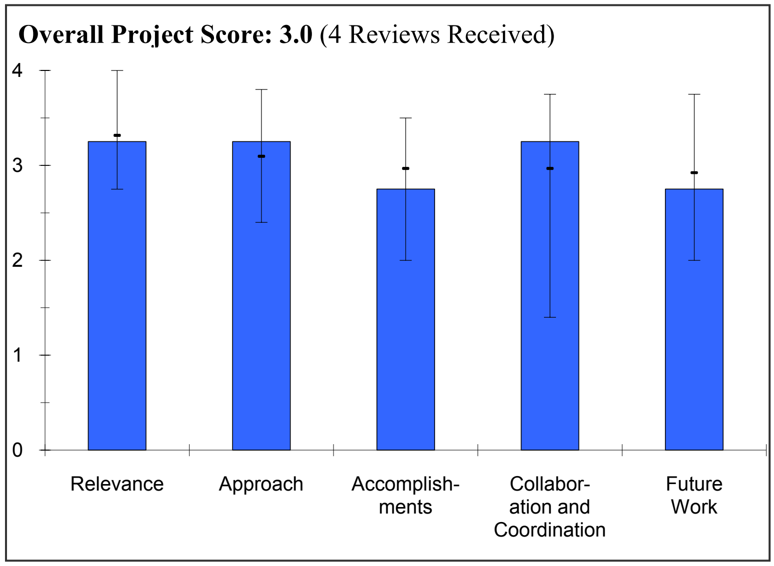
- The team should consider reevaluating technical viability and feasibility.

Project # PD-55: Progress in the Study of Copper Chalcopyrites as Photoelectrodes in Photoelectrochemical Cells

Jess Kaneshiro; Hawaii Natural Energy Inst.

Brief Summary of Project

The overall objective of this project is to develop copper chalcopyrite materials for incorporation into a hybrid photoelectrode (HPE) device capable of splitting water for hydrogen production when immersed in a suitable electrolyte and illuminated by sunlight. Material development objectives are to 1) identify methods of increasing the bandgap of copper chalcopyrite films to pass more light to an underlying solar cell; 2) perform surface modifications, including decreasing required voltage bias, improving surface kinetics, and increasing durability; and 3) identify methods to move the valence band maximum lower, including using silver in place of copper, using sulfur in place of selenium, and surface treatments.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is at a very low technology readiness level. Each of the collaborating members is focusing on different synthesis materials, and there are many more materials for consideration.
- It is not clear why one would want to degrade the efficiency of a solid state device by burying it with an over layer and using it to split water. While an elegant solution to the problem of not having good materials for splitting water directly from the sun, it seems that this approach adds significant complexity and cost, making the comparison to PV plus electrolysis even less attractive.
- Without a single material in existence that meets all criteria, it is important to examine multiple materials avenues to meet the targets.
- The project is aligned with DOE objectives.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The research focuses on evaluating different syntheses, and laboratory evaluation of small samples shows some promising results.
- The reviewer is not sure why the team does not just use copper indium gallium (di)selenide (CIGS) PV technology with electrolysis, since that seems easier than trying to do band matching.
- Different facets affecting the ultimate performance of a device have been isolated, and approaches to the improvement of performance, as measured by the DOE targets, have been identified for investigation.
- The project addresses appropriate barriers to cost-effective and efficient PEC production of hydrogen.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- There is still planning of work and tradeoff needs to be done and more materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.
- The team has made some progress, but they do not show much traction towards milestones since 2009 and there appear to be limitations to getting farther. The team should try to use modeling to see if this is intrinsic or if in theory these issues can be overcome.
- Various pieces seem to be coming together, and the path forward is clear. The phase segregation seen in AIGaSe is not entirely unexpected. The segregation does raise some questions regarding the usefulness of the preliminary data shown.
- The project is making good progress in achieving goals.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material for the same technology and sharing the results.
- Collaboration in general between the groups seems very good in terms of information sharing. They need leverage information to determine the pathways forward.
- The exhibited ties to and use of collaborators are good.
- The team is a participant in the PEC Working Group.
- The team is collaborating with appropriate partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- There are many other materials and experimental methods that need to be explored and evaluated. The question is whether any of them would pan out.
- The team did not really answer a reviewer question from last year on the go/no-go decision. Slide 6 is not a "milestone plan," it is the end goals for each year. The team needs to describe the science that is proposed to meet these milestones.
- The proposed future work addresses the work needed to reach targets and to overcome barriers in a way that is not redundant with other work going on in DOE-sponsored projects.
- There is a structured approach that meshes well with other partners in the activity.

Strengths and weaknesses

Strengths

- Focusing on a single material and evaluating its potential and viability is a strength.
- The material has very high photocurrent, which the team would like to leverage in this project.
- This project approaches system challenges in novel ways. These paths have basis either in theory or in correlation to PV systems, showing good rationale for the choices made. Their progress appears to be good, despite uncertain funding during the course of project.

Weaknesses

- Technology viability, scalability, and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- The cost of this approach compared to other options is a concern and should be addressed. It is unclear what counter electrode will be looked at long term and how much will that hurt efficiency, how the system will be designed, and if these electrodes can be made in a large active area.
- While all of the work is interesting and useful, of the three main facets, surface treatment and back contact work seem to be a different general scope than the silverization. Ultimately, there should be some focus on cost of

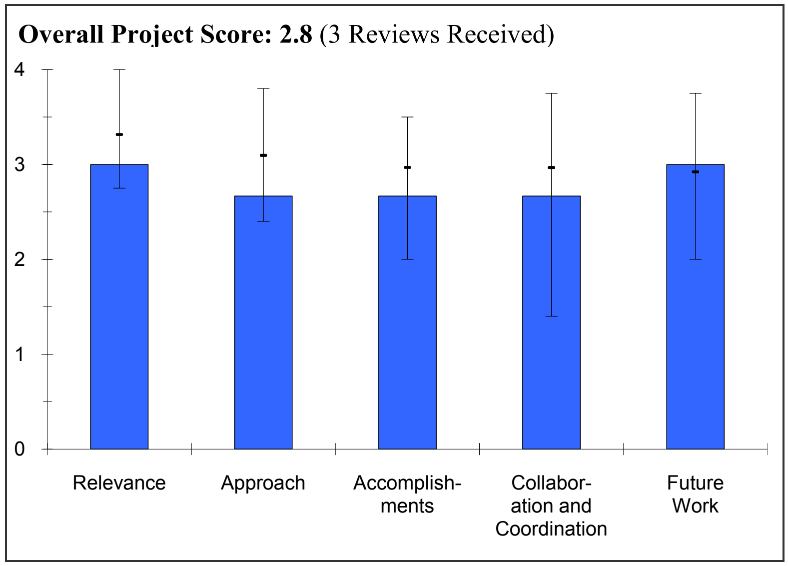
materials. Although some digging shows that silver is likely to be cheaper than indium, it is probably still more expensive than copper.

Specific recommendations and additions or deletions to the work scope

- The team should consider reevaluating technical viability and feasibility.
- The team consider separating the silverization into a separate project, if the segregation cannot be easily overcome.

Project # PD-56: Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen

Liwei Xu; Midwest Optoelectronics, LLC



Brief Summary of Project

The overall objective of this project is to develop critical technologies required for cost-effective production of hydrogen from sunlight and water using thin film silicon-based photoelectrodes. Two approaches are taken for the development of efficient and durable PEC cells: 1) an immersion-type photo electrochemical cell in which the photo-electrode is immersed in electrolyte, and 2) a substrate-type PEC cell in which the photoelectrode is not in direct contact with electrolyte.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The study may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is at a very low technology readiness level. Each of the collaborating members is focusing on different synthesis materials, and there are many more materials for consideration.
- The comparison approach is attractive, although this project requires a lot of engineering. The reviewer likes the idea of directly comparing immersion versus substrate, rather than being limited to one.
- The objectives are aligned with DOE.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The research focuses on evaluating different materials, and laboratory evaluation of small samples shows some promising results in lowering the cost and increasing the efficiency.
- This project has some good work, but it seems a little scattered in terms of laying out an organized experimental pathway.
- If the project is successful, the work will contribute to overcoming barriers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- There is still planning of work and tradeoff needs to be done and more materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new technology. Some technologies show great and promising results, but they never enter the marketplace due to low feasibility.
- Progress has been made despite no FY 09 funding. The chemistry seems reasonable; although, as stated above, the device engineering is difficult and seems a little outside the expertise of the team.
- The team is making good progress in the fabrication of materials.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material that would apply to the same technology.
- Interaction between partners is not very clear. The presentation did not describe, for example, when the team is meeting and how tasks are coordinated.
- The team has active collaborations with appropriate partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- There are many other materials and experimental methods that need to be explored and evaluated. The question is whether any of them would pan out.
- The project is leveraging partner research and expertise.

Strengths and weaknesses

Strengths
- Focusing on additional potential materials and evaluating their potentials and viability is a strength.
- The team is learning from issues and making progress. Direct comparison between the substrate and immersion approach is refreshing, compared with teams that ignore direct electrolysis.

Weaknesses
- Technology viability, scalability, and manufacturing in large scale are not addressed or recognized. Collaborating partners are all focusing on different materials, which shows how much work needs to be done.
- There is substantial industry knowledge on the alkaline electrolysis side that could be better leveraged. It might be worth consulting with GE or another company with technology in this area. It seems like there is a lot of work left to cover in the remaining time and budget.

Specific recommendations and additions or deletions to the work scope

- The team should consider reevaluating technical viability and feasibility.

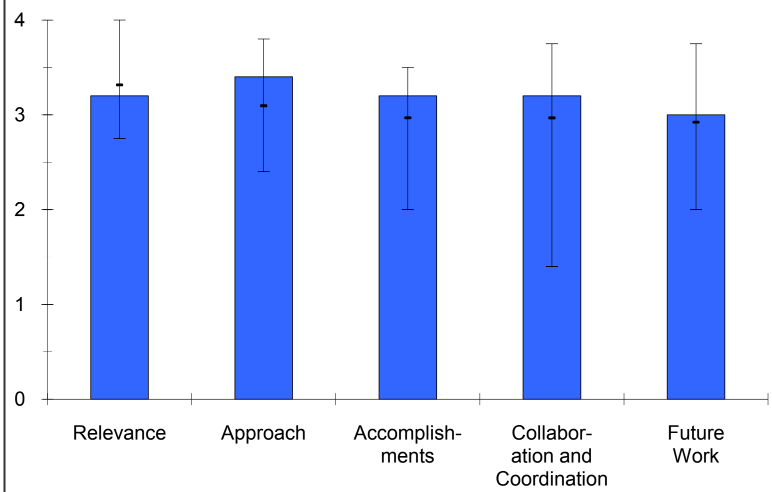
Project # PD-58: Characterization and Optimization of Photoelectrode Surfaces for Solar-to-Chemical Fuel Conversion

Tadashi Ogitsu; Lawrence Livermore National Laboratory

Brief Summary of Project

The objectives of the project are to 1) understand the underlying mechanism of surface corrosion of semiconductor-based PEC cells, 2) understand the dynamics of water dissociation and hydrogen evolution at the water-photoelectrode interface, 3) understand the electronic properties of the water-electrode interface, and 4) understand the relationship between corrosion and catalysis.

Overall Project Score: 3.2 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The technology may meet the DOE research, development, and demonstration intent, but the current state of proposed technology is at a very low technology readiness level.
- This project is doing very important background work for understanding what materials will work and what will not work. The reviewer would like to see this applied to more relevant semiconductors.
- The team is studying some interesting phenomena, but somebody with a background in the catalysis of hydrogen evolution should be advising them.
- Understanding the surface chemistry of the system, both intended and side reactions, is an important facet to being able to tailor robust materials.
- Understanding the fundamental properties of materials could be helpful to other members of the working group.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Surface corrosion of semiconductor-based PEC materials is important for further understanding of the corrosion effect on different materials and how it impacts their performance.
- The project has a nice capability in modeling and demonstration of the role of water structure. It is challenging to get to this level of detail and good progress is being made.
- The reviewer is not sure what the full limits are of what they can calculate. Certainly there is a fundamental problem in that semiconductors are not very catalytic as electrodes, and so understanding adsorption energies of hydrogen, oxygen, water, and their intermediate species is of interest. It is unclear if the team can model energies if we put a few nanoclusters of platinum on the indium phosphide (InP) surface. The reviewer was a little concerned by the remark that they "can't simulate excited states."
- The work here provides important information on the decomposition pathway of a material that shows promising properties before decomposing. It is always important to properly understand the failure modes before trying to design a solution.
- The simulation results could lead to new directions for experimental work.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The research did evaluate the surface corrosion effect on several oxides in depth.
- The project has made impressive progress in a short time. Despite a reviewer question about only being a surface model, their animations seem to show propagation of a corrosion mechanism already (one can see underlying bonds starting to break apart). It will be important to extend the model to a longer term.
- The team is just getting started and has many interesting calculations ahead of them. The reviewer was not too excited seeing water adsorption on InP and is unclear why modeling a few picoseconds of dynamics teaches us anything about catalysis or corrosion. Those surface oxygens eventually begin to work their way into the lattice, oxidizing the entire electrode. The team should clarify if the lattice oxygen diffusion is going to be modeled.
- In the short time that the project has been active, it seems that good progress has been made towards establishing a baseline model from which to work, including observation in the simulation of the degradation mechanism.
- The team has interesting computer simulations.
- The team needs to corroborate calculated behavior with experimental data.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Collaboration is apparent among the team, in that each member is working on different material on the same technology and sharing the results.
- Real collaboration is not clearly demonstrated. It is unclear shat feedback is being given to other partners or being taken into the model.
- The reviewer hopes the team will have a lot to say and that the experimentalists will listen to them. However, it would help if they crunch numbers on the materials that the experimentalists are making.
- Although it is not clear that the collaborative exchange had begun at the time the project was started, it is clear that the lines of communication are now identified and open.
- The interactions with the PEC Working Group are very useful.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The team seems to be aware of logical next steps in the project and limitations to be addressed. They could use discussion on what other materials will be addressed, as well as an overall map of the project.
- Most of what the team has proposed has yet to be performed, so the reviewer's comments are basically the same as the Approach section.
- The future work appears well laid out to build upon the work reported here and addresses several potential avenues to model improving the system.
- Collaborations with experimentalists will be helpful to the understanding of calculated and observed responses of materials.

Strengths and weaknesses

Strengths
- Focusing on surface corrosion characteristics is a strength.
- The project has made good progress in understanding and technique.
- The team has a potentially powerful method for understanding catalysis of the water-splitting/hydrogen-evolving reaction.
- The project fills a hole in the overall research portfolio.

Weaknesses
- There is still planning of work and tradeoff needs to be done and more materials need to be evaluated. The current technology readiness level is very low, below TRL1. Market variability, manufacturing in a large scale, reproducibility, reliability, and durability are major issues with any new technology. Some technologies show great and promising results, but they never enter the market due to low feasibility.

- The project still has a way to go on the model to extend below the surface and apply to less expensive semiconductors that will have more relevance.
- The reviewer thinks there are more relevant systems than InP that the team could be looking at and that they could at least put some platinum on the surface.

Specific recommendations and additions or deletions to the work scope

- The team should consider reevaluating technical viability and feasibility.
- The project should evaluate the photoanode also. It would be useful to have a good semiconductor for oxidative water-splitting to evolve oxygen.
- The team could add work to examine modified electrode materials

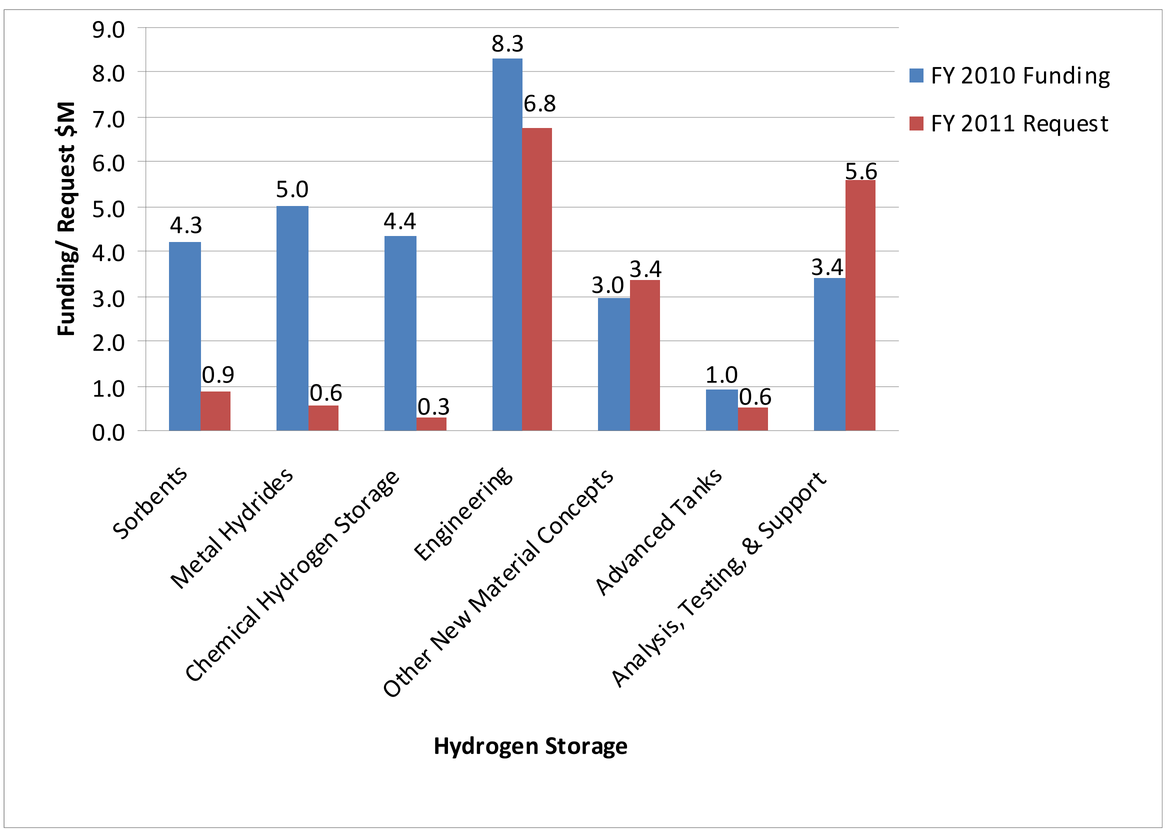
2010
Hydrogen Storage
Summary of Annual Merit Review of the Hydrogen Storage Sub-program

Summary of Reviewer Comments on Hydrogen Storage Sub-program:

The Hydrogen Storage sub-program portfolio remained focused in FY 2010 on materials based R&D and expanded efforts in system engineering for onboard transportation applications. The primary goal has been development and demonstration of commercially viable hydrogen storage technology to enable greater than 300-mile vehicle driving range, while meeting safety, vehicular packaging, and cost and performance requirements. R&D efforts remained focused on applied, target-oriented research of materials systems including high-capacity metal hydrides, chemical hydrogen storage carriers, and high-surface area adsorbents with the potential to meet the vehicular technical targets. In addition, the sub-program continued to support advances in physical storage (e.g., compressed hydrogen gas) for nearer-term applications.

Hydrogen Storage Funding by Technology:

The chart below illustrates the appropriated funding in FY 2010 and the FY 2011 request for each major activity. It should be noted that the three materials Centers of Excellence (CoEs) are scheduled to end in FY 2010.



Majority of Reviewer Comments and Recommendations:
The Storage portfolio was represented by 44 oral and 47 poster presentations in 2010. A total of 41 projects (29 presentations and 12 posters) were reviewed. In general, the reviewer scores for the storage projects were good, with scores of 3.9, 3.0 and 1.8 for the highest, average, and lowest scores, respectively. The projects were reviewed by three to eight reviewers each. Reviewers remarked favorably on the coordination and management of the Storage Materials CoEs. Key recommendations and major concerns for each project category are summarized below.

Chemical Hydrogen Storage: The goal of chemical hydrogen storage applied materials R&D is to develop materials with high hydrogen capacities from which hydrogen can be safely liberated at rates to meet vehicle powerplant requirements and develop energy efficient and cost effective pathways to rehydrogenate the materials. The general approach has been to use theory-guided experimental research to develop high capacity materials with favorable, ideally near thermoneutral, hydrogen release thermodynamics. Cost and energy efficiency analyses have been carried out on rehydrogenation pathways with the output used to guide improved processes. Other areas of investigation center on development of hydrogen release catalysts and strategies to minimize loss of volatile species along with the released hydrogen.

The Chemical Hydrogen Center of Excellence (CHCoE) has focused on use of ammonia borane and its derivatives as storage materials. Research is also being carried out within the CoE on liquid heterocyclic compounds that contain carbon, boron, and nitrogen (CBNs). For the CBN work, the reviewers found the idea of liquid phase storage materials favorable and they felt more effort is needed to understand the release mechanisms and limits to hydrogen capacity. In general, the reviewers found the CHCoE to be well organized and to have a strong collaborative effort.

Advanced Metal Hydrides: The overall goal of metal hydride materials applied research is to develop materials that can be charged with hydrogen on-board the vehicle at conditions amenable to the vehicle environment. Key barriers to this goal are the hydrogen charge and discharge kinetics at acceptable temperatures and pressures and the thermodynamics of the reactions which directly impact the net available capacity of the material. Metal hydride research within the Program has used theory to guide experimental efforts towards systems projected to be most favorable. One area where theory has lead to experiment is in predictions that particle size might be used to modify or “tune” thermodynamics. Progress has been made in designing and carrying out experiments to validate the projections on thermodynamics affects as well as to understand kinetic benefits to small metal hydride particles. New material discovery work has also continued with research on novel materials and use of unconventional solvents.

The DOE metal hydride research portfolio consists of projects carried out through the Metal Hydride Center of Excellence (MHCoE) and independent research programs. Overall the reviewers were favorable towards the metal hydride work. Coordination among the independent projects and with the MHCoE is recommended. The continued use of down-selections to eliminate efforts on material systems that do not show promise toward meeting targets and focus on systems that do is recommended.

Sorbent Materials: The goal of sorbent applied materials R&D is to develop materials with high hydrogen volumetric and gravimetric reversible net available capacities at closer to ambient temperature and at moderate pressure. “Net available” means that the temperature, pressure, thermodynamics, and transient delivery/uptake rates are taken into account to determine the amount of fuel available to the power plant. The general approach is to identify and design (often via theoretical modeling) high surface area per volume porous materials with increased hydrogen uptake capacities and higher binding energies for molecular hydrogen that will enable storage above cryogenic temperatures (e.g., higher than 77K, liquid nitrogen temperature). The key challenges have been to synthesize materials with high surface area per unit volume while retaining a narrow pore size distribution to maximize micropore over mesopore volume. As the hydrogen binding energy is increased using various strategies, the challenges then include achieving a "constant" heat of adsorption with coverage; then, as the binding energy further increases, challenges include issues with hydrogen uptake kinetics, net availability of hydrogen adsorbed, and thermal heat rejection upon refilling.

The DOE portfolio for sorbent materials has included the Hydrogen Sorption CoE and independent R&D projects. The reviewers noted that while many of these materials do show promise, issues still remain with achieving "net available" volumetric and gravimetric capacities and transient performance metrics that can meet DOE vehicular targets. Furthermore, retaining these properties at closer to ambient temperature/moderate pressure has proven difficult, as hydrogen/adsorbent site binding energies remain too low. In addition, external (to the research group) verification of excess capacity isotherms and isosteric heats of adsorption is recommended for all experimental groups. For many of these materials, additional characterization is recommended to complement the storage performance measurements such as NMR, neutron characterization to provide feedback into material design and verification data for the accompanying theoretical modeling efforts. Down selection of sorbents from further evaluation should be based upon criteria rooted in laboratory measurements, rather than upon theoretical predictions that had not been previously validated by experiments.

Engineering: Established in FY 2009, the Hydrogen Storage Engineering CoE (Engineering CoE) is a fourth CoE. The Engineering CoE comprises 10 partners and is led by Savannah River National Laboratory. The Engineering CoE is charged with developing complete system models and system designs for the three hydrogen storage material classes; evaluating the current state-of-the-art against the complete set of on-board vehicle hydrogen storage performance targets; identifying engineering R&D gaps and carrying out R&D to address the gaps. Phase III of the effort will include building and testing up to three complete sub-scale system prototypes for the material classes if sufficient criteria are met. Overall, the reviewers rated the Engineering CoE projects favorably. Concern has been expressed about the need for efficient coordination and communication between the Engineering CoE partners. It was noted that the matrix organization can be an effective structure if well managed and that certain partners appeared to have too diverse responsibilities or their activities were not thought to be in complete alignment with the Engineering CoE as a whole. Reviewers suggested that some restructuring and re-scoping may be necessary. Concern was also raised about potential scope creep, which needs to be carefully managed.

Advanced Tanks: The advanced tank R&D is conducted by a small but diverse group of researchers from industry, universities, and National Laboratories. A new effort this year, an investigation to reduce the cost of high-strength carbon fiber through use of melt-spin processing to reduce PAN precursor material, was highly rated by reviewers. Technical improvements in reducing the weight and volume of cryo-compressed tanks have continued, which could provide a pathway to meet the 2015 storage targets.

Analysis, Testing, and Support: The Analysis, Testing, and Support efforts provide independent verification of the potential of the various materials and technologies being developed through the sub-program. Performance and cost analyses efforts were completed on compressed and liquid hydrogen storage systems. The report, which will be publicly available, includes analyses of various options including Type III (metal liner) and Type IV (non-metal liner) cylinders and single versus multiple tank designs. The analysis efforts were also carried out to include the evaluation of the current cryo-compressed designs, indicating they have the potential to meet 2015 gravimetric and volumetric targets, and MOF-177 cryo-sorbents systems. Preliminary performance analysis of ammonia borane in ionic liquids was completed and a potential strategy to control the temperature of the exothermic hydrogen release proposed. Testing efforts include independent material testing to verify gravimetric and volumetric capacity claims. The reviewers felt these efforts were well executed and extremely beneficial to the Program.

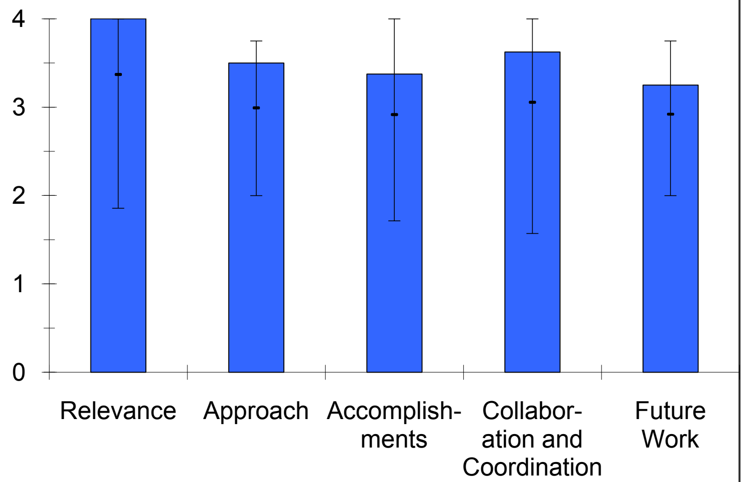
Project # ST-01: System Level Analysis of Hydrogen Storage Options

Rajesh Ahluwalia; Argonne National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) perform independent systems analysis for the DOE and to provide input for go/no-go decisions, 2) provide results to the Centers of Excellence (CoE) for assessment of performance targets and goals, addressing all aspects of on-board and off-board storage targets including capacity, charge and discharge rates, greenhouse gas emissions, and cost, 3) model and analyze various developmental hydrogen storage systems including on-board system analysis, off-board spent fuel regeneration efficiency, and reverse engineering, and 4) identify interface issues and opportunities and data needed for technology development.

Overall Project Score: 3.5 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The system analysis from this independent project is a healthy way of thinking and making decisions. This activity seems to work very well throughout the entire program.
- Argonne National Laboratory (ANL) is providing excellent systems analysis support to the Hydrogen Storage sub-program with respect to the assessment of various storage approaches compared to performance targets for light-duty vehicles. Their results provide important insights on the attributes and limitations of current configurations to meet technical and cost goals. This information is very useful for making go/no-go decisions on the continuation of the storage development projects.
- This project is vital to the success of the Hydrogen Storage Program. At this stage of the program, this project's comprehensive, system-level analysis is critical to proper assessment of different storage options, development of detailed performance and cost metrics, and a creation of a firm basis for go/no-go decisions. Results from ANL's analysis will undoubtedly be important for development of optimized systems by the Hydrogen Storage Engineering CoE (Engineering CoE).
- Having a good analysis is essential to the storage effort.
- ANL serves as an honest broker for this program.
- This project has always been highly relevant.
- This work is well-aligned and important for assessing the progression towards the DOE's RD&D objectives. Having a common analysis source of the various hydrogen storage systems is very helpful to ensure consistent assumptions.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- With various collaborators, it is possible to have appropriate input for modeling. Their approach is excellent.
- The ANL approach generally considers most, if not all, of the relevant technical parameters needed to assess the ability of a given storage system to meet both the on-board and off-board refueling performance targets. They collect and update input from various sources to obtain reasonably complete descriptions of several storage systems. Their analysis methodology seems to be thorough and sound

from an engineering perspective.

- The major limitation is the lack of sufficient details on specific properties of incompletely characterized systems (e.g., reliable rates for hydrogen reaction with the storage media in the appropriate operating temperatures). The consistent application of trade off studies to determine influence of various parameters is valuable to identify which have the most impact on achieving or limiting the performance targets.
- The team has adopted a solid approach that incorporates rigorous thermodynamic and kinetic models to understand important physical and chemical storage processes from a systems standpoint. Close collaboration with CoEs and other projects is ensuring that relevant issues are addressed. However, since the CoEs serve as the primary benefactors of these analyses, the dissolution of the centers in 2010 could dilute the overall impact of project in the future. A close collaboration with the Engineering CoE will be critical.
- Employing a systematic engineering approach by ANL ensures consistency across multiple storage solutions allowing for common-ground comparisons.
- ANL has done a good job of targeting the levels of analysis that provide relevant data but avoids complexity that would make modeling efforts convoluted and expensive.
- This project is scheduled for four additional years and requires a fair amount of materials information for the analyses to be viable, useful, and correct. With many materials projects coming to an end, how will this project acquire the input it needs to function appropriately?
- The overall approach is good. The recent analysis of attribute variations with the multiple tank system will be helpful.
- The validation of results and/or detailed explanation of assumptions should increase to promote confidence in the analysis. As indicated at the AMR, the analysis of the various storage systems are at different levels of maturity. It would be helpful to provide an indicator of the analysis level and/or confidence intervals for the results.
- The project team should be encouraged to provide details on their model approach.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **3.4** based on accomplishments.

- Significant progress has been made during the past year on updating assessments of physical storage
- systems, adsorption by metal-organic frameworks (MOF), and the ammonia borane (AB)/ionic liquid (IL) chemical hydrogen storage system.
- There are many case studies for various systems investigated under the hydrogen storage sub-program.
- ANL's project has been consistently a high performer. The rating here reflects the inevitablity of a reduction in resources for 2010.
- Their analyses indicate that while compressed gas storage can meet weight targets approximately and most of the functional targets, volumetric capacities cannot be achieved. On the other hand, their analyses of the cryocompressed storage vessels appear to confirm Lawrence Livermore National Laboratory (LLNL) results that nearly all on-board performance targets can be met. Yet well-to-tank (WTT) efficiency is impacted because of the need to form liquid hydrogen.
- The team presented a good, initial assessment of the on-board performance of AB/IL chemical hydrogen storage systems that indicated thermal management will be challenging for this approach. Overall, a broad range of systems have been evaluated by the ANL team.
- The team has made good progress on understanding important systems issues in compressed H_2 storage, cold gas/liquid storage, sorption using MOF-177, and storage in AB/IL systems. Results in 2010 on MOFs and AB represent a straightforward extension of work that was initiated in 2009 on these systems. Given the extensive work conducted on the AB/IL system, presenting a conclusion about the efficacy of this material in a complete storage system would be helpful.
- In chemical hydrogen storage systems, efficient rehydrogenation remains a serious challenge. Although this was addressed in a cursory way in 2009, a more focused effort on this problem is needed. Also, a detailed assessment of risks and identifying major obstacles to satisfactory system performance are not evident in the 2010 presentation. Quantitative risk analysis information is crucial for making well-reasoned, go/no-go decisions.
- The addition of fatigue analysis is a significant improvement on capabilities.
- Analysis corroboration of cryocompressed experimental results is a significant move forward for program.

- It would have been good to see better or more information comparing the different technologies as an outcome of the work to date.
- The results from this analysis shows the project continues to make excellent progress. It is very important to have an independent assessment of the various hydrogen storage technologies.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- There are various collaborators supporting this project. In particular, major industries are contributing critical analysis.ANL worked very closely with TIAX LLC on several storage systems. Apparently, there were close interactions and exchanges of information with the Metal Hydride (MH) and the Chemical Hydrogen Storage (CHS) CoEs that led to valuable feedback to all parties.
- There is a strong collaborative effort among ANL's SSAWG team and the other COEs and industry stakeholders. A wide range of systems issues is addressed. This is being successfully accomplished through a well-managed division of effort among the partners in the project.
- ANL has identified and established relationships with a wide range of partners to ensure it has the data needed for accurate analysis.
- They appear to be working closely with the Savannah River National Laboratory (SRNL) CoE consortium.
- This project makes a good effort to involve a significant number of collaboration partners. It would be helpful to ensure the collaboration partners are able to provide their feedback to the analysis.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- This project is expected to support the Hydorgen storage sub-program significantly.
- The immediate work is fairly clear. The more important questions are how to preserve the work done here and how to effectively leverage this work with the Engineering CoE's plans.
- The reviewer fully supports performing more comprehensive analyses of the compressed and cryocompressed storage vessels that include variations in design configurations and optimization that address manufacturing constraints for safety and structural materials (e.g., carbon fibers, aluminum versus stainless stell).
- Revision of the alane slurry system should only be done if more complete and update kinetics and composition results are available from Brookhaven National Laboratory (BNL).
- The fuel cycle efficiency of regeneration AlH_3, $LiAlH_4$, and AB are based on current schemes from the MHCoE and CHSCoE teams. On the other hand, there is little need for analyses by ANL for the on-board metal hydride or adsorption systems as this work should be done by the Hydrogen Storage Engineering CoE.
- Finally, the development of advanced concepts for organic liquid carriers would be of little value unless a real breakthough material is discovered since the energy efficiency is just too low for these higher temperature options.
- The future work is clearly stated, and it addresses important systems issues. However, the description of work on metal hydrides (e.g., reversible metal hydride storage system) does not provide sufficient detail to describe what will actually be addressed in future work on those materials. A forthright statement of obstacles and challenges plus mitigation strategies is needed. Close collaboration with the Engineering CoE should receive greater emphasis
- More detailed work on regeneration costs on alane or AB, for example, with an independent industrial validation of costs would be a good addition to this work. ANL should seek committment from an energy company to validate assumptions and methods.
- The future work plan for this project has the appropriate focus and continues to build on the progression of the past modeling progress.

Strengths and weaknesses

Strengths

- This project supports the entire hydrogen program.

- ANL has developed very comprehensive analytical tools for making a detailed engineering assessment of both the on-board and off-board aspects of hydrogen storage. Their results appear to be very reliable and robust from the current, comparisons-based knowledge and experience with available prototype and demonstration storage systems. The engineering staff is talented and industrious, and they provide clear presentations of their methods and results. Analyses are based upon best available data from their sources.
- This is a well-organized, comprehensive systems analysis effort that is being conducted by a highly qualified team. This effort is providing information that supports the entire hydrogen storage community on convergence toward an optimum storage material and overall system design. There is good communication between this project and the material COEs that has been vital to overall project success.
- The project team presented a high-quality, well-focused analysis.
- This analysis from a common source is key for comparing hydrogen storage technologies.
- The project involves the collection of input from a variety of sources.

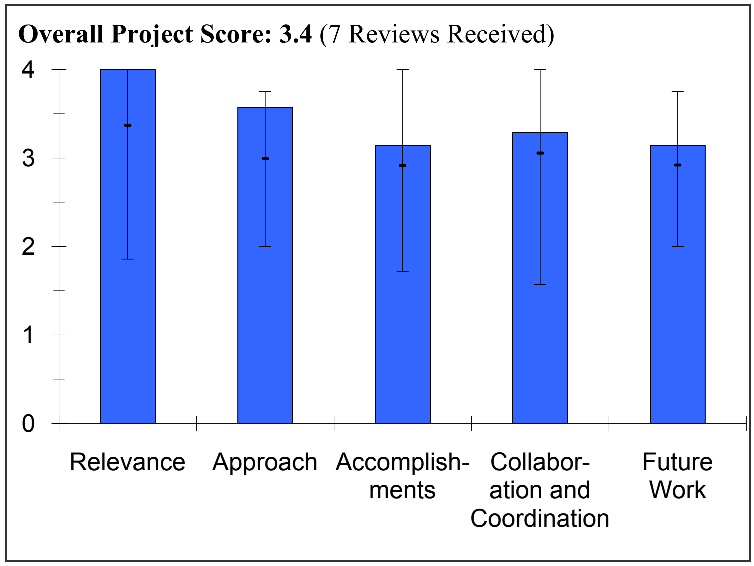
Weaknesses

- There is a lot of output. Being indepth and precise is an issue.
- In general, system analyses are only as good as the correctness and completeness of the input data used by ANL. Often, ANL has had to resort to estimates and extrapolations or use potentially unreliable input parameters, as well as somewhat oversimplified storage system designs. Predicted system performance levels can be either too optimistic or pessimistic depending on the circumstances. Further work by ANL will probably be negatively affected by the absence of updated materials and properties due to the end of the materials CoEs contributions in 2010.
- A clear and definitive statement of obstacles and system challenges as well as mitigation strategies is needed for each of the approaches being investigated.
- The PI should be strongly encouraged to publish their analyses in archival journals.
- This information is too valuable to be lost in presentations that do not convey the whole picture. While it seems like the pace of publication has picked up somewhat, it is still far too slow.
- The project team should be encouraged to document the assumptions (i.e. archival reports) and validate the results.

Specific recommendations, additions, or deletions to the work scope

- This is an important project that has provided excellent analytical information for various storage options. It is recommended to preserve some aspect of this program within the Engineering CoE.
- A summary document should be developed detailing project results and providing overall guidelines for the storage system design.
- ANL should continue to focus on comprehensive assessments of the physical storage systems in configurations that can be used in vehicles in the near term and for early market applications. They should also emphasize analyses to optimize efficiency of the off-board aspects that are related to cryogenic/liquid
- hydrogen and regeneration of spent fuel from the various chemical hydride storage systems. ANL should minimize analyses of most on-board aspects of the three materials storage systems since this effort mainly belongs within the Engineering CoE. While some overlap is useful, duplication would be wasteful. Instead, the PIs should be strongly urged to foster more interaction, even outright collaboration, between ANL and the Engineering CoE to maximize information exchange.
- It is necessary for stronger emphasis on evaluating spent fuel regeneration schemes for chemical hydrogen storage materials including alane. A better connection and more collaboration with the Engineering CoE would be desirable as well.
- This project depends strongly on the CoEs to provide technical input and be the beneficiaries of the project results. Given the pending conclusion of the CoEs, it is unclear whether this project can operate as successfully as it has so far. A plan should be formulated to address this important issue.
- It is recommended they establish a partnership with an energy company to validate both monetary and energy regeneration costs.
- This project team and the Engineering CoE should establish a formal relationship to align assumptions and avoid duplicate analysis.

Project # ST-02: Analyses of Hydrogen Storage Materials and On-Board Systems
Stephen Lasher; TIAX LLC



Brief Summary of Project

The overall objective of this project is to help guide the DOE and developers toward promising R&D and commercialization pathways by evaluating the status of the various on-board hydrogen storage technologies on a consistent basis. Objectives are to: 1) evaluate or develop system-level designs for the on-board storage system to project bottom-up factory cost and weight and volume, and 2) evaluate or develop designs and cost inputs for the fuel cycle to project refueling cost and well-to-tank (WTT) energy use and greenhouse gas emissions.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- This project estimates the costs for both the on-board and off-board aspects of large-scale (i.e., 500,000 units per year) manufacturing and operation of hydrogen storage systems. This information is significant to identify which proposed hydrogen storage methods has the potential to meet cost targets, if any. These analyses are based on detailed engineering information from Argonne National Laboratory (ANL) and other segments in the fuel cell and vehicles communities.
- The project is highly relevant and fully supports the DOE Hydrogen Program objectives. The cost and performance assessments being performed here are complementary to the storage materials development efforts within the material CoEs as well as in the Argonne National Laboratory (ANL) and Storage Systems Analysis Working Group (SSAWG) system-level analysis projects. Overall, it is providing important information that facilitates the evaluation of specific materials and systems approaches for on-board hydrogen storage.
- These analyses have been highly useful and relevant.
- The project is highly relevant and critical to the hydrogen program.
- A consistent and reliable cost comparison study is essential to gauge the commercial readiness of the program's multiple technology pathways.
- This project is highly relevant to the overall storage objectives because it concerns analysis of storage system cost. Until the Engineering CoE started participating, this project was the only source of rigorous system cost data. It is unfortunate that this project is coming to an end, since it has been invaluable for identifying and understanding cost-related barriers.
- This project provides an important assessment of the progression of various hydrogen storage systems toward the DOE RD&D objectives. It is helpful to have this analysis performed from a common source to ensure consistent assumptions.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- The TIAX approach is predominantly to use established cost analysis methods and tools based on engineering designs and manufacturing inputs from ANL, component vendors, and other sources. Costs are projected for large-scale production rates, which are subject to significant uncertainty for storage systems still in R&D development stages. The use of ranges for individual components and materials to determine the overall limits is good and is much more helpful than relying on just fixed design values. However, the analyses can be misleading for near-term, lower production rates and projected cost reductions by economies of scale when technologies are still being developed. The absence of reliable cost estimates for undeveloped materials can cause the TIAX results to be overly optimistic.

- The overall approach uses technology reviews and bottom-up modeling to calculate cost and performance for specific on- and off-board storage technologies. The approach is sound and provides an indication of the acceptability of different candidate technologies. The description of key design assumptions is an important component of the analysis, and it provides added credibility to the accuracy of the predictions.
- All the important factors and aspects have been taken into account.
- The study approach is sound and well established. The collaborative nature of seeking input from multiple partners is essential.
- Overall, the approach is well designed and focuses sharply on efficiently constructing cost models for diverse storage systems. The only area related to the approach that remains a bit unclear is how, or to what extent, the input information is vetted by technical experts and developers. Also, this approach has relied on extensive and frequent communications between TIAX and ANL, the creator of the system models and bill of materials. Given that ANL is frequently making changes to their models, it is unclear if all of these minor changes are captured in the corresponding TIAX cost models.
- The general approach is focused and effective. Even though cost models are difficult to validate, further explanation of assumptions and/or confirmation with industry partners could be provided to increase confidence in the analysis. Also, cost estimates at lower volumes would be very useful to assess potential of early-market commercialization.
- Although the project's title only names on-board hydrogen storage systems, the study presented results for both on-board and off-board hydrogen storage systems.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- During this project, TIAX has evaluated a number of hydrogen storage options in considerable detail.
- Their results seem mostly reasonable, although considerable extrapolation has been made from the current status of materials availability and processing. While there are differences in predicted costs, nearly all are very similar and well above the current DOE cost targets. Such studies are useful, but they do not directly lead to innovation or approaches that would significantly lower costs. One possible conclusion from these analyses is that no single storage option really provides a best cost alternative compared to the others. Improvement in the specifications of components and processing are apparent, but probably remain quite optimistic when compared to current status and costs.
- The 2010 effort is focusing on on-board assessments of MOF-177 sorbent materials and physical storage approaches. Solid progress was achieved on developing performance and cost metrics for systems comprising compressed and cry-compressed H_2, liquid-H_2, and MOF-177. Scale-up based on high-volume manufacturing considerations allowed predicted system performance and especially cost to be evaluated in the context of DOE targets. The high-volume manufacturing estimates assume 500,000 units per year. Although this is useful, it should be supplemented with estimates based on more realistic volumes appropriate for practical, near-term market entry.
- Since the project is now more than 90% complete, a more definitive statement about up-selected or recommended systems and approaches is needed. The project ultimately should provide input to DOE about the most highly recommended candidate among the storage systems evaluated.
- Their accomplishments and progress are very significant.
- The consistent and apples-to-apples cost comparison conducted by TIAX using the H2A model on a number of on- and off-board storage systems is valuable.
- The lack of reliable MOF-177 material cost, and even manufacturing capability, adds significant uncertainty to an otherwise impressive analysis.There needs to be a clear recommendation for the Engineering CoE or decision makers.
- The cost information for the updated MOF-based system will be very useful once the AX-21 (an activated carbon from Anderson Development Company) material placeholder cost is replaced with suitable MOF cost information data. While these updates are being made, other updates to the cost information for the MOF

system should be included – in particular, the newest ANL MOF system, which now operates at 150 bar rather than the original 250 bar operating pressure that ANL had first used.
- Also, the progress on well-to-wheels (WTW) performance is a bit unclear. These WTW energy efficiency values seem rather high and are more in-line with well-to-powerplant (WTP). For example, in the very optimistic scenario where we assume about 80% efficiency for production, about 95% for compression (e.g. 350 bar), and about 60% for fuel cell efficiency and neglecting delivery, the WTW efficiency for 350 bar would be approximately 45%, whereas the table presented lists 56%.
- The accomplishments from this project show they are making good progress in supporting the DOE's goals. There were several instances of using updated, previously performed analysis and, in general, a lot of recycled work.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- It is quite evident that the TIAX staff has worked very closely with ANL, Lawrence Livermore National Laboratory, Los Alamos National Laboratory , and others on many aspects of their assessments. TIAX has been responsive to design and configuration changes. TIAX also coordinated activities with the SSAWG members and DOE management with respect to areas of focus.
- Excellent collaboration among TIAX, ANL, the CoEs, and the SSAWG, especially in the area of WTT energy use and assessment of system life cycle issues. A well-formulated and executed division of effort among collaborators is evident. Good communication with stakeholders (e.g. the Freedom Cooperative Automotive Research (FreedomCAR) Hydrogen Storage Technology Team and other developers) aids in maintaining focus on the the most important issues.
- The project team has done an excellent job of interacting with the appropriate institutions to obtain accurate assessments.
- There appears to be a sound collaboration system in place for this project.
- It's not clear if there was a communication issue in one instance where the ANL team was not aware of TIAX's liquid hydrogen (LH_2) cost analysis results.
- It's not clear how TIAX's current proprietary cost model can be accessed by other PIs not part of the partnership.
- It is very unclear whether there are extensive collaborations with ANL and other partners. It is unclear to what extent this information is being efficiently shared with the Engineering CoE. Beyond the meetings and a final report, it would be very valuable if there were a formal process for TIAX and the Engineering CoE teams to interact and share information and ask questions.
- There appears to be a good number of collaboration partners involved in assessing this project. It would be helpful to ensure collaboration with an appropriate, cross-section hydrogen storage component and system suppliers to confirm the model assumptions. Only Quantum was referenced here.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- This is not applicable since the project is ending in September 2010.
- There needs to be an effort to preserve the work that this project has accomplished in the past few years. The transition and/or transfer of this project into the Engineering CoE would be beneficial to the hydrogen program.
- TIAX has proposed to perform and/or complete a number of analyses prior to the end of the project in September 2010. It is unlikely that it all can be done in the remaining few months especially with the departure of two key staff members working on this project. TIAX should focus on completing the physical storage materials assessments rather than try to estimate incompletely developed technologies such as alane or ammonia borane.
- The future work seems realistic for the limited period of performance that remains for this project. It will be critical for the TIAX team to offer compelling conclusions and definitive recommendations to the DOE, especially the Engineering CoE, on optimum system design(s) derived from this work. Cost analyses based on

moderate-volume manufacturing should be included to provide the DOE with cost estimates for near- and mid-term system deployment.
- The project ends in September 2010, but the future activities are very well structured.
- If it isn't already, a recommendation of the best hydrogen storage systems should be included in the final report.
- The project team should focus on completing the final reports instead of initiating new assessments this year, unless funding is extended for FY 11.
- They should prioritize findings and recommendations according to commercial readiness.
- As they mentioned in their presentation, making any and/or all updates – to the extent it is possible – to existing models is very valuable. More importantly, the culmination of all cost information and referenced assumptions in a final report is vital. The seamless transfer of this project's approach, tools, and learnings to the Engineering CoE, or other relevant projects, is imperative for this information to be efficiently used in the future.
- The next steps for this project appear to be aligned with the priorities of the analysis.

Strengths and weaknesses

Strengths
- Analyses by TIAX were closely coordinated with the engineering and design efforts by ANL and others. Both bottom- up and top-down scenarios were used to bound projected costs. Details of components and their costs were checked for feasibility when extrapolated to large-scale production levels.
- TIAX addressed off board issues with several technologies related to cryogenics and high pressure gas.
- TIAX also showed good flexibility in adapting its assessment to adhere to the changing priorities from the DOE's sponsors.
- The focused effort on cost analysis conducted in this project is crucial to optimizing the total storage system design. The TIAX team is well qualified to conduct this work; their results and conclusions could have an important bearing on the selection, development, and deployment of the most viable, near-term and far-term storage technologies.
- They made excellent independent assessments of various different types of hydrogen storage systems.
- The team presented a clear, consistent basis and assumptions of its cost analysis.
- The collaboration system is in place with key partners.
- This team is highly competent at cost modeling and has done an extremely good, efficient job taking system models from ANL and performing cost analysis for the systems.
- They exhibited very clear communication of information in presentations and making relevant comparisons to other storage technologies.
- This project is a common source of cost analysis for the various hydrogen storage technologies.

Weaknesses
- All cost estimates done by TIAX required extrapolation from current R&D activities and prototype or small-scale manufacturing volumes. Predicted costs can be unrealistic. For example, TIAX assumed that MOF-177 adsorbent would have the same materials cost as widely produced activated carbons during their assessment. Since precursors for MOF-177 are currently very expensive, it is highly unlikely that under any circumstance their costs be similar. The projected cost for hydrogen storage systems using MOF-177 is surely underestimated significantly. Even reliable cost estimates on any of the storage systems does not readily lead to a means to reduce the greatly over-target values. A close examination of technology hurdles and obstacles and how they affect the cost analyses is needed.
- There is a lack of clear prioritization and recommendation of storage systems, or showstoppers, for a given system.
- The analysis should include confidence levels for the cost estimates. The results appear to be absolute, but there is a different maturity of analysis for each system and for elements within the modeling such as purchased components.

Specific recommendations and additions or deletions to the work scope

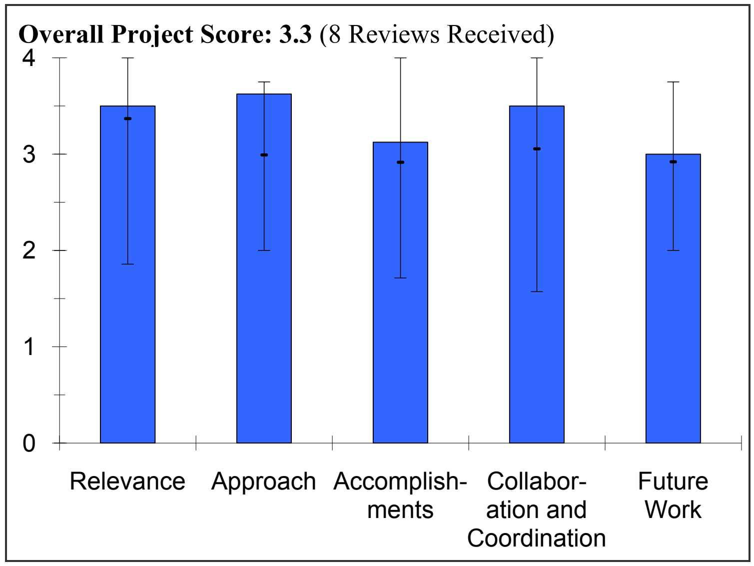
- This project has provided valuable analyses for many years. It is important to document a summary of the results and the methodology and assumptions used in the analyses.
- There needs to be an effort to preserve the work that this project has accomplished in the past few years.

- The transition and/or transfer of this project into the Engineering CoE would be beneficial to the hydrogen program.
- In the time left for this project, they should focus on on-board physical storage systems, such as compressed and cryocompressed/liquid, that could find near-term applications. In particular, they should assess the impact of much smaller production levels (i.e., hundreds to thousands per year) and alternative configurations. There is little to be gained from the incompletely defined chemical and hydride storage systems.
- A candid discussion of remaining technological hurdles and challenges for each of the technologies and how those challenges will, or might, be addressed would be useful. Although it would be difficult to do at this late stage in the project, a closer examination of alane system cost (especially spent fuel regeneration) would be informative. A more accurate assessment of MOF-177 cost is needed to properly assess the full-system cost. A cost estimate for volumes appropriate for the near- and mid-term market should be given in the final report.
- The project team should make their recommendations – good, bad, or ugly – based on current knowledge.
- See the comments on future work for scope additions in the remaining time.
- It would be helpful to have an additional estimate for each technology at a lower annual volume to assess the economies of scale.
- It is recommended to establish a formal relationship between those working on this project and the Engineering CoE to align assumptions and avoid duplicate analysis, for example, transfer of knowledge and model background.
- The project team needs to provide clear cost drivers for each technology to influence ANL system design.
- It is recommended to provide cost assessments at different capacity levels, instead of just 5.6 kg capacity level, since the cost ratio of dollars per kilowatt hour is not a linear relationship based on capacity.

Project # ST-03: Compact (L)H_2 Storage with Extended Dormancy in Cryogenic Pressure Vessels
Gene Berry; Lawrence Livermore National Laboratory

Brief Summary of Project

The overall objective of this project is to reduce or eliminate H_2 venting losses by researching vacuum stability, insulation, and para-ortho conversion. The objectives are to: 1) determine para-ortho effect on pressurization and venting losses, 2) directly measure para-ortho populations, 3) determine vessel heat transfer mechanism (radiation versus conduction), 4) evaluate vacuum stability by measuring pressure vessel outgassing, 5) test ultra-thin insulation for improved vessel volume performance, and 6) improve vessel design based on experimental results.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Hydrogen storage with sufficient volumetric and gravimetric densities for vehicular application at a lower cost is needed. Moreover, this is an original approach not pursued by others.
- This work is relevant to the Hydrogen Storage Program and DOE R&D objectives.
- Cryo-compressed hydrogen storage is a better alternative to compressed gas or liquid hydrogen storage.
- The project is relevant to the hydrogen program and the goals and objectives in the multi-year RD&D plan.
- This project directly addresses the crucial issue of on-board storage.
- The project is very relevant to the DOE program objectives. It offers a path to meeting the 2015 hydrogen storage targets.
- The reported gravimetric and volumetric densities of cryogenic hydrogen systems exceed the 2015 DOE targets. The project is attempting to optimize an actual H_2-storage vessel on board a real vehicle.
- Cryogenic H_2 storage systems are considered close to commercial reality, perhaps after the compressed gas vessel.
- Although the funding availability does not reflect it, the project is extremely relevant to the overall program objectives.
- The project is directed nicely toward the DOE goals with the appropriate emphasis on cryotank volume, weight, cost, and dormancy.
- The project has the appropriate overall focus on a high-density and low-cost hydrogen storage systems.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- It is difficult to improve this approach significantly. The project is progressing slowly but surely.
- One of the key issues is dormancy. This work aims to characterize two of the factors affecting dormancy with their studies of para-ortho conversion and vacuum stability.
- The project is focused on technical barriers such as volume, weight, cost, and hydrogen boil-off.
- The researchers have made continuous progress identifying the key needs to improve their system with minimal funding. Their work on identifying how the contribution of ortho-para conversion to extending dormancy helps to show the advantages of the system.

- The approach is good. The concept has been developed and refined for a number of years. Several iterations of the tank have been designed, fabricated, and tested. The analysis of the design and performance has been extensive. This year's effort was an attempt to reduce or eliminate venting by taking advantage of the para-ortho hydrogen conversion.
- The project's approach is sharply focused on developing compact, lightweight, and low-cost cryogenic on-board hydrogen storage systems.
- The project participants are looking at most of the critical variables in a logical and fairly complete manner.
- Their activities range from pure experimental such as para–ortho effects and insulation out-gassing, to modeling (CO_2 and cost calculations) associated with LH_2 manufacturing versus cryocompression.
- The approach with the analysis of para-ortho conversion and the benefit of dormancy was useful. Additional dormancy sensitivity studies would benefit the project such as variation in ambient conditions, initial states, and venting strategies. The project approach should expand on failure modes and noise factors that affect the system's robustness like vacuum insulation lifetime variations.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- The project is following the initial plan.
- The project team has collected good data in monitoring para-ortho conversion.
- The PI showed the results of the cost analysis, but it is not clear how they arrived at the results. Because the analysis is sensitive to assumptions, models, etc., it is important to present the models used and the assumptions that go into the analysis.
- Their progress is good and concentrated on the following:
    - Composite out-gassing research that is necessary for establishing suitable getters for long-term vacuum stability;
    - Cycling vessels with cooled gas to separate mechanical and thermal effects without compression heating;
    - Out-gassing from vacuum-cured vessels with or without ultraviolet coating;
    - Attempting to acquire pressurized cryogenic H_2 fueling capability;
    - The focus of hydrogen liquefaction as energy and capital and intensive safety issues.
- The group has met 2015 targets.
- This project has produced a working system with weight and volumetric densities that exceed the projected capacities for any other system.
- There is no other hypothetical system close to this real, working tank.
- A lot was accomplished with very little funding in FY 10. Lawrence Livermore National Laboratory (LLNL) demonstrated that dormancy can be extended by a factor of two by taking advantage of the para-ortho conversion of hydrogen. There are issues that remain with vacuum integrity under automotive conditions. Out-gassing of hydrocarbons from the composite vessel will require an effective getter.
- This data may be the first on para-ortho hydrogen conversion in a full-size tank.
- The preliminary analysis of long term vacuum pressure data does not indicate increased heat transfer even at temperatures (140-240 K) nearing ambient.
- The project has clearly demonstrated its system optimization goal through para-ortho conversion and vacuum stability to minimize hydrogen loss and extending dormancy.
- The experimental results of the para-ortho conversion factor are impressive, especially in light of the low funding resource.
- Much progress has been made. There is at least one tank design (Generation 3) that meets the DOE 2015 weight and volume targets.
- The beneficial (endothermic) effect of the para-ortho conversion in increasing dormancy is nicely shown.
- The presentation dwelled excessively on the measurement details of this natural phenomenon.
- The out-gassing of the insulation and resulting decay of the vacuum and insulating properties of the vacuum space is sobering. It is unclear whether this is the principal remaining technical barrier at this point.
- The Generation 3 cyro-compressed pressure vessel appears to have made good progress and appears to reach the 2015 targets. The project should continue to focus on developing the counter points to cyro-hydrogen

disadvantages (i.e. energy penalty versus cost) and should avoid policy discussions involving carbonless energy premiums.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- There have been serious and active collaborations with original equipment manufacturer (OEMs) on the vehicular and the reservoir sides. There has also been excellent collaboration with the system analysis group.
- They have had frequent exchanges with BMW that was very helpful to the project.
- Collaboration with BMW and pressure vessel manufacturers to create a cooperative R&D agreement (Cooperative Research and Development Agreement, (CRADA)) will be helpful.
- Working with BMW is good. Unfortunately, the U.S. OEMs appear to be uinterested in this approach.
- Collaboration with BMW is key to demonstrating the viability of this technology in automobiles. Structural Composites, Inc., (SCI) will provide tank manufacturing expertise.
- The established CRADAs with an automaker and vessel manufacturer is the right direction for the project.
- There are two good collaborations: BMW and SCI.
- It seems there should be at least one more formal collaboration in the critical area of getter technology.
- The collaboration with BMW appears to be a good effort. Although, it would be useful to include and get feedback from cyro-system suppliers that could eventually produced these systems for OEMs.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The project team focused mainly on finishing para-ortho experiments, filling with LH_2, designing fourth generation, and developing modular approach.
- Their proposed future work is fair.
- More understanding and data on para-ortho conversion is needed.
- The idea of using multiple volume vessels could significantly reduce the gravimetric and volumetric capacities.
- The proposed future work to explore performance limits of vessel and cryogenic hydrogen behavior related to shape, scale, refueling speed, and energy efficiency is good.
- The only negative here is the fact that the DOE has cut funding for this project.
- The DOE needs to increase funding for this effort to enable continued progress.
- The researchers appear eager to continue improving the system but have inadequate funding.
- A full automotive prototype tank will be built in 2011 by BMW. BMW indicated that they are developing an effective getter for compounds out-gassed by the composite pressure vessel.
- LLNL is not expecting DOE funds at least in the early part of fiscal year 2011, so future work plans are somewhat unsettled.
- The proposed future work is appropriate and necessary. However, the scope may need to match the funding level.
- The plans for future work are generally reasonable.
- It seems that relatively little continuing para-ortho measurement work is necessary to optimize the tank
- design.
- It is not at all clear how the optimum getter will be selected to handle the vacuum-jacket/out-gassing problem or whether LLNL have expertise in this area? During the Q&A session, a representative from BMW indicated a major effort was under way on the getter problem. He expressed confidence.
- The general explanation of future work seems to be clear and will build on their past progress. It may be worth including information regarding cycle robustness and degradation testing since this needs to be fully understood to gain confidence in this technology. The next steps regarding improving the out-gassing would also be very useful since this could be a significant item to develop the appropriate getter.

Strengths and weaknesses

Strengths

- It is a unique project generating lots of new knowledge. It may not be applicable for common vehicles, but many niche applications could benefit from the technology.
- They presented good experimental work.
- The PI has an excellent understanding of the problems concerning cryo-compressed systems.
- The institution and the PI bring good credentials for the proposed work.
- They have made great technical achievements, and have a good approach of blending fundamental understanding with real tank design and construction.
- H_2 liquefaction is energy intensive and costly, but total user energy and cost is what matters. LLNL estimates that the energy cost of H_2 liquefaction is outweighed by on-board storage and refueling savings with superior range and volume.
- LLNL made a case for lower user cost if a carbon premium from purchasing renewable electricity is assigned to the additional electricity needed for liquefaction when compared to compression.
- The project addresses practical, real-world engineering design challenges.
- The team has strong engineering and experimental capabilities.
- They have established collaboration with an automaker and a vessel manufacturer, which is beneficial.
- There is good scientific and engineering expertise on cryotank technology.
- LLNL has in-house ability to fabricate and test tank designs.
- The project strength is that the storage system attributes appear to be leading other technologies in the projections at this time.

Weaknesses

- There is a need for a well-to-wheel analysis and a clearer cost analysis.
- Materials issues need more focus that relate to fatigue, scale-down effect, types of fiber, and their overall affects on cost and safety.
- It is unclear if a liquid hydrogen fueling infrastructure will ever become widespread in the U.S.
- The volumetric efficiency decreases relative to tank capacity. The system looks more favorable for larger tanks.
- Customer preferences must be considered. Liquid hydrogen can result in a very inconsistent fill rate. There is also still the possibility of venting. These are things that can be overcome, but they are currently barriers.
- The discussion on carbon premium is unclear in the presentation.
- There were no weaknesses identified.
- They reported rather weak plans to solve the vacuum decay in the organics-containing insulation layer via getters.
- They are apparently relying heavily on BMW to solve the problem.
- The project needs to maintain an objective and non-bias view of the technology. This past year's effort has made progress in the area of providing data regarding the challenges and potential for cyro-compressed technology.

Specific recommendations and additions or deletions to the work scope

- The project should relate to available codes and standards for LH_2 and compressed gas vessels.
- They need increased funding. Let the researchers explore scaling for possible applications to buses and other large storage systems.
- Some funding should be made available at least to complete the development and testing of the automotive prototype tank.
- The project team should perhaps consider focusing their engineering design focus on potential early commercial applications, namely large heavy-duty vehicles , to take advantage of the lower unit cost of large storage vessel systems.
- The para-ortho measurements should be completed as soon as possible.
- Getter optimization should be a high priority.

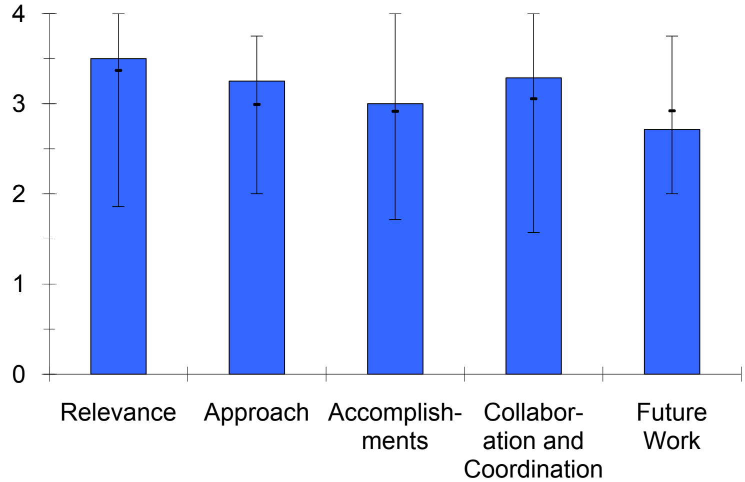
- As mentioned an earlier section of this review, the scope should include a robust analysis of failure modes and noise factors that would influence the safety or performance of the system.
- In the scope of work, the team could consider approaches and suggestions developed for codes and standards organizations regarding validating a cyro-based storage system.

Project # ST-04: Hydrogen Storage Engineering Center of Excellence
Don Anton; Savannah River National Laboratory

Brief Summary of Project

The primary technical goals for this project are to: 1) quantify the requirements for condensed phase hydrogen storage systems for light-duty vehicle applications, 2) coordinate with all other DOE hydrogen storage programs to compile their media and systems requirements and data, 3) demonstrate the technologies required to achieve the DOE hydrogen storage 2015 goals, and 4) disseminate new design tools, methodologies, and components required to develop condensed phase hydrogen storage systems. The management goals are to: 1) effectively integrate the partner's required key technical activities, 2) facilitate their collaboration; and 3) interface with external stakeholders to communicate progress and transfer technology.

Overall Project Score: 3.2 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The Hydrogen Storage Engineering Center of Excellence (Engineering CoE) is critical to the goals of the program to develop automotive capable H_2 storage systems.
  The engineering of hydrogen storage materials must be the first priority to realize the goal of the DOE Hydrogen Storage Program.
- The Engineering CoE represents the most tangible outcome of the entire hydrogen program. Not only it is one of the most relevant programs, the CoE is also a critical transitional step for the hydrogen program.
- When looking at the list of barriers addressed,it is hard to imagine that there is a deficiency of any kind in the degree to which the Engineering CoE will support the goals and objectives of DOE's hydrogen program. They are listed here:
  - A. System weight and volume.
  - B. System cost.
  - C. Efficiency.
  - D. Durability.
  - E. Charging and discharging rates.
  - G. Materials for construction.
  - H. Balance of Plant (BOP) components.
  - J. Thermal management.
  - K. System lifecycle assessment.
  - O. Hydrogen boil-off.
  - P. Understanding physi- and chemi-sorption.
  - S. By-product and spent material removal.
- The Engineering CoE is a broadly-based consortium that is addressing the critical issues impacting the successful development of an efficient and cost-effective condensed phase hydrogen storage system. The Engineering CoE draws extensively from completed and ongoing work by the other CoEs and independent storage projects. It directly supports DOE objectives and is a critical element in the overall DOE hydrogen program.

- The relevance of the Engineering CoE is questionable due to the fact that a viable material meeting program requirements has not yet been discovered. This is not a fault of the CoE but is related to work that preceded it.
- Because all of the hydrogen storage material CoEs have been terminated and none have found a suitable material that meets DOE targets for H_2 storage material, it is doubtful that the activities of Engineering CoE will generate any system-level success and therefore it is neither relevant nor critical to the overall DOE goals and targets set for the system-level, on-board H_2 storage.. The remaining money would be better spent on funding other worthwhile projects instead of continuing with the Engineering CoE..

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- How does the effect of the materials centers shutting down affect the strategy of building 0-3 prototype storage systems? The materials centers have provided guidance as to what materials are ready to be handed over. This seems different compared to what the Engineering CoE is considering. A strong variable will be the availability of materials in large quantities. Perhaps the Engineering CoE should elaborate on whether these materials will be representative of future materials. There should be some kind of sensitivity toward the general material subsets and the effect on systems.
- The Engineering CoE is performing vehicle-level modeling, which is necessary to understand the interactions with the fuel cell and the vehicle to understand system buffer and startup requirements, etc, however the project team needs to be careful with the modeling approach..
- Next year, the CoE should do a better job of describing the inter-relationships of partners and why the particular work structure was selected since having many partners doing several tasks with the potential for mission creep for everybody and the overall coordination of tasks and responsibilities is somewhat confusing..
- The Phase I go/no-go requirement of having to meet a minimum of 40% of the targets across the board seems low. Original equipment manufacturers (OEMs) need to pick carefully to select the four target have to be 100% met. It is suggested that the project team selects targets that are least dependent on material properties or that can improve on previous tank work. Also, the targets need to be more engineering-based and not materials-based.
- It is questionable if the 100g size of the proposed prototypes will be reflective of full 4-6 kg-sized automotive systems. For instance will they accurately represent heat transfer, material compression and decreption characteristics? It is understood that the systems are smaller because of the potential material availability and cost issues. The principal investigator (PI) should clearly describe these limitations and provide some kind of sensitivity analysis or projections of what larger systems will look like. A $40 million Engineering CoE should have the resources and capability to build full-scale systems. Richard Chahine at the University at Trois Rivieres has certainly built multi- kg sorbent-based systems, albeit not automotive ready.
  The funding and size of the prototype seem to be too small with only 15% of the budget will be spent on a 100-g scale prototype studies. Modeling is the major part of the project. There is much to learn from prototype studies.
- It could have been a discrepancy in the way different sub-programs have presented their work scopes, but it gave the impression that there are considerable redundancies. There appears to be multiple, parallel technical efforts in various areas of engineering design.
- It is not clear if there is duplicate work, since some of the work scope presentations were similar.
- Spider charts are a good way of measuring the state of readiness of each technology. For most long-term observers of the hydrogen program, the results are obvious. The question that was not addressed is: what pathway will make it to the prototyping stage?
- The institutions that were principal players in the Material-CoEs that started in 2005 have developed a keen sense of how to approach the issues that they unearthed during the preceding five years. The focus of effort is indeed sharp and to the point, seemingly in all respects. The organization of the Engineering CoE is all mapped out and in place, the roles and responsibilities have been spelled out, and the technical matrix approach looks like it will suit the Engineering CoE well. They used the spider graphs well. They track the whole development story for a given storage material type on one page. Also, the philosophy that go/no-go decisions require the Engineering CoE to consider and approach each of the DOE goals individually, and not concentrate only on one or two, is a good business approach.

- The approach is well formulated, and proven engineering analysis methodologies and strategies are being employed to quantify system requirements and demonstrate technologies capable of meeting the DOE system targets. The approach comprises contributions from Savannah River National Laboratory (SRNL) and partner organizations in all technology areas that are relevant to the evaluation of system design concepts and the demonstration of prototype systems. Assigning system "architects" to specific technology areas is a compelling approach to ensure that the Engineering CoE examines each technology in detail without ideas and concepts slipping throught the cracks.
- The project teams' focus on modeling coupled with experimentation is appropriate.
- Using matrix-based organization is a good fit for this CoE.
- Determining go/no-go based on targets doesn't mean a lot without cost targets. The DOE needs to provide cost targets as soon as possible. Materials regenerated off-board must include regeneration costs and forecourt costs, among others. To achieve this, the PIs may need to coordinate with other technical teams.
- The PIs have tried to map out the activity stream and required tasks and subtasks, despite the complexity of the project at hand. Non-material technology barriers have been identified satisfactorily. It is unclear how the lack of viable H_2 storage materials will affect the conclusions and deliverables of this CoE. It is also unclear why costing of the storage system was left out. Needless to say, cost plays a major role in choosing among the different designs and system approaches. Another issue is that the Engineering CoE managers have not clearly identified which four of the DOE 2010 numerical system storage targets will be met by the first go/no-go decision point. Also, it is not certain which six of the DOE 2015 numerical targets will be satisfied by the second go/no-go time, and it is unknown why cost is not one of the decision criteria considered.
- An integrated team and approach are crucial success factors for this CoE.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The CoE has progressed well in a year with most of the effort focused on modeling. Go/no-go criteria and the decision tree for what and/or how many systems will be constructed should be provided.
- There have been a lot of results but depth of the PIs' consideration is a question mark. For example, it is unclear whether the comparison of sorbents to determine whether they are commercially available was really needed.
- The program is still in its early stages. The subprojects have already provided valuable information. However, a large portion of the results presented here appear to be duplicates of earlier projects. It would have been helpful to delineate from the original work done here versus previously performed prior work.
- In reality, the project is better than good, perhaps a rating of 3+ on this metric. The results portion of the presentation is, in its present stage, a floor plan for what is to come. Work is just underway on most tasks, so progress is hard to gauge. It is fair to say that the early results, where there are results, look promising. It is anticipated that progress in the coming year will be significant in several areas.
- Solid technical progress was achieved in four of the six technology focus areas – the project is off to a good start. However, the presentation of accomplishments in the Multiple Model Level Coordination task and the Storage System Modeling task lacked sufficient detail to allow a reasonable review. The spider charts derived from the system analyses provide an excellent means to communicate the results in a concise and understandable way to the reviewers and technology community. The charts also are used to highlight problem areas where additional resources and technical efforts are required.
- This project was good for the first year.
- The acceptability envelope is a good tool. It would have been good to have seen results for various materials.
- Now, more than a year through the life of the Engineering CoE, the CoE still appears to be in flux and the accomplishments presented were limited. This is not surprising in the light of the complexity of the whole operation.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- There is good collaboration with the key partners that have built systems in the past. It is critical that the OEMs remain engaged, particularly on vehicle-level modeling and in providing key performance metrics that the CoE must consider and understand for automotive designs.
- There are three materials CoEs but only two are involved in the Engineering CoE as collaborators. Modeling is important but it is not a magic technology. Both experimental and modeling work with real materials is indispensable for the Engineering CoE to learn from.
- The Engineering CoE could benefit greatly by developing some form of collaborative work with Argonne National Laboratory (ANL) and TIAX LLC system analysis projects. It is imperative to maintain the institutional memory and build on previous experiences.
- As the last remaining CoE, the Engineering CoE has the added responsibility to maintain and preserve the institutional memory of the entire Hydrogen Storage Program. The current level of interaction and specific leveraging of the other centers are not adequate. This is an organizational gap that the DOE managers must address.
- The leadership of this project has done an excellent job of pulling together its diverse portfolio of partners, defining roles and responsibilities, and getting collaborative research under way in the past year. The plan for intra-center communications is certainly adequate. It remains to be seen how things will go with the extra communications.
- They have excellent collaborations, and cooperation among participating partners are evident. There is good division of work effort among all partners. A serious management challenge with a project of this size and scope will be the ability to effectively perform rapid mid-course corrections in a way that doesn't totally disrupt project flow.
- They exhibit good coordination and communications with partners.
- There are many entities involved – perhaps too many. It is not clear how closely these groups are working together. There was no example of close collaboration that resulted in a tangible result given that which otherwise could not have been possible. A few months before the first go/no-go decision deadline in October of this year, Engineering CoE PIs needed to present solid progress toward meeting the DOE system-level hydrogen storage targets.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- They critically need input from a materials CoE. One of the original intents and benefits of the Engineering CoE was to provide clear feedback to the materials CoEs and its scientists to better guide them in the development of materials. It is strongly recommend that the remaining independent material programs be aggregated into some kind of a materials CoE to maintain this critical information link between materials and engineering considerations
- Modeling is important but it is not a magic technology. Results from materials work and investigation of prototypes in more detail on a much larger scale is indispensable.
- The future plans for this project do clearly build on past progress, albeit mostly from the findings of the Materials-CoEs, and are sharply focused on critical technical barriers. The individual Engineering CoE partner presentations more details about future Engineering CoE plans at a task-by-task level than did the Engineering CoE overview presentation.
- The future work is a straightforward and appropriate extension of the current work already underway. It is keenly focused on addressing important engineering obstacles and issues. The major challenge in the coming years will be the integration of engineering solutions with materials that have properties that are marginal in terms of the DOE goals and objectives.
- The project needs quick incorporation of costs even without targets.
- They need to clearly define the benefits of prototyping systems for materials whose performance is far below target values for critical targets.
- It would be good to have a better understanding of proposed outcomes, such as proof of concept systems and prototypes that will be the outcomes of the work.
- The proposed future work to achieve the level of progress needed to pass the first go/no-go decision point ties with achieving four of the DOE 2010 numerical system storage targets. The remaining numerical targets having met at least 40% (an arbitrary target) or higher of the DOE target misses the no-cost data being compiled and

the no-cost targets are set. None of the materials advanced to tier one are able to furnish a system-level device capable of meeting DOE near- and long-term numerical hydrogen storage system targets.

Strengths and weaknesses

Strengths

- Engineering is key to realizing the hydrogen economy without doubt.
- This is a very strong technical team.
- OEM participation is a strength.
- The accumulated knowledge and insight of the participating institutions and individuals concerning the subject matter of this project are outstanding. The participants have engaged this project with comprehensive understanding of the issues, barriers, targets, and overall goals.
- The knowledge, data, insights, tools, and techniques developed during the years of the Material-CoEs are at the disposal of this project. The foundation for beginning the work of this project is well established.
- This is an exceptionally well-qualified team who are well organized and managed. The team is addressing a broad set of engineering challenges in the development of an on-board storage system in a timely and comprehensive way.
- This is a strong team.
- They exhibit good organization.
- Communications appear to be working well.
- A large number of well-qualified researchers and scientists are involved in this Engineering CoE.

Weaknesses

- One of materials CoE is not officially included in the activities of the Engineering CoE. [DOE note: liassons exist between the Engineering CoE and all 3 of the ending Materials CoEs, addtionally Engineering CoE partners include partners from each of the 3 Materials CoEs]
- The major activities are modeling, not prototype testing.
- The safety issues and codes and standards are not included and even collaboration on these activities were not presented at all.
- There are too many redundancies in the technical area.
- They need to develop a more organic transition from ANL and TIAX to the CoE.
- The project leaders should make an effort to prioritize their goals and objectives. Even though there is a sense that every task area is critical, the program must be prepared to deal with the prospects of budget uncertainties that are likely to plague DOE programs in the coming years. The present administration balances the funding of energy research against the need to reduce federal deficits.
- It seems that from a hydrogen program standpoint, the output of this CoE will be the final, and arguably the most important, contribution to the overall program. However, to date, no single material has the necessary thermodynamic and kinetic properties or the volumetric and gravimetric capacity characteristics to satisfy DOE goals. The excellent work on system engineering being performed by this CoE, notwithstanding, seems unlikely to produce a final system solution that meets DOE goals without additional materials development. With the dissolution of the material CoEs, a serious concern going forward is that the Engineering CoE will be limited to examining only materials with limited or not fully-tested storage characteristics that are currently in the pipeline.
- The lack of promising materials to work on is a weakness.
- This is one CoE whose time has not come yet since there are no viable hydrogen storage materials available that meets DOE H_2 storage material targets. The lack of cost considerations is a real weakness.

Specific recommendations and additions or deletions to the work scope

- The reviewer strongly recommends that the Engineering CoE increases the portion of the budget for prototype testing and invite people from the Metal Hydride CoE.
- Safety, codes and standards are critical for system development and acceptance by the society. However, this project does not contain and/or collaborate with those who engaged in these issues. It is strongly recommended that the Engineering CoE communicate, converse with, and collaborate with those people.

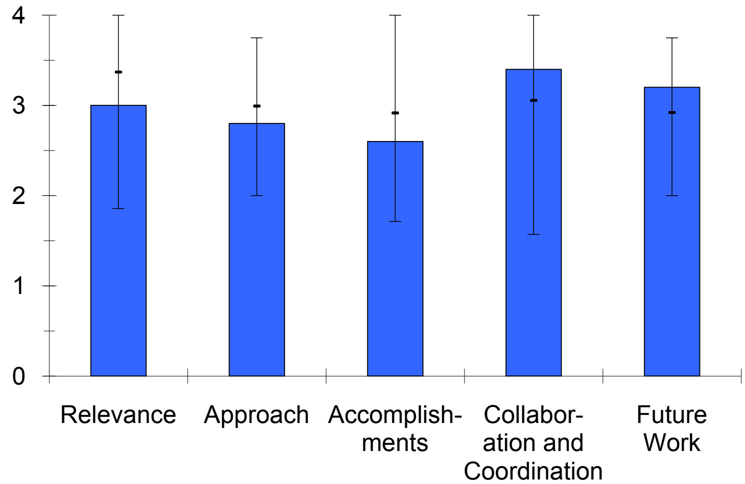
- Doing a risk assessment is also significant for applications and should be included in the objective of the project.
- As the last remaining CoE, this center has the added responsibility to maintain and preserve the institutional memory of the entire hydrogen program. The current level of interaction and the specific leveraging of the other centers is not adequate.
- It remains to be seen whether allocating 15% of resources to the prototype development and/or testing will be adequate. It may very well be enough depending on what criteria are used for the go/no-go points.
- Much of the system design engineering work has been done in parts in various projects and there should be efforts to use this knowledge.
- Since there are some duplicate efforts in subprojects there needs to be some rationalization or centralization of the work beyond what is proposed.
- Some research on hydrogen storage materials has to continue, because after five years of the Material CoEs there still is not a specific hydrogen storage material that meets the DoE's gravimetric and volumetric system targets, and other targets that haven't been demonstrated. Hydrogen storage projects that are going to continue beyond the Material CoEs must be responsive to the needs of the Engineering CoE.
- Given that the Materials CoEs are terminating this year, it is unclear how new materials ideas will be input to the Engineering CoE and how feedback from the Engineering CoE to materials researchers will take place. With the termination of the Materials CoE work, advances in emergent materials properties are unlikely to be forthcoming. It is not apparent that engineering solutions can overcome the shortcomings in inherent material performance. This issue should be addressed more explicitly by the Engineering CoE and the DOE.
- This CoE should consider formally bringing ANL and TIAX into the CoE. Their efforts are integral to what is being done. In any event, the Engineering CoE should not duplicate ANL and TIAX efforts.
- The PIs need to add cost considerations and justify continuation in the absence of viable hydrogen storage materials.

Project # ST-05: Systems Engineering of Chemical Hydride, Pressure Vessel, and Balance of Plant for On-Board Hydrogen Storage
Darrell Herling; Pacific Northwest National Laboratory

Brief Summary of Project

The overall technical objectives of this project are to: 1) design a chemical hydride hydrogen storage system and balance plant components, 2) reduce system volume and weight and optimize storage capability, fueling, and hydrogen supply performance, 3) mitigate materials incompatibility issues associated with hydrogen embrittlement, corrosion, and permeability, 4) demonstrate the performance of economical, compact, lightweight vessels for hybridized storage, and 5) guide the design and technology down selection through cost modeling and manufacturing analysis.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- As a key member of the Hydrogen Storage Engineering Center of Excellence (Engineering CoE), Pacific Northwest National Laboratory (PNNL) has a primary responsibility to develop and assess the solid phase ammonia borane (AB) as a viable hydrogen storage system that can meet the 2015 DOE performance targets. PNNL also plays a lead role in the assessment and modeling of selected metal hydrides and structural materials and/or components in support of the general Engineering CoE program. PNNL has proposed initial concepts and have performed its first phase modeling on a solid AB storage system. While this system may satisfy some targets, it looks to be a rather complex configuration by requiring transport of as-prepared fuel and spent AB pellets into and out of a reactor aboard a moving vehicle. This approach is fraught with difficulties in many aspects related to the probable combination of high pressures (up to approximately 500 bar) and temperatures possibly as high as 500 °C combined with the foaming of AB during hydrogen release. Thermal management as well as materials compatibilities look very challenging. At this time, there are reservations that PNNL can devise a practical and robust storage system based upon their current configuration for solid AB pellets.
- This project addresses many (about 10) barriers that are critical to the success of the Hydrogen Storage Program and fully supports DOE RD&D goals and objectives. There are a great many things going on in this project. Hopefully, these many pieces of the project will remain well integrated in the coming years.
- The comment about creating a system for materials with several obvious issues might be a hurdle toward reaching the DOE targets.
- It is uncertain that the end point of the research would lead to a practical storage system with practical system density, system H_2 mass fraction, simplicity, and cost.
- The storage costs are an important element with respect to enabling cost effective, on-board hydrogen storage and hydrogen vehicles.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- PNNL currently appears to have two major roles in the Engineering CoE: 1) Consolidate and assess the properties of chemical storage and/or metal hydride storage media and structural materials such as carbon and steel

containers and 2) develop an on-board hydrogen storage system based on solid AB decomposition.
- Both tasks are relevant at this phase of the Engineering CoE. It is unclear whether a truly viable storage system can be achieved based on AB decomposition because of the need for thermal control of this strong of an exothermic reaction and the physical changes that occur in AB as hydrogen is released. The role and approach of PNNL to determine the materials properties of storage media and materials seems sufficiently broadbased and thorough enough to be of value to the DOE storage program, irrespective of the challenges with solid AB storage.
- There are many different types of work going on in this project. Giving the benefit of the doubt to the team performing the various tasks, the approach seems to be appropriate and effective. It is hard to say that there is a particularly sharp focus on any one technical barrier because of all the different studies that are in progress.
- There is an issue focusing on creating a system for neat AB that suffers from impurities forming and several other issues that could be improved by using ionic liquids, for example. It would be worthwhile to consider refocusing the effort on the most promising AB systems, like AB/ionic liquids, rather than neat AB.
- The H_2 ballast tank used for vehicle propulsion during reactor warm-up is an old concept, prior to hybrid electrics and plug-in hybrid electrics. A hybrid car battery may obliviate any benefit of a hydrogen ballast tank for this energy storage need. However, for a load following functionto buffer the reactor, a ballast tank may be relevant.
- Overall, the approach is well structured, but the effort would benefit from inclusion or consultation with people who have developed prototype chemical hydride systems previously, Chrysler and Millennium Cell, in particular.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- In the nominal 16 months since work in the Engineering CoE commenced, PNNL has made good progress in developing and initially assessing an innovative solid AB hydrogen storage system. However, there are various unresolved issues with this concept relating to pellet handling, temperature management, minimizing hydrogen losses during fuel loading and unloading and operating, and the arrangement of the various components, such as storage of pellets, augers, and reactors. These issues need significant resolution prior to the go/no-go review in March 2011. A number of interesting properties on other storage media and structural materials have been collected and, at least initially, evaluated by PNNL. PNNL has not done much yet with other potential chemical storage media, namely liquid AB, alane, Lithium aluminium hydride ($LiAlH_4$), and liquid organic carriers, and these warrant more attention as back-up to the solid AB approach. In any case, there has been a significant amount of positive results at PNNL since the start of the Engineering CoE.
- Significant is the right way to describe the collective progress toward meeting objectives and overcoming one or more barriers. It was overwhelmeding to follow the number of presentation slides, including the supporting slides. Clearly, there is a lot of on-going work for the allocated funding source, possibly too much activity. Trying to tackle 10 barriers with $1.5 million per year may become problematic in the long run.
- The summary of accomplishments is more a list of what was done rather than what was learned.
- There is little indication of technical or cost-target progress mentioned in the main presentation. There is some information in the backup slide,s but it is not clearly presented.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- PNNL appears to be actively participating with its partners in the Engineering CoE as well as other research groups at ANL, TIAX, and the CHCoE. The information interchanges look to have been very productive.
- Slide 31 presents a clear picture of the roles and responsibilities for the partnering in this project. This slide gives a sense that the collaboration and coordination aspects of the project are appropriate and effective.
- More visible collaboration with ANL and TIAX is recommended, since it is not so obvious.
- There is a good list of collaborators, but cross fertilization is not obvious.
- There appears to be a good interchange with other Engineering CoE participants, but the project team should consult with former chemical hydride system developers.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- PNNL presented a strong plan for their activities in the coming year. They aggressively should prepare for the go/no-go review with respect not only to the solid AB storage options, but also the alternative systems identified on slide 35 for the AMR presentation.
- The future plans are consistent with the goals and objectives of the project. At this point in time, good is an accurate description of the planning because the tone of the planning is still in a very generalized state. As more concrete results are obtained (e.g., for go/no-go decisions), planning targets should sharpen considerably. It would be good to see more achievement statements in the future plans and less "determine this" and "complete that" type plans.
- For all AB and other non-reversible systems, special focus should be given to finding working strategies to recover the spent fuel. However, it does not seem to have the highest priority.
- The technical side of the effort is reasonably laid out, but there needs to be more emphasis on evaluating projected costs.

Strengths and weaknesses

Strengths

- PNNL has built upon its extensive AB development effort from their CHCoE research while directing their current efforts directly towards the critical engineering issues for a solid AB storage system. They have developed a viable thermal modeling effort for the solid AB storage designs. The properties for a number of structural materials have been compiled and evaluated. Good directions for next stage efforts are identified.
- The project team is well organized and ambitious.
- They exhibit strengths in engineering modeling and balance of plant.

Weaknesses

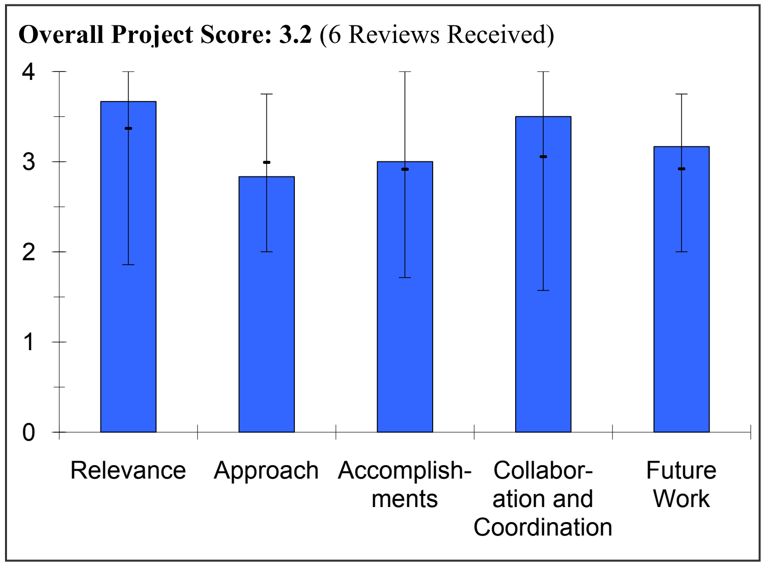
- PNNL has invested much of its manpower and resources on the complicated and risky concept of on-board storage using solid AB pellets that must be transported into a variety of locations, probably at the expense of other options. The issues related to impurities released from decomposing AB phases do not seem to have been seriously considered in their system designs.
- Their project goals are perhaps too ambitious for the allocated funding.
- The selection and focus on creating a system for neat ammonia borane selected suffers from the impurities formation, unless it is placed in scaffolds or ionic liquids. It will be worthwhile to consider the most promising AB systems, like ionic liquids-AB, rather than neat AB.
- For all AB and other non-reversible systems, special focus should be given to finding working strategies to recover the related spent fuel.
- Not assessing the risk in shipping and storing ammonia borane based on its reactivity at temperatures found in some shipping and storage environments is a weakness.
- There needs to be more emphasis on evaluating projected costs.

Specific recommendations and additions or deletions to the work scope

- More effort needs to be devoted to the other chemical storage options as the solid AB system probably will be extremely difficult to achieve for vehicle applications. Rather than devote a lot of energy into the resolution of the solid AB problems, look at requirements and benefits of these alternative approaches. In any case, the go/no-go review should include a very hard check of the feasibility of solid AB storage.
- It's hard to appreciate all the details of a project with such a broad scope in a 20-minute talk. With 36 slides to take in, it’s possible to have missed some of the important messages. Perhaps in 2011, emphasis could be placed on the most informative and compelling results.
- Do keep the project; however, its recommended that it refocuses on more promising AB systems.

- The creation of a real system and just relying on calculations is recommended.
- It is suggested that the PIs initatie more interaction among modeling partners, OEMs and engineering CoEs to accelerate model-based development.
- Do a critical, comparative assessment between the hydrogen ballast tank and hybrid vehicle battery for propulsion during reactor warm-up.
- Make a preliminary cost assessment as a high priority since having a technology that is technically feasible is insufficient.

Project # ST-06: Advancement of Systems Designs and Key Engineering Technologies for Materials Based Hydrogen Storage
Dan Mosher; United Technologies' Research Center



Brief Summary of Project

The objective of this project is to design materials-based vehicular hydrogen storage systems that will allow for a driving range of greater than 300 miles. The project focuses on metal hydride, chemical hydride, and hydrogen cryo-sorption materials for hydrogen storage.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- The project team presents a good understanding of materials compaction and stabilization, which is critical to designing automotive storage systems.
- This project addresses the DOE targets.
- This project fully supports the Hydrogen Storage Program by directly addressing key DOE RD&D goals and objectives. Specifically, it is focused on the design of materials-based vehicular hydrogen storage systems that will allow for a driving range of greater than 300 miles. How much more relevant could a Engineering CoE project be?
- For the transition of materials technology from the laboratory to real-world, working storage system prototypes are an extremely important aspect of the hydrogen program. However, there are clearly a number of vacancies in critical materials data that will need to be addressed, or should have already been addressed by the Materials CoEs. For example, the affects of material compaction on the intrinsic gravimetric and volumetric capacity was brought to the forefront three or four years ago, yet no comprehensive study was ever initiated among the CoEs to address this important property. The burden is now on the Engineering CoE to fill in the gaps without a cohesive organization of the Materials CoE.
- By its integration into the Engineering CoE, this project covers key components addressing the quantitative DOE storage targets and barriers.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- As with most of the Engineering CoE principal investigators (PIs), they have many seemingly unrelated tasks. The most important is likely the materials compaction and stabilization in the systems. This is the reason all the projects in the Engineering CoE will lose at least one point for approach.
- Their approach to design proper systems seems to be very good.
- The approach is fairly standard, and the United Technologies Research Center (UTRC) team has done system design work in a previous DOE-funded program. It is not clear what has been learned or leveraged from the previous system design and what is carried over to this program.
- There is a higher expectation from a team and/ or organization that has been involved in the same specific work and funded by the same funding agency. No new innovative engineering solutions were presented.
- This project is indeed sharply focused on critical technical barriers. Simply stated, the approach involves leveraging of in-house expertise and experience in various engineering disciplines with metal hydride system prototyping to advance materials-based hydrogen storage for 300-mile range automotive applications.

- To those on the outside, there appears to be a significant amount of repetition planned in the approach relative to experimental measurements of intrinsic material properties. While this actually may not be true, it is recommended that a matrix of missing properties be prepared so that the Engineering CoE can justify the experimental efforts and delineate this from data that may or may not already be available within the Materials CoE.
- The approach is reasonable; UTRC uses its past expertise and experience to contribute to several specific areas within the Engineering CoE, as mentioned on slide 4. It leads the efforts on reactivity and compatibility, H_2 purity, forecourt requirements, and risk assessment and mitigation on slide 5.
- There are many areas of effort listed. It is not clear that all can or will be done, and whether possible areas of duplication will be avoided.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- This project receives a good rating.
- To take sodium alanate ($NaAlH_4$) as the test material is very good idea, even if $NaAlH_4$ itself does not meet the DOE target.
- Well-defined material for fundamental studies is needed.
- The project is still in its early stages.
- There is impressive progress in several key areas:
- The project shows good progress in the area of improved volumetric capacity and thermal conductivity through compaction.
- They showed that $NaAlH_4$ is a good model material but cannot achieve gravimetric capacity targets at fast refueling times.
- The project team showed that a hydrogen purification cartridge for adsorbing ammonia (NH_3) appears viable.
- They established a framework for performance comparisons of all three hydrogen storage materials against DOE targets on a common basis.
- Good progress is shown in light of the early stage of the program.
- Although this project is relatively new, it seems like some good progress has been made so far.
- Hydride compaction has impressively increased density and thermal conductivity.
- The reactor modeling suggests that it will be difficult to reach the DOE combination of gravimetric capacity and refueling time. Although this is a negative result, it is important to know.
- H_2 impurity work in this project suggests that a disposable purifier is the best approach to remove deleterious impurities such as NH_3 from H_2 exiting ammonia borane and amide storage media. It is implied that the purity target can be met at less than one part per million of NH_3.
- The PIs'vehicle simulation modeling seems to be making good headway.
- There are other areas within UTRC's responsibilities that are not mentioned that are under progress, namely reactivity and compatibility. It is unclear whether work in these areas has started.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- The PIs are working with all the appropriate Engineering CoE researchers and OEMs.
- UTRC collaborates with a wide range of contributors of this project.
- Collaborations with other institutions and partners are well coordinated. Ford Motor Company, General Motors, Pacific Northwest National Laboratory and the National Renewable Energy Laboratory are working together on system-level modeling. The collaborations must be working well, because the progress-per-project-dollar has been excellent.
- On paper, the collaboration structure of the Engineering CoE seems excellent.
- It is not clear how strong the collaboration is. There are possible small overlaps. Has duplication been avoided so far?

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- There are a lot of tasks so it might be beneficial if they concentrate on a few of them at a time. This is symptomatic of all the partners in the Engineering CoE.
- Impurity getters may be a necessary evil at this point. As much as possible, the DOE should favor impurity prevention over filtering projects since filtering could imply the loss of backbone material like cycling capacity.
- Their work should focus on the materials compaction studies and stabilization efforts, namely aluminum mesh. These techniques and designs are the critical missing information links between the systems and materials developers.
- The future plan sounds good.
- It is unclear how much of the proposed work is actually had been completed or being replicated from the previously funded project.
- Because of the history of the previous system design work of the UTRC within the DOE hydrogen program, the PIs of this project must clarify the proposed work from the previously performed work. There is considerable duplicating of tasks within the project.
- Slide 15 of the presentation is a perfect future plan portrayal. It lists the objectives that are all well framed and appropriate and gives milestones for completion. This same slide should appear in the 2011 presentation with indications of how the project has progressed.
- They must assess a matrix of missing properties before moving forward.
- This project seems fully appropriate and within the needs and objectives of the Engineering CoE.

Strengths and weaknesses

Strengths

- The design of the system and the R&D of technologies needed for the system are the strong points of this project.
- The project team is well organized, properly focused, and productive.
- They exhibit the much needed effort to accelerate the transition of storage materials technology to useful, fully characterized storage systems.
- The project has the potential to identify critical data, such as intrinsic material properties, that may be missing, and have that data quickly generated among the four CoEs.
- This project team includes good engineering capability and experience.
- UTRC's past work modeling, building, and testing prototype hydride containers are great pluses.

Weaknesses

- Safety, codes and standards are not included in the presentation or even as a subject for collaboration.
- None discerned.
- There appears to be a large potential for redundancies in both experimental measurements and system modeling. This perception, whether real or not, needs to be addressed.
- There was no clear pathway defined on how candidate materials emerging from the Materials CoEs will be selected, and who within the overall organization will make that decision.
- There seems to be a possible overlap with Los Alamos National Laboratory (LANL) on purification beds for H_2 from ammonia borane. Hopefully, there is complete collaboration there. The PI said there are direct interactions with LANL on this.

Specific recommendations and additions or deletions to the work scope

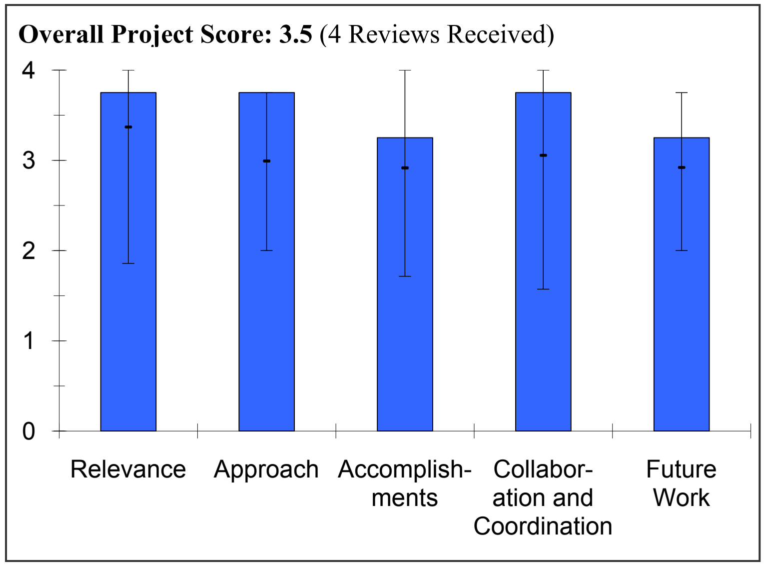
- This project should collaborate with people on the safety, codes and standards. Without these issues addressed, the system may not stand and be robust.
- Because of the history of the previous system design work at UTRC in the DOE hydrogen program, the PIs of this project must delineate the proposed work from the previously performed work. There is considerable duplicating of tasks within the project.

- It is understandable to have access to UTRC expertise and project institutional memory. As a result, the performance and expectations from this project are higher than other projects.
- The presentation was excellently planned out in terms of having the right number of slides as opposed to having way too many, as most presenters did. And, the slides presented a clear picture of what was accomplished, how it was done, and what it meant in terms of goals and objectives.
- A matrix of missing properties should be prepared so that the Engineering CoE can justify the experimental efforts and delineate this from data that may (or may not) already be available within the Materials CoE.
- The UTRC disposable purifier should be tested with a proton exchange membrane fuel cell as per the LANL approach to firmly establish its viability. During Q&A session it was stated such FC tests are planned.

Project # ST-07: Chemical Hydride Rate Modeling, Validation, and System Demonstration

Troy Semelsberger; Los Alamos National Laboratory

Brief Summary of Project



In support of the goals and objectives of the Hydrogen Storage Engineering Center of Excellence (Engineering CoE), Los Alamos National Laboratory (LANL) will contribute to modeling, designing, fabricating, and testing a prototype hydrogen release reactor for a hydrogen storage system based on chemical hydrides. Objectives for the project are to: 1) develop fuel gauge sensors for hydrogen storage media, 2) develop models of the aging characteristics of hydrogen storage materials, 3) develop rate expressions of hydrogen release for chemical hydrides, 4) develop novel reactor designs for start-up and transient operation for chemical hydrides, 5) identify hydrogen impurities and develop novel impurity mitigation strategies, and 6) design, build, and demonstrate a subscale prototype reactor using liquid or slurry phase chemical hydrides.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This project is highly relavent. Accurate fuel level monitoring of H_2 in solid state materials has been difficult and cumbersome to measure. A simple, cost-effective, and tunable system is required.
- Materials aging characteristics are important
- To investigate a system using a small-scale prototype strongly supports the Hydrogen Storage Program. Engineering success is indispensable in realizing the hydrogen economy.
- This project addresses critical issues in the implementation of chemical hydrides in a hydrogen storage system and also seeks to develop key enabling technologies for other hydrogen storage system types. The tasks of this project must be accomplished to assure that system parameters are well defined, and key enabling technologies are sufficiently well developed such that reliable systems analysis and prototype testing can be accomplished for chemical hydride-based fuel cells.
- The project addresses some areas that are critical to DOE's hydrogen program. Specifically, they are the development of fuel gauge sensors, models of aging of hydrogen storage materials, novel reactor designs, and building a subscale prototype reactor.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- The principal investigator's (PI) approach is good but it is suggested they concentrate on two topics as opposed to four, specifically in order of priority, the fuel gauge and tank integrity work are the key topics on which they should focus.
- This project contains various and significantly important pieces of technologies for the engineering of hydrogen storage materials.
- If not outstanding, this project is certainly very good and is rated at 3 or higher. The project is indeed well designed and well integrated with other closely related efforts. Each of the seven tasks focuses on a critical issue. It is doubtful, however, that all the critical issues for chemical hydride-based systems are covered, but the

ones that are covered do need to be addressed. Some of the approaches to specific critical issues are highly innovative and show a keen sensitivity to what it will take to meet performance targets for 2015 and beyond.
- The project focuses on significant technical barriers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- This project is excellent, particularly on the fuel gauge work. No PI has worked on this issue before. It is an often overlooked but a critically important requirement for constructing automotive systems. The task is not trivial.
- It is a great success to develop a homemade acoustic hydrogen fuel sensor. The shelf-life modeling study is a little bit behind the schedule but others are on or ahead of schedule.
- The project is off to a good start. Closure on some of the tasks is in sight. There is clear evidence that the project is driven by a full understanding and appreciation of DOE goals and targets. Even at what is considered to be a lean funding level, good progress is being made on a majority of the seven tasks.
- Acoustic sensors appear to be an interesting solution. Proof of concept is demonstrated.
- Current models for shelf life of ammonia borane (AB) were found to be inadequate.
- Reaction models for hydrogen release need to be refined.
- Low-temperature dehydrogenation catalysts for AB have not been found to date.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- The project team is working with the appropriate partners in the Engineering CoE .
- LANL leads the team and seems to be well organized.
- The project appears to be exceptionally well coordinated. Partnering looks to be appropriate and seemingly well linked to the individual tasks. The Material CoEs have consistently demonstrated acute attention to well-coordinated, effective, collegial collaboration, and it appears the Engineering CoE will follow that path as well.
- Collaborations among the CoEs are excellent.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- It is strongly recommended to include the fuel gauge work into future tank builds in the Engineering CoE.
- This reviewer questions whether a 100-g tank that the center plans to build be able to accommodate the project's objectives, including whether materials aging predictions be validated in small 100-g tanks?
- It is recommended that the PIs deemphasize impurity getters in favor of projects that mitigate impurities by preventing the breakdown of the storage material such as diborane and borazine, among others.
- The future work proposed is appropriate, however, the size of the prototype seems to be small.
- Proposed future work is planned in a logical manner with appropriate decision points. The barriers to progress are understood. The need to consider alternate development pathways is recognized by the PI.

Strengths and weaknesses

Strengths
- This project contains the technologies that are necessary to fabricate a hydrogen storage tank using hydrogen storage materials from a fuel-sensing system to the tank.
- This team's PI is knowledgeable, enthusiastic and fully dedicated. His poster presentation was excellent. That presentation demonstrated the kind of commitment to success that is needed for the goals of this project to be reached.
- The project team comprises a group of participating partners that are veterans in the hydrogen storage and fuel cell community.

- The project addresses a number of important issues. If it's successful, the system design will be more refined.

Weaknesses

- It seems to be a little bit uncertain that the design concept of the tank is using AB as the hydrogen media.
- At a funding level of a little more than $700,000 in fiscal year FY 10 and no clear information about what the funding will be in future years, it will be difficult to plan and manage this technically diverse project and to achieve the goals in a timely manner. With seven tasks in all, the FY 10 budget breaks down to about $100,000 per task. That doesn't buy much effort at national laboratories in this day and age.
- None.

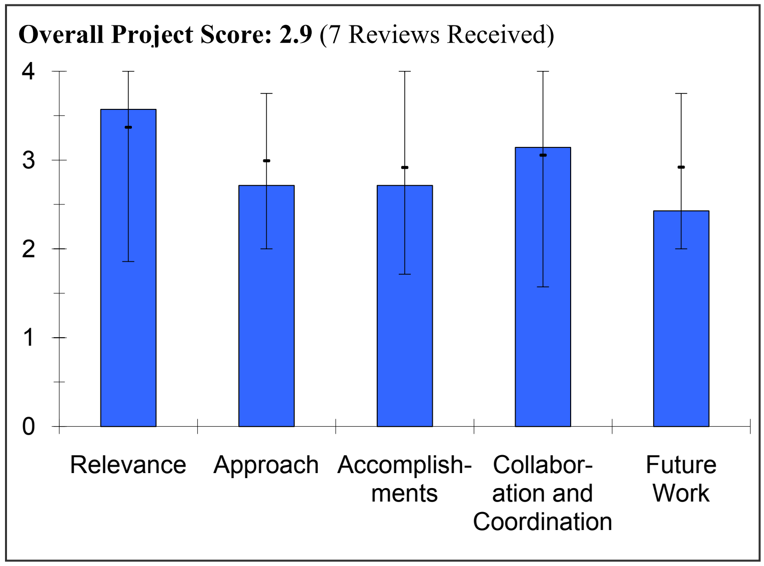
Specific recommendations and additions or deletions to the work scope

- The size of the prototype should be larger. In case of ammonia borane, regeneration of AB on a larger scale may be tested in the proper scale.
- At the present funding level, the scope may already be too broad.
- The slide presentation for this project was very comprehensive and informative. One aspect that could use more emphasis in future years is to show where each partner organization fits into specific tasks and to give an indication of their role in the task somewhere in the task description.
- None.

Project # ST-08: System Design, Analysis, Modeling, and Media Engineering Properties for Hydrogen Energy Storage

Matthew Thornton; National Renewable Energy Laboratory

Brief Summary of Project



The overall objective for this National Renewable Energy Laboratory (NREL) project is to provide system design, analysis, modeling, and media engineering properties for hydrogen energy storage. Objectives for the project are to: 1) manage Hydrogen Storage Engineering Center of Excellence (Engineering CoE) performance, cost, and energy analysis technology area, 2) develop and apply a model for evaluating hydrogen storage requirements, performance, and cost trade-offs at the vehicle system level, 3) perform hydrogen storage system well-to-wheels (WTW) energy analysis to evaluate greenhouse gas (GHG) impacts with a focus on storage system parameters, vehicle performance, and refueling interface sensitivities, and 4) assist the Engineering CoE in the identification and characterization of sorbent materials that have the potential for meeting DOE technical targets as an on-board system.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The relevance of this project is generally high. Systems-level knowledge is required to understand how to build an automotive-grade tank
- This project meets the target of the Hydrogen Storage Program at present. However, that is uncertain in the near future.
- This project aims to address some of the key barriers to hydrogen storage. The modeling is an extremely useful tool to identify how various hydrogen storage systems might behave in a fuel cell (FC) vehicle.
- This project is a key piece of the Engineering CoE. The emphasis is on performance, cost, and energy analysis, modeling and evaluation of hydrogen storage requirements at the vehicle level, WTW energy analysis and GHG impacts assessment, and identification and characterization of sorbent materials that have the potential to meet DOE technical targets for on-board systems. All of these focal points are critical to meeting DOE RD&D objectives.
- It is not entirely clear the extent to which design, analysis, and modeling will cover vehicle systems. A diagram or table of vehicle system components would have been useful here.
- High-level integration of storage characteristics into vehicular performance requirements, such as performance and emissions, is an important effort to help coordinate and integrate several different areas and to gauge subsystems' effectiveness. This type of effort is also very useful in establishing or modifying technical targets.
- This effort is part of the Engineering CoE, which is clearly oriented toward the DOE's targets and technical barriers.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- A clear explanation of the viability index was not provided. These kinds of statements are dangerous to make without OEM involvement and are likely to be considered proprietary as well. If it is based on only two vehicle types from 2004 data, this hardly seems like a representative vehicle set.
- The PI did not explain what design envelope was considered for allowing powertrain component sizing to float. For example, it was not clear whether the PI is using DOE targets for other FreedomCAR technical teams for fuel cells, power electronics, and vehicle weight. Similarly, it is not clear whether the model assumes all targets will be met or partially met or whether the model is based on current vehicle component sizing. No information was provided for vehicle-type and performance baselines.
- The WTW analyses seem outside the intended scope, are potentially mission creep, and are redundant to efforts with H2A models already in existence.
- It is not clear what vehicle modeling is. If they really wish to do so, representation from the car industry should be invited.
- The term "viability index" was used for sensitivity analysis. However, the definition of a viability index has not been shown clearly. It is a question that trades off between cost and vehicle performance and which is critical. Are these two factors really in trade-off relations and is that significantly important?
- The approach to meeting the project objectives makes sense if the investigators can get the data they need to run their models.
- It would be useful to show a plot comparing the performance of the various H_2 fuel cell systems against the performance of an electric vehicle or hybrid electric vehicle. Obviously, this could only be done later in the project, once the vehicle and FC storage models are integrated. But, this will give us a much better sense of how far away they are from a viable system.
- Not much has been done yet. At the present stage of the project, the approach is rather loosely developed. Presumably, more details about the approach will be available for presentation next year.
- No clear approach was presented.
- All tasks are well conceived toward executing the work and are well integrated with other efforts. Go/no-go decision points seem to be related to what is being assessed rather than this specific effort.
- This project serves to manage and participate in the general area of vehicle performance, cost, and energy analysis. This is an important effort within the Engineering CoE and interacts with several other CoE participants, including the OEMs GM and Ford.
- NREL is developing the important model that connects the vehicle performance and cost with the storage media parameters.
- There are several other Engineering CoE partners helping with the modeling effort. It is not clear how the others contribute to this approach.
- The project team's WTW analysis is important.
- It is highly logical that NREL is so active in adsorption media property development within NREL and among other members of the Hydrogen Sorption CoE.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **2.7** based on accomplishments.

- The project is showing good progress so far.
- The model shown here does not seem to fit the request from the car industry.
- The definition of viability index is not clearly explained.
- Preliminary results on GHG emission and WTW efficiency figures need to be improved very much and seems to be very much behind schedule.
- The project's accomplishments seem a little weak. The investigators claim they have validated models, but where are the data? They have link/run models/simulations listed under milestones for July 2009 but haven't really shown any data. The key plot that seems to be missing is a comparison of the modeling results with some real experimental data to demonstrate that the model gives reliable results.
- The project is not far enough along to gauge its success in terms of meeting objectives and demonstrating targets. Slides 7-10 don't present much of a message, other than to create the impression that the project's computational tools address the issues at a high level of detail. The viability index study stands out as a seminal, path-directing finding, but not one that is at all surprising. WTW and GHG results are just preliminary.

- There is good progress on the development of a hydrogen storage vehicle model. However, it was unclear how much of the total vehicle this modeling platform takes into consideration. For example, it is not clear whether the modeling considers drivetrain components and vehicle dynamics.
- The so-called viability index needs a clearer definition. Examples should be given to aid in the interpretation of this metric.
- The principal factors contributing to the WTW and GHG comparisons need to be defined.
- Technical accomplishments to date are acceptable and appropriate for the project schedule.
- A rather large number of accomplishments were cited.
- The Hydrogen Storage Simulation (HSSIM) model has been developed. Examples of the needed inputs and outputs were shown, but the general working mode was not disclosed in any detail. A few preliminary results were cited. Apparently, much progress is expected in the next year.
- WTW and GHG analyses have not really started, yet the preliminaries are underway. Work with thStorage Systems Analysis Working Group (SSAWG) and Greenhouse gases, Regulated Emissions and Energy in use Transportation (GREET) model has just begun, but the details of those connections were not made very clear.
- The team's progress has been especially good in understanding the potential of sorption media.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- The project team is working with the appropriate Engineering CoE and OEM partners.
- Collaboration is a little bit poor.
- The investigators seem to be collaborating well with other Engineering CoE partners. Key questions are: Where are the investigators going to get the materials data on the new materials needed to run the models? Are there any collaborations with materials groups that can supply the necessary input parameters?
- There are numerous, seemingly appropriate partners and collaborators, but the specifics of the roles and responsibilities of the individual partners were not clearly spelled out in this presentation.
- The project is well coordinated with a number of other organizations and efforts.
- Connections to the partners in the Engineering CoE, as well as the other DOE CoEs are listed in slide 2, but there is very little detail on exactly how those collaborations work. Without that detail, it is difficult to determine the true level and practical function of such collaborations.
- There is interest in the knowing more about the communication mechanisms with the partners and how successful those communications have been for the NREL modeling activities, such as the input from the OEMs GM and Ford. The Engineering CoE's general presentation and the Ford presentation did fill in some of the gaps.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- The PI should deemphasize WTW work and concentrate on modeling the storage system requirements such as buffer capacity requirements, heat flux of coolants, and fuels.
- The PI needs to show a systems sensitivity chart based on the likelihood of meeting FreedomCAR technical team targets.
- The presentation needs to show vehicle type and performance baselines that the model is based on.
- The material requirements from the engineering point of view may not be accepted by material scientists and should be in the sense of materials.
- This project is a modeling study but not a study of material development. The first description in the viewgraph of the future work cannot be understood.
- The first line under the future work section states that to meet DOE 2015 targets, new materials must be used, but it is not clear where these materials are coming from. The problem with the future work plan is really a problem with the entire Engineering CoE, not just this project. The Engineering CoE was established on a feedback principle where the input parameters for this type of modeling were coming from the materials CoEs. Now that the materials CoEs are gone, it is not clear where the Engineering CoE will get the materials properties to run the models. It seems that this project and the Engineering CoE are stuck with existing data in

the literature. It may not make sense to continue looking at a sodium alanate tank when it's already known it will never meet DOE targets.

- It's as good as possible at this time. Hopefully, in the coming year as progress is made in the various task areas, the more general nature of the plans presented at the review will sharpen. The viability index plots and spider web charts should serve as meaningful portrayals of progress.
- There is not enough information presented to clearly understand what's missing in the various modules of the vehicle model and what features exactly will be integrated in future versions. The future work for new sorbent materials, which includes the measurement of intrinsic storage capacities, kinetics, and so forth, seems redundant since most of this has already been completed.
- This is good. The future tasks are clearly identified, though some additional information regarding how they are interrelated and scheduled would be helpful. The media engineering task's future work needs more clarity relative to schedule and decision points.
- The future work plans seem reasonable. Special effort must be put forth to avoid duplication with other DOE-supported modeling activities. The possibility for good synergy is there.

Strengths and weaknesses

Strengths

- This project carries out modeling to vehicle performance from material properties.
- WTW efficiency and GHG emissions will be analyzed.
- Computer modeling will be very useful to evaluate different tank designs, especially once the model is integrated with the FC models.
- This project's staff is well qualified, informed, and dedicated.
- The project is starting from a position of strength in terms of defining and addressing the issues based on the progress made in recent years by the material CoEs and within other parts of the Fuel Cell Technologies Program.
- If total vehicle performance modeling is the goal here, then the project has the potential to quickly evaluate storage system characteristics and how they impact total vehicle mobility.
- The project team presented a good, overarching program to integrate and assess a number of other more detailed efforts.
- Their team comprises members with past experience with modeling and vehicular systems.
- They have a close connection to NREL and Hydrogen Sorption CoE sorption activities.

Weaknesses

- The materials to be analyzed are limited to sorbents, and a proper sorbent for real application does not exist. It cannot be understood that modeling to optimize the system is carried out independently for each material such as chemical hydrides, metal hydrides and sorbents.
- The accomplishments seemed a little weak. In the future, more data should be presented.
- The PIs should also present a clear plan for how they are going to evaluate new materials in the absence of the materials CoEs.
- No significant weaknesses here.
- It is suggested that, in future years, slides like 7-10 be replaced with simple graphics that give the big picture of what the modeling and evaluation studies are showing. It's certain that the codes work properly and produce a plethora of numbers. The bottom lines are what the reviewers want to hear and see.
- A clear description of the big picture relative to the vehicle modeling is lacking. This deficiency can be corrected by showing a diagram or table that lists the pervasiveness of the vehicle model and exactly what systems will be taken into consideration.
- A clear approach toward the project objectives was not described.
- The future work for new sorbent materials, which includes measurement of intrinsic storage capacities, kinetics, among others, seems redundant since most of this has already been completed. The project team needs to assess what data is actually missing before moving forward with additional analyses.
- The project's collaborations are not clearly established. They are very important for this project.

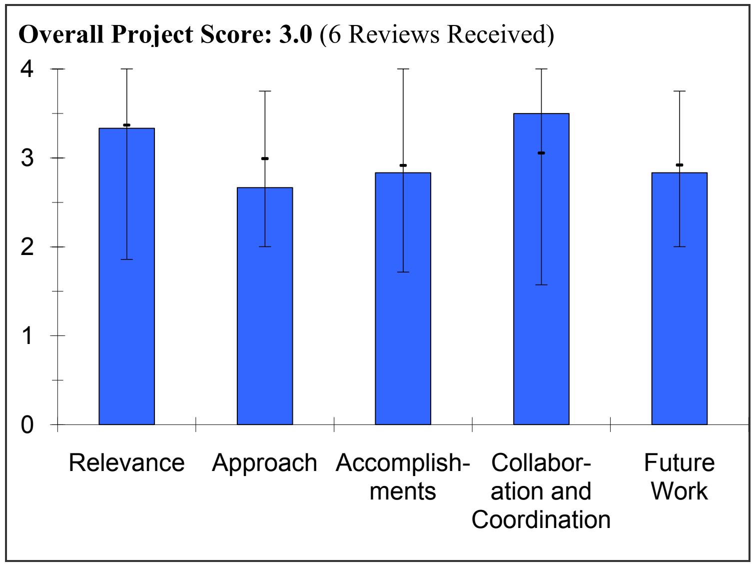
Specific recommendations and additions or deletions to the work scope

- This project tries to treat a wide range of issues, but it should be focused to a few significantly important ones.
- It seems that the Engineering CoE needs to be completely reorganized given the loss of the materials CoEs. It may be worth including some materials development work in the Engineering CoE.
- The Media Engineering Properties task requires that some efforts continue within the Fuel Cell Technologies office to develop hydrogen storage materials. The materials needed to meet all the targets on the boundary of the spider web are still not in hand.
- In future briefings, a diagram of what vehicle systems are included in the vehicle modeling would be extremely helpful.
- The project team should develop an approach detailing how each of the major goals of the project will be accomplished.
- The PI needs to assess what material property data is missing before proceeding with additional measurements.
- None at this time.

Project # ST-09: System Design and Media Structuring for On-Board Hydrogen Storage Technologies
Darsh Kumar; General Motors Company

Brief Summary of Project

The overall objective for this project is to develop systems for on-board storage of hydrogen for motor vehicles. Objectives for the project are to: 1) develop criteria for storage materials in the metal hydride and adsorbent material categories and identify storage materials in those two categories, 2) build system simulation models for metal hydrides, 3) build system simulation models for adsorbent material hydrogen storage systems, 4) explore pelletization of AX-21 and sodium alanate, and 5) work with the National Renewable Energy Laboratory (NREL), the Ford Motor Company, and United Technologies Research Center (UTRC) for integration of hydrogen storage models in a common framework with vehicle system models and fuel cell models.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Sodium alanate as a model material to develop the engineering tools is probably a good candidate for a metal hydride system. However, by using this material, there is a risk of the project experiencing "tunnel vision".
- This is a new project in the Hydrogen Storage Engineering Center of Excellence (Engineering CoE) and is only 20% complete. However, the objectives appear to be consistent with the overall goals of the Engineering CoE and are relevant. It is too early in the project to judge whether it will contribute substantially to the success of the Engineering CoE.
- This project is critical to accelerating the development of prototype storage systems that accommodate the special needs of candidate storage media.
- This project is providing guidance to the Engineering CoE regarding characteristics of metal hydride and adsorbent systems required for service on board a vehicle. It is very relevant to the DOE program objectives to develop a materials-based hydrogen storage system for transportation applications.
- This project has a logical approach to potentially increasing the practicality of hydrides and adsorbents.
- Storage costs are an important element with respect to enabling cost-effective on-board hydrogen storage and hydrogen vehicles.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The approach to design issues is standard.
- The approach proposed for this project appears to fit well in the overall scheme of the Engineering CoE with a focus on the metal hydride and adsorbent systems. Although the approach is fairly diverse involving both system modeling and pelletization of the storage media, there are questions regarding the system designs, which were the result of a joint Engineering CoE effort. This project team has to work with the design adopted by all of the Engineering CoE partners.
- After five or so years of materials R&D from the Materials CoE, it is surprising that the only materials selected under the current plan of this project are ones that are very conventional. Greater flexibility in the selection of

materials for system design and media structuring is warranted. However, studies on binder effects to improve volumetric capacity are important.
- The approach is sound. The PI identified materials properties for AX-21 and sodium alanate. Neither of these materials is of much interest for automotive applications, but they are being used as placeholders for advanced materials that may be developed, because their properties are considered representative of their class(es) and their properties are available. System simulation models will be developed for each material, then the materials will be pelletized. Finally, the model of the storage system will be integrated with the fuel cell and vehicle system models.
- The approach would be improved if a viable material(s) were identified and its properties were known and used in the analysis and simulation.
- The work plan needs to better describe the timing and phasing of the tasks. Also, the modeling work does not incorporate any model validation steps.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The PIs have worked on an alanate system under different capacities and programs. There is also ample data in open literature and within the DOE hydrogen program. There is enough data to make a comparative analysis. It would have been useful to have a comparative analysis within the presentation.
- Progress and accomplishments are minimal to date, but it is very early in the effort and perhaps the progress is acceptable. The first task was completed, but they only had an advisory role in this task. Preliminary work on pelletization is good.
- Their accomplishments appear to be consistent with stated objectives and the proposed timeline.
- The accomplishments are reasonable and consistent with the funding level and the amount of time since the project's inception. Various heating schemes were simulated to release hydrogen. A constant rate was found to be most effective of the AX-21 system. A heating rate that followed the load demand for hydrogen was found to be too complex. Both of these systems fall short of the DOE targets. This points out the need for more advanced materials.
- GM indicated the simulation models were partly validated with data from previous work.
- Compacted materials have not been used in the models. There needs to be an account of compaction characteristics and thermal expansion of the materials.
- An estimate of the maximum storage capacity of the system would be helpful to assess the potential for these systems.
- The PI shows good results in the AX-12 pelletization and with use of sodium alanate.
- The project is only showing modest progress towards overcoming barriers, and there is no indication of progress toward cost targets.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- There are many partners in the Engineering CoE, and they all appear to be working well together. GM appears to be a contributing member to the CoE.
- There seems to be some redundancy among the collaborators. The specific contributions need to be more clearly defined.
- Collaborations are strong among the members of the Engineering CoE. Automotive partners are providing guidance to the researchers regarding the applicability of approaches to the automotive market.
- There is a very good match between the analytical and the practical across the different sub-tasks.
- The partners appear to be reasonably well involved and contributing.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Plans for the future appear to be reasonable, sound and consistent with the overall Engineering CoE plans.
- The PI needs to develop a matrix of material candidates that defines how many different types of materials will be evaluated and on what basis the candidates will be selected.
- There was no discussion in the presentation regarding the criteria that must be met for continuation of the project into subsequent phases.
- It is not clear whether the remaining work will take three years to complete. A greater explanation is needed to support the schedule.
- Regarding cryo-adsorbents, it may be useful to compare the system weights and volumes to those of a pressurized cryogenic gas phase H_2 tank. The only key difference between the two is the extra mass of pressure vessel overwrap, which should be less than the mass of the adsorbent.
- The technical side of the effort is reasonably laid out, but there is still no indication of when system costs will be evaluated.

Strengths and weaknesses

Strengths
- The project staff comprises a strong technical team.
- There are strong partnerships with meaningful interactions.
- The project team has an extensive background in experience and expertise with hydrogen storage systems.
- GM certainly has automotive experience and can contribute strongly to system design and integration.
- The project properly addresses key factors in designing and modeling a hydrogen storage system, including the practical requirements for the densification of storage media.
- The integration of the storage system model with vehicle and FC models is being approached as a team effort, drawing from the expertise of several groups.
- The automotive OEMs provide guidance to the Engineering CoE.
- The collaboration and approach both seem to be well adjusted to the project's objectives.

Weaknesses
- No weaknesses are apparent yet, because the project is too new!
- There is a need for greater flexibility in the selection of materials for system design and media structuring. The current selection of materials is narrow and quite conventional.
- From the information presented, there appears to be some redundancy in at least two tasks. For example, three different organizations are assigned to address media compaction.
- From the sodium alanate simulation plots on page 6, the parasitic load from the burner is 22% to 25%, which should significantly derate the useful hydrogen density of the system and proportionally increase vehicle fueling cost.
- Only modest technical progress has been made, and no cost evaluation was offered.

Specific recommendations and additions or deletions to the work scope

- It is not clear if this work is in support of what the transport subgroup is doing within the Engineering CoE, or if there are some duplications.
- The models need to be validated as they are developed.
- Parasitic losses and heat input requirements need to be minimized.
- New storage materials with better hydrogen storage capacity need to be evaluated as they become available, if they do!
- Identifying a go/no-go decision point on the pelletization work would be advisable.
- The project team should prepare a matrix that indicates the number and type of candidate materials to be evaluated.
- The PI should assess the overall scope of the project and remove any potential redundancies from tasks involving multiple organizations.
- It is recommended that the simulation model be compared with the engineering models developed by Argonne National Laboratory for similar systems.

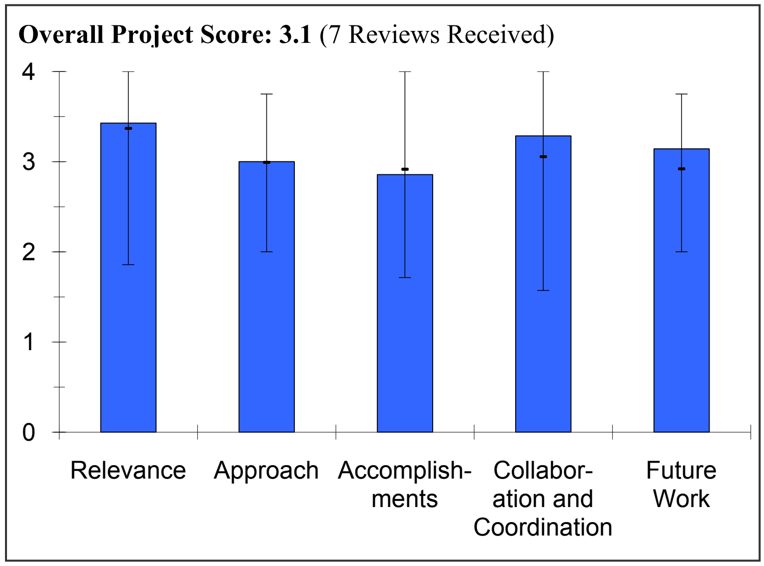
- The simulation models could be used to indicate what material properties would be required to enable the viability of an automotive system.
- The team should make a preliminary cost assessment a high priority since having a technology that is technically feasible is insufficient.

Project # ST-10: Ford/BASF-SE/UM Activities in Support of the Hydrogen Storage Engineering Center of Excellence

Andrea Sudik; Ford Motor Company

Brief Summary of Project

This project will address key technical obstacles associated with development of viable hydrogen storage systems for automobile applications. Project goals are to: 1) develop dynamic vehicle parameter model elements for the hydrogen storage system interfaces during realistic operating conditions, 2) develop a manufacturing cost model for hydrogen fuel systems based on a supply chain assessment, and 3) devise and assess optimized, system-focused strategies for packing and processing of framework-based hydrogen storage media.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project is addressing a critical issue in the storage system design using reversible carbon-based sorbents.
- This project is very new and only 20% complete. However, the project's role in the Hydrogen Storage Engineering CoE (Engineering CoE) with a focus on metal-organic framework (MOF) storage materials is definitely relevant to the Engineering CoE's objectives and DOE goals and objectives.
- This project focuses on three topics that are critical to developing commercially-viable hydrogen storage systems: system modeling, cost modeling, and materials engineering. Most project aspects are aligned with the Hydrogen Storage Program and DOE RD&D goals and objectives. The project contributes to the system-level assessment and feasibility of framework materials (FM) to meet hydrogen storage targets. To fully appreciate this project, the project team must also have some conviction that the type of FMs chosen for study have a reasonable chance of meeting the entire range of 2015 hydrogen system performance targets.
- The relationship between the Engineering CoE technical goals and the specific project goals are clearly illustrated.
- This project directly supports the objectives of the Engineering CoE. It provided vital OEM support and guidance for the CoE.
- If MOFs are going to play a role in hydrogen storage, this project will be important in optimizing the properties of the material put in the storage vessel and help in understanding dynamics of the vehicle's propulsion and its hydrogen storage system.
- The storage costs are an important element with respect to enabling cost-effective, on-board hydrogen storage and hydrogen vehicles.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The PIs are well aware of the solid packing issue of the sorbents. The heat transfer, albeit small, could have a large impact especially in cryogenic or near-cryogenic system.
- The Ford effort focuses on system and cost modeling and identifying approaches for processing MOF-based storage media. The proposed approach appears to be responsive to the task that has been assigned by the Engineering CoE.

- The PIs' project goals and objectives are clearly spelled out and seem appropriate for the type of storage materials chosen for this study. The results of this project should contribute to overcoming some, but not all, barriers to framework material implementation in fuel cell (FC) vehicles. The entire spectrum of subprojects in the Engineering CoE is replete with modeling of one kind or another. Hopefully, at some level, all the modeling efforts will connect in a complementary manner.
- Operating requirements for MOF materials will likely require feedback and/or optimization at the level of material synthesis rather than material processing and/or compaction alone. For example, crystallite size is best optimized during synthesis that may then lead to optimized compaction.
- The modeling effort seems to be duplicated elsewhere with the Engineering CoE. A clearer understanding of how this project's effort differs from similar activities within the CoE is needed.
- The approach is good and addresses several needs within the Engineering CoE. The project provides chemical and physical properties for framework materials such as MOFs. The project also will improve on FC power plant models to account for the dynamic interactions between the FC and hydrogen storage systems to determine the storage system requirements. The three tasks in the project support the activities in the Engineering CoE to understand the materials operating requirements and transport phenomena in storage systems.
- It is not clear if there are decision points in this project besides the first phase, go/no-go point for the Engineering CoE.
- The combined framework material optimization, fuel cost modeling and vehicle FC system and/or fueling optimization is a good combination of important parameters to help understand the practical potential for MF storage systems on a vehicle.
- University of Michigan should include mass transport into their modeling of packing density and thermal conductivity of MFs.
- The approach is well thought out in terms of material property assessment, material processing and the resulting uptake characteristics, and identifying the decision point before prototyping. Modeling and cost efforts are properly designed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- This project is still in its early stages. This category may not have relevance at this point.
- This project has been underway for about a year, and work on storage media processing appears to have been limited to a materials database and properties for the selected MOF identified, apparently all taken from the existing literature. Little or no original work on the storage media appears to have been done. Minimal progress on testing design and assembly has been accomplished. Some progress has been made on the modeling tasks that have been accomplished.
- Moderate progress has been made toward elucidating several hydrogen storage issues for FMs, including:
  - Aided in the development of universal modeling framework that embraces vehicle, FC, and storage systems issues;
  - Developed FC waste heat and power models that account for interactions between the H2 storage system and the vehicle propulsion system;
  - Refined their assumptions for the system's component costing matrix;
  - Delivered data sets to the Engineering CoE modeling team for one Basolite, performed property measurements for selected FMs, and initiated a study aimed at reducing variation in critical hydrogen storage measurements.
- Densification studies are just getting started.
- The project's technical accomplishments are consistent with its milestone schedule.
- Five FMs of interest are being surveyed, and data sets of the properties relevant to hydrogen storage are being compiled. The data set for MOF-5 has been compiled and delivered to the other Engineering CoE participants. The selection of a material with which to carry forward has not yet been made. Waste heat profiles for a fuel cell vehicle were assessed, and the required enthalpy of hydrogen release under these profiles has been determined.
- Their progress has been good given the duration of the project to date. The range of uptake for MOF-5, single crystal to loose powder, is quite large and does represent some risk to delivering acceptable processed material

uptake results. Modeling efforts have shown an appropriate level of progress, but the cost effort has yet to get underway.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Close collaboration with a major MOF manufacturer is critical for this project, but the PIs should keep their options open in this area.
- Having close collaboration with the developers of Lawrence Livermore National Laboratory's cryo-compress system may be beneficial.
- Interaction with other Engineering CoE partners appear to be good and working with specific project partner is good. It appears that BASF will play a major role in the project in the media processing task.
- The roles and responsibilities of the project's partners and collaborators were clearly spelled out in the presentation. The flow of modeling results and performance data to the Engineering CoE seems appropriate. Considering there are three primary partners plus the Engineering CoE partners, the annual funding seems pretty lean.
- It is recommend that the PIs' collaborations include Omar Yaghi's group from the University of California, Los Angeles.
- Collaborations are strong among the members of the Engineering CoE. The automotive partners are providing guidance to the researchers regarding the applicability of the approaches to the automotive market.
- This is a very good team of collaborators. The addition of UM for their task on framework materials will be valuable to the overall project.
- The participating organizations' roles are well defined and seem appropriate.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- Plans for future work appear to be reasonable and consistent with this project's role in the overall CoE's plan for future work.
- The project planning is sharply focused and has been carried out in a logical manner. The decision points are included in the work plan. Barriers to the realization of the proposed technology are being addressed in a seemingly unbiased and independent manner. The risk is mitigated by providing alternate development pathways.
- It will be interesting to see what densification does to FMs and whether the unit's cell-level structure is robust or whether the nano-porous framework will collapse. Also, hopefully, the binder volume and weight fractions won't be too large.
- The project team should include close interaction with synthetic and process chemists to consider other optimization schemes at the level of MOF product synthesis.
- The future work should include characteristics and operating requirements for promising FMs. If that is successful, the modeling component should provide the dynamic requirements for storage systems that have the potential to meet DOE targets.
- Information regarding the schedule of activities for this project was not presented. A high-level schedule should be provided, at least.
- The densification of FMs is very important for this project.
- The work plan has an appropriate work flow and decision points, and it shows a good balance between technical and cost target assessments.

Strengths and weaknesses

Strengths
- This project comprises a strong technical team.
- The project team has members with strong automotive experience and background.
- Strong materials expertise is coming from a project partner.

- The approach based on a fuel-cell dominant system solution is good.
- This is a strong team with well-integrated partnerships and a sufficient understanding of the barriers to reach correct conclusions about the viability of FMs.
- The project offers very well-defined goals and activities.
- The project addresses at least three areas toward developing commercially-viable hydrogen storage systems.
- This project focuses on physisorption systems, in particular MOFs, and does not attempt to diversify itself too much on other material systems.
- This project has a strong team experienced in framework materials and in dynamical modeling of FC systems. Its activities are focused on providing information in support of the phase one go/no-go decision.
- The overall team is a strength. The overall approach and interrelationship of the tasks is also a strength.
- This is a well-designed program plan with a good balance between technical and cost assessments.

Weaknesses

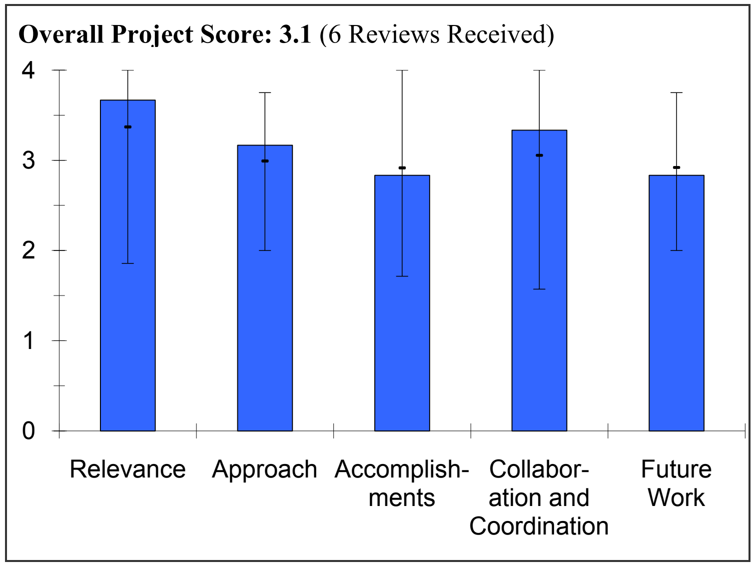
- Progress on assigned tasks needs to be accelerated to ensure that this project contributes to the overall Engineering CoE goals and objectives.
- It is not clear where this project goes if the chosen FMs fail any of the litmus tests for phase two viability.
- Their approach needs to include material optimization at the level of synthesis, not only material processing and/or compaction.
- The baseline data also needs to include adsorption and desorption kinetics that are measured according to a standard geometric configuration.
- A specific material has not been selected.
- Future work extending out to 2014 was not well defined.

Specific recommendations and additions or deletions to the work scope

- The hydrogen storage capacity of the compacted pellets needs to be validated and compared to the bulk media capacity, before compaction, as soon as possible.
- In this project, there are three tasks, three major partners, and numerous other collaborators within the Engineering CoE. Assuming only the three major partners receive the indicated funding, a figure judged to be about a $500,000 per year total, there would be less than $200,000 per task. In this day and age, that doesn't buy many labor hours in industry and at national laboratories. There is concern that this project and numerous others like it linked with the Engineering CoE are going to have a hard time making significant progress in the many task areas they propose to address.
- They should revise their approach to include material optimization at the level of synthesis.
- The project team should develop a plan to gather baseline kinetic data for MOFs.
- It is recommended that the PIs study, or show the relationship among, dormancy – a full cryo-MOF tank sitting for weeks – insulation, environmental temperature, and any hydrogen venting from cryo-tank systems designed for given MOFs.

Project # ST-11: Fundamental Reactivity Testing and Analysis of Hydrogen Storage Materials
Don Anton; Savannah River National Laboratory

Brief Summary of Project



The objective of this study is to understand the safety issues regarding solid state hydrogen storage systems through: 1) developing and implementing internationally recognized standards for testing techniques to quantitatively evaluate both materials and systems, 2) determining the fundamental thermodynamics and chemical kinetics of environmental reactivity of hydrides, 3) building a predictive capability to determine probable outcomes of hypothetical accident events, and 4) developing amelioration methods and systems to mitigate the risks of using these systems to acceptable levels.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This project is crucial for hydrides-based hydrogen storage, especially for the systems based on air- and/or moisture-sensitive, complex hydrides.
- This project addresses several of the safety issues for representative solid materials used for hydrogen storage. It combines empirical test procedures with model simulations for a couple of the more probable accident scenarios. Some possible mitigation methods that potentially reduce flammability and water reactivity hazards were proposed, although those were not explicitly identified during the AMR presentation, and initial behavior of the treatments with typical hydrides were assessed. Savannah River National Laboratory (SRNL) made relative comparisons of trends in reactivity with air, water, and other fluids found in vehicles on a few materials they considered to be representative. However, their choices were clearly not extensive.
- A comprehensive understanding of safety issues associated with solid-state hydrogen storage systems and the development of accident mitigation strategies are vital to the successful deployment of H_2/fuel cell systems using these storage media. This project addresses important safety issues in the use of several promising solid-state storage materials. System safety is recognized by the DOE as a critical element of the total Hydrogen Storage Program. This project comprises experimental testing, modeling, and risk mitigation strategies that fully support the DOE's program objectives.
- The safety of hydrogen storage media is of paramount importance.
- This project helps to fill an important niche in the practical understanding and use of solid-state hydrogen storage media, namely reactivity and safety.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The basic water-drop tests are fine for establishing a baseline understanding of these materials. The PI has made efforts to study these materials under more pertinent system conditions, for instance, in pelletized form. The focus of the project really needs to shift to achieving a better understanding of how these materials will actually be packaged in a system, and how they will be released in the case of a tank failure. It is critical that this project is closely linked toSandia National Laboratories' (SNL) ST-013 projects – the reactivity properties of hydrogen storage materials in the context of systems – for guidance.

- The approach is quite good. However alane (AlH_3) was tested for material prepared by only one method. The tested material contained metallic aluminum, which is pyrophoric and highly air sensitive on its own. Also, U.S. Department of Transportation (DOT) testing is missing.
- Most of the materials screening assessments of reactivity were qualitative to derive trends and relative rankings for a few metal hydrides, ammonia borane (AB), and activated carbon. On the other hand, various quantitative models were formulated to stimulate a couple of simplified accident conditions. While response of the tested materials to standard safety tests does confirm the greater potential of hydrides to cause more damage than the carbon or AB, the mitigation methods do offer some expectations for safer behavior upon water exposure or powder dispersion during an accident. These materials may all be too reactive to satisfy vehicle safety requirements.
- The approach comprises materials testing, model development, and risk mitigation tasks. The approach is straightforward, and it is being employed to understand the mechanisms of ignition and to test strategies for accident mitigation. Good communication with the materials CoEs is apparent; this is ensuring that the most promising candidate materials are being tested in the project.
- Standardized test procedures are being employed. Mitigation strategies are being pursued.
- Reactivity testing of candidate hydrides and hydride mixtures is greatly needed. This need is being filled here.
- The thermodynamic model to predict possible ignition from combustion and dehydriding heats of reaction, as well as other physical properties, will be very useful.
- Mitigation efforts are centered around the pelletizing of the storage media with the simultaneous addition of retardants, which is a good approach.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The PI established a good baseline data set of the material properties.
- It would be good to have some more information on the basic premise of the mitigation techniques and relative pros and cons as soon as possible. Is it similiar to the polystyrene work conducted by the SNL project? If not, what are the different mitigation strategies?
- The results on AlH3 are very incomplete. Taking into the consideration the high profile of this project and the fact that it is close to completion (90%), it will be a great disappointment if it ends before all the tests are performed. In general, the presenter has not shown too much progress for a five-year project.
  Most of the testing results for water and air exposure of the metal hydrides were done on as-prepared samples from limited (i.e., one or two) sources. It would have been much more valuable to have also assessed the behavior of cycled and desorbed materials, which are often much more reactive and sensitive to the environment. The rankings are really of limited scope. The amount of effort required to develop and to extend the simulation models for the hydrides and AB appear to have only limited value. They considered this to be rather ideal processes that may not be typical for real accidents. These models did apparently suggest some means to control the reactivity.
- Good technical progress has been achieved on the testing and modeling tasks. However, given the considerable amount of information that already exists about the ignition properties of alane upon water exposure, it is surprising that so much effort was devoted here to that work. Comparisons should be made with prior work on alane reactivity. Also, the testing of reversible storage materials should be made after several sorption cycles to determine if the reactivity of the cycled material is the same as for the starting material.
- Modeling results that were obtained regarding the ignition characteristics of $2LiBH_4/MgH_2$ as a function of pellet size are especially intriguing, and they suggest a compelling pathway to mitigate accident risk. Unfortunately, proprietary technology issues limit the extent to which the four proposed mitigation strategies can be understood and reviewed.
- The studies of ignition properties of AB were initiated in this project in 2009. The work in 2010 on cylinder self-heating is a useful extension of the earlier work, and it provides important new information that can be used in the risk analysis of systems that may employ this promising candidate material.
- The ignition and reactivity properties of only one type of alane (from SRNL by Zidan, et al., slide 7) are being studied. However, the presenter contended that there were differences that depended on AlH3 synthesized in

different ways. That needs to be definitively demonstrated. Also, the ignition behavior for powder scale as a function of the size and/or quantity of material is needed to make contact with practical applications.
- There are significant safety issues with water reaction of alane that have been identified.
- Quite a lot of reactivity data has been obtained on several candidate hydrides. It has been clearly shown that there can be substantial variations among various materials.
- Is a particle size of 40 nm much too fine for alane? Many metal powders, especially Al, would be hazardous at that particle size.
- SRNL seems to have developed a viable risk mitigation approach using pellets and additives that reduce reactivity. There is little detail because of pending patent disclosures. At least, it would be useful to know how much the gravimetric density is reduced by the incorporation of a presumably inert additive.
- The pellet model promises to be very useful.
- The United Technologies Research Center (UTRC) talk (ST-012) also uses a pelletizing approach. It is not clear how the SRNL mitigation approach differs from UTRC's.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The level of coordination has improved through the years especially now that it's part of the Engineering CoE.
- More consultation with the OEMs is still needed to understand how these materials will behave in a real automotive environment.
- This project exhibits good collaboration that is quite sufficient for such a project.
- There were substantial technical interactions of SRNL with other safety projects at SNL and UTRC, as well as with the international hydrogen storage research community. This appears to have been of benefit to all parties.
- Good collaboration between SRNL and the material CoEs in selecting the most promising materials for further study is evident. Close collaboration with selected partner organizations is apparent, especially in the areas of pellet size effect modeling (SNL) and ammonia borane cylindrical modeling (PNNL). However, noteworthy contributions in 2010 from other partners from Forschungszentrum Karlsruhe (FZK) in Germany, the Université du Québec à Trois-Rivières (UQTR) in Canada, and UTRC are not evident from this presentation. A chart showing roles and responsibilities of partner organizations would have been useful.
- There is a good level of interaction with other institutions.
- The collaborations seem to be especially good, including the international ones within International Partnership for the Hydrogen Economy (IPHE) and International Energy Agency Hydrogen Implementing Agreement (IEA-HIA).

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The focus of the project really needs to shift to achieving a better understanding of how these materials will actually be packaged in a system, and how they will be released in case of a tank failure. Questions of interest include:
  - What is the real spray pattern if the system depressurizes?
  - How much hydrogen is coming out, and what is the true particle size and distribution?
  - What binders, mitigators, and initiators are involved?
- Lithium-ion batteries can be flammable, but appropriate packaging techniques are employed as mitigation techniques. This project should look at it from the same perspective and understand how it looks in the system first before testing its properties. Heavy coordination with SNL (ST-013) is required.
- The proposed work section is too short. It is not clear what is planned to fill all the remaining gaps in a short period of time.
- The tasks identified to be performed prior to the end of the project by October 2010 appear reasonable and would provide at least some measure of completeness. It would be more helpful to the hydrogen storage and engineering teams for SRNL to assess more thoroughly the mitigation methods that apparently have had relatively limited evaluations. It is not so obvious that completing the phase three

modeling efforts would contribute significantly greater insights on the safety characteristics of the materials. Also, it is unclear how much progress can be made with the newly purchased high-pressure differential scanning calorimeter in the next two to three months remaining for this project. That is, unless this capability would be directly applied to the Engineering CoE work at SRNL.
- The future work represents a clear extension of ongoing work in 2010. However, it is not apparent whether any new materials identified by the CoEs and independent projects will be tested. Future work should include a study of the reactivity as a function of pellet size and sample volume.
- This project will end in September 2010 and is in a windup mode.
- It will continue as planned. The contract will end soon.
- Should there be specific go/no-go downselections soon for materials that are shown to be especially hazardous?

Strengths and weaknesses

Strengths
- As already mentioned, this project is crucial for metal hydride-based hydrogen storage, especially for the systems based on air- and/or moisture-sensitive materials.
- The major strengths of the project include a combination of experimental tests and modeling as well as a broad collaboration with others.
- A fairly diverse group of hydrogen storage materials were subjected to basically identical safety screening tests for water and air reactions to give a relative ranking. The concept of combining experimental tests with detailed modeling to simulate chemical reactions during possible accident conditions is attractive and should be followed in future safety evaluations. Using the results from modeling to develop mitigation methods does provide a more efficient means to improve safety.
- This is one of the few projects in the Hydrogen Storage Program that is devoted to the important issues of materials safety, risk assessment, and risk mitigation. The SRNL engineering team and their partners are well qualified to conduct this work, and solid experimental testing and modeling efforts are underway.
- The project team's focus is on the important safety-related issues.
- They have shown good, practical testing and modeling.

Weaknesses
- Taking into consideration the high profile of this project, it will be a great disappointment if it ends before all the materials selected are properly tested.
- There was no explanation for why the DOT tests are missing.
- Many of the screening tests are qualitative and difficult to systematically reproduce. Also, assessment of any storage material should have included tests on depleted and partially reacted materials before drawing conclusions on relative stability and reactivity of the prepared materials alone.
- It is not clear how the material ignition characteristics scale with the sample size. Only small samples were tested here. An analysis that extends the results to real-world sample sizes is needed.
- None.
- All the reactivity scenarios cannot be covered.

Specific recommendations and additions or deletions to the work scope

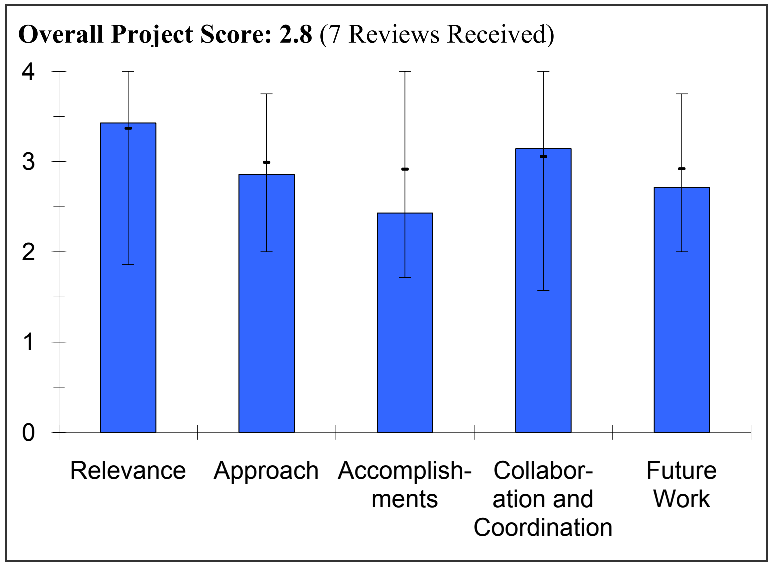
- Complete the tests and include the DOT tests.
- It is recommended that the project team use the remaining time in this project to assess the possible effects of the mitigation methods to improve materials safety. Complete the materials characterizations and include evaluations of cycled and/or depleted materials.
- A comparison of ignition results with data from prior studies would be useful.
- Specifically, a thorough review of previous work on alane ignition characteristics and mitigation strategies is needed. The results of this project should be examined within the context of work that has already been done on alane and other metal hydrides.
- None.

- Be prepared to add new, promising materials that might come from the DOE CoEs and other independent hydrogen storage materials R&D.
- The PIs should consider redoing the alane work with a more reasonable, coarser particle size.

Project # ST-12: Quantifying & Addressing the DOE Material Reactivity Requirements with Analysis & Testing of Hydrogen Storage Materials & Systems

John Khalil; United Technologies' Research Center

Brief Summary of Project



The high-level objectives of this project are to: 1) contribute to quantifying the DOE on-board storage safety target of meeting or exceeding applicable standards, 2) evaluate reactivity of key materials under development in the materials CoE, and 3) develop methods to assess and reduce risks. The primary tasks include: 1) risk analysis, including qualitative risk analysis (QLRA) to develop a broad range of scenarios and quantitative risk analyses (QRA) for key scenarios, 2) dust cloud material testing with standard and modified American Society for Testing and Materials (ASTM) procedures, reaction kinetics material testing for air exposure and/or time resolved X-ray diffraction, 4) material-based risk mitigation, and 5) risk mitigation for subscale prototypes.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The project supports the Hydrogen Storage Program and the DOE's goals and objectives. It is an important component of the materials evaluation process.
- This project addresses accident risk factors and the probability for accidents with respect to the safety of on-board hydrogen storage system based on solid media. Nearly all reported efforts were for metal hydrides, specifically $NaAlH_4$. Extensive effort was shown in fault-tree analysis for a specific accident scenario with a metal hydride. However, little attention seemed to be given to other media such as activated carbon adsorbents or ammonia borane (AB). $NaAlH_4$ beds as analyzed are very high safety risks with only minimal attention apparently given to potential mitigation schemes. An experimental assessment of powders during "blow down" testing could be useful in rating relevant risks of different storage materials.
- Finding safe and compact ways to store hydrogen is very relevant to DOE's goals. The data obtained through the materials tests and prototype testing in this research should be very useful in determining which materials to use for hydrogen storage.
- A quantitative analysis of risks and failure modes for a range of hydrogen storage scenarios, and an assessment of risk mitigation strategies are vital to the hydrogen program. This project is directly relevant to DOE objectives.
- This risk assessment study is highly relevant to the safety of hydrogen storage systems.
- This project directly addresses DOE objectives to evaluate and mitigate safety aspects of hydride storage tanks.
- It is a needed and critical effort to establish whether solid-state materials are safe to use in public hydrogen transportation scenarios.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- The PIs presented a reasonably good combination of theoretical and experimental data.
- The primary effort has been the development of qualitative and quantitative risk analysis (RA) models for

reversible storage media, particularly metal hydrides such as $NaAlH_4$, supplemented with some materials characterization and testing of powder flammability and reactivity with air and/or water. Detailed RAs were focused on a limited set of presumed high-probability accident scenarios such as the rupture of a hydride tank or inward leaks of air. While such RAs can be very involved, they are highly dependent on the validity of the input parameters that are usually extremely difficult to determine with accuracy and reliability.

- The quantitative risk analysis, using the event tree model for an external fire or the fault-tree model for in-vessel air leakage, is interesting and should yield some useful results. There are, however, some questions concerning the material reactivity risk mitigation tests on $NaAlH_4$ + 4 mole% $TiCl_3$ that should be addressed. In testing the loose powder, an unspecified amount of water or windshield washing fluid was dropped onto the $NaAlH_4$ causing a fire. Then, when a one-gram "wafer" of $NaAlH_4$ was dropped into 25 milliliters of washing fluid, there was only a mild reaction without flame. How much water was dropped onto the powder, and why wasn't this same procedure used for the wafer? Dropping a wafer into a relatively large quantity of liquid is not the equivalent to dropping some liquid onto dry powder. An identical process should be used for all tests before any legitimate comparisons can be made.
- A risk analysis framework has been developed that includes work on materials testing, modeling, accident mitigation, and prototype testing tasks. The approach efficiently addresses critical safety issues in a straightforward and detailed way. The use of fault-tree analysis methods for accident and risk prediction is a solid addition to the project. However, the accuracy of the results depends strongly on the correct assignment of branch probabilities, and the method for assigning preliminary probabilities is questionable. Experimental studies are largely limited to fast blowdown and/or depressurization tests. However, this addresses only a limited subset of possible accident scenarios.
- There is a need to complement the expert panel with data where possible (e.g. National Transportation Safety Board (NTSB) data for initiating events).
- This presentation needs to be made clearer to the non-specialist audience.
- The relevance of all the experimental efforts to this project was not well understood. Much of it appears to duplicate work being done elsewhere in the Engineering CoE.
- This project seeks to quantify the risks associated with one hydrogen storage system that has already been well studied and characterized.
- Once the system's risk probabilities for various accident scenarios have been calculated, will they be compared to actual system failure tests?
- The project objective is to develop detailed fault-tree analysis models of possible accident events such as rupturens and spills. In that sense, it importantly goes beyond the SRNL project (ST -011) to cover the whole storage system.
- The expert panel should be very useful to this effort.
- Some of the storage media testing and mitigation approaches overlap a bit with the SRNL project, but coordination seems to be largely avoiding overlap (e.g. dust clouds at UTRC versus piles at SRNL). Where there is H_2O contact testing, UTRC seems to be focusing more on $NaAlH_4$, a hydride they have used extensively in prototype tanks.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- The elaborate calculations produced reasonable-looking conclusions. However, the recommendations developed by theorists require an additional review. For example, the compaction of $NaAlH_4$ powder can have a different-than-predicted effect. If exposed to water and/or moisture and oxygen, the material on the surface of the pellet may form a dense crust containing sodium peroxides. This effect could cause serious problems, because they can explode in contact with the remaining metal hydride once the crust is destroyed as the exposed material is removed from the damaged container. The content of peroxides in compacted material may exceed the powder's content where peroxides can be easier eliminated because of the presence of water and metal impurities. This explains the "fair" rating.
- Also, X-ray diffraction results on slide 23 should be normalized to NaCl (sodium cloride) as an internal reference - the NaCl content remains unchanged during the cycling. In such a case, the concentration of $NaAlH_4$

does not increase with cycling. On the contrary, the concentration of trisodium hexahydroaluminate (Na_3AlH_6) does slightly increase. Apparently, the chemistry of cycling experiments requires an additional evaluation.
- While UTRC has performed an extended RA fault-tree simulation for the in-leak of air into a hydride bed, this is just one of various potential accidents, each with different weighting factors. Without a support test matrix assessment, it has really limited predictive capability for vehicle accidents and safety. Most measurements of reactivity appears to have been performed on $NaAlH_4$ and not other types of solid storage options. The development of a blowdown test facility looks like a potentially useful tool to assess the behavior of various materials during the remainder of the project.
- A good presentation of the various approaches used to evaluate, quantify, and assess risk is given. However, there are relatively few results. There are some results for the material reactivity risk mitigation tests on $NaAlH_4$ + 4 mole% $TiCl_3$, but no results are presented for tests done in the fast depressurization apparatus. The results on the simulations are interesting and could be useful.
- Although the fault-tree analysis is a useful protocol for assessing accident initiators, the results are difficult to understand based on the information provided in this presentation. The principal results of the fault-tree and sensitivity analyses, as well as the stochastic simulation results, need to be presented in a clear, concise way. Also, all branch probabilities in the fault-tree analysis are presently stated as being preliminary. However, the assignment of probabilities based solely on the advice from experts is flawed. Since the credibility of the entire analysis can be traced directly to the accuracy of those probability assignments, a much more careful and quantitative approach is needed. What approach will be used to assign final probability values?
- The fast blowdown and depressurization approach for assessing results of accidental breach of the hydride reaction vessel can potentially provide important information. However, results from those tests weren't presented. Results on the cycling of $NaAlH_4/TiCl_3$ are presented after the description of the fast blowdown test system. These results have been presented previously by other investigators, yet there is no apparent relationship to the blowdown studies. It is not clear why those results are presented here.
- The model is developed and the probabilities are populated.
- How are results from the experimental work incorporated into the risk assessment analysis?
- The project's progress has been good.
- The fault-tree model seems to have been developed with much detail and rational thought.
- UTRC is in a good position to start the important dust cloud tests soon..
- The pelletizing work has been very useful and practical. There may be a little overlap in this area withSRNL.

<u>Question 4: Technology transfer/collaborations with industry, universities and other laboratories</u>

This project was rated **3.1** for technology transfer and collaboration.

- The project involves an outstanding group of experts. However, a collaborator with chemical expertise may strengthen the team and the project.
- UTRC has interacted with several organizations regarding safety issues, risk testing, and materials properties. These appear to have been of mutual benefit. It would be good if the safety data from UTRC can be communicated to the organizations involved with developing codes and standards for hydrogen storage and fuel cells.
- There is evidence that this group has active partners and collaborations both within and outside the CoEs. These seem to be appropriate and should continue.
- Although many collaborators and expert panel members are listed, their specific roles and what they actually did on the project are not apparent from the presentation. The involvement of Kidde-Fenwal on dust cloud testing is an important addition to the project. However, only limited results are presented.
- The PIs need to involve other partners, like OEMs and NTSB, in the risk assessment effort.
- Why is there no OEM involvement?
- The project team's collaboration with others has been excellent and well focused.
- Communication with the other two DOE reactivity laboratories, SNL and SRNL, seems to be adequate.
- Many other good collaborations including international organizations IPHE and IEA-HIA are included.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated **2.7** for proposed future work.

- It is strongly recommend that the project team do experimental testing only after a reality check of theoretical results.
- UTRC has identified two primary tasks to perform during the project: 1) Continue to develop and expand the RA methods for reversible hydrogen storage and 2) conduct various reactivity experiments and characterization using the blowdown test facility. It was not clear how many different materials and their variations based on source and processing conditions can be assessed during the remainder of the project. The later work is probably much more valuable to the DOE Hydrogen Storage Program.
- In particular, risk model simulations are highly dependent on input assumptions and relative probabilities for different scenarios.
- The work that is planned seems appropriate. The tests that are planned on material and system testing and mitigation have already begun and should continue. The quantitative risk analyses on accident sequences are also appropriate.
- The future work is a continuation of the technical effort currently in place. A risk analysis for off-board regeneration is an important addition. A more definitive and quantitative method for assigning branch probabilities is needed to ensure that the analyses are credible.
- This work needs to focus more on developing realistic probabilities for events by seeking external data rather than relying on an expert panel.
- The project's future plans look good.
- Most of the PIs' future efforts listed will be clearly useful.
- The value of $NaAlH_4$ developed from past prototype experience is understood, but it seems this material has a spectrum of problems, including safety that suggest it should be abandoned in favor of more promising, modern alternatives.

Strengths and weaknesses

Strengths
- Having a good combination of theory and experimentation is a strength.
- Another strength is the combination of detailed, formal RA simulations of accident scenarios with performing selected materials reactivity under various conditions.
- The fast blowdown depressurization rig is well designed and should eventually produce some useful data.
- UTRC has considerable experience and expertise in hydride system safety issues, analysis of system failure modes, and exploration of mitigation strategies. Properly applied, the fault-tree analysis is a useful way to examine various accident scenarios.
- The model is developed.
- The system risk probability approach is interesting.
- The project's research includes past material and prototype experience.
- The project shows good engineering and statistical abilities.

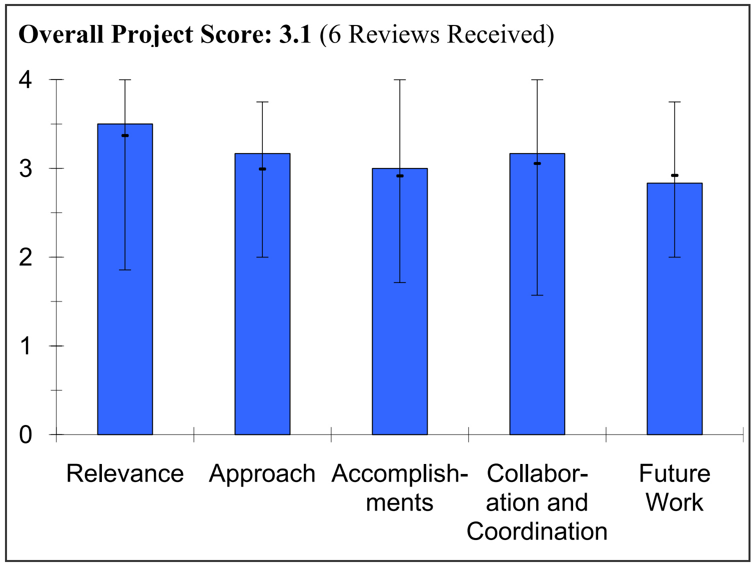
Weaknesses
- The project is weak on the chemistry side. It is strongly recommend that the PIs involve a chemistry partner(s) into the safety tests design.
- The RAs are susceptible to subjective assignments of various risks and their probabilities that are often very difficult to validate. Only a limited number of accident scenarios were actually evaluated in detail.
- The approach used to compare the material reactivity risk mitigation tests on $NaAlH_4$ + 4 mole% $TiCl_3$ should be re-evaluated to make sure that all comparisons are being made under similar conditions.
- Only limited information is supplied about how probabilities are assigned in the fault-tree analysis, and how uncertainties in those probabilities are determined. This information is critical in establishing the viability of the analysis.
- So far, the experimental efforts are the project are extremely limited. At this stage of the project (about 65% complete), it is expected that the experimental work would be further along, especially since it is apparently being used to provide input to the quantitative studies on the project.
- There is a lack of validated data for model.
- The project team needs more experimental data to assign risk probabilities to each individual event.

- None really significant.

Specific recommendations and additions or deletions to the work scope

- It is recommended that the UTRC limit further detailed RA simulations on various accidents, and instead prepare thorough documentation on their approach and results obtained to date. This information should be archived with the DOE and made available to the general hydrogen fuel cell R&D community.
- It is suggested that UTRC concentrate on performing the blowdown tests on $NaAlH_4$ and as many other materials as possible. Also, the tests should be performed on samples of cycled and depleted materials, as these are often much more chemically reactive than starting hydrides.
- The PIs need to provide more detail about the assignment of final probabilities and uncertainties in the fault-tree analysis. A risk assessment of the fault-tree procedure itself is needed. The team should accelerate experimental work using the blowdown and depressurization system.
- They should incorporate real data into the model.
- This project needs more emphasis on risk mitigation based on experimental work.
- The PIs should consider terminating the $NaAlH_4$ work in favor of other, newer materials.

Project # ST-13: The Reactivity Properties of Hydrogen Storage Materials in the Context of Systems
Daniel Dedrick; Sandia National Laboratories



Brief Summary of Project

The overall objective of this project is to understand the behavior of hydrogen storage materials and systems in preparation for deployment. The objectives are to: 1) enable the design, handling, and operation of effective hydrogen storage systems for fuel cell applications, and 2) provide a technical foundation for eventual codes and standards (C&S) efforts.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Gaining knowledge of the environment and conditions under which materials will be exposed is critical to understanding how to design systems for safety and performance.
- New materials can't be used for practical applications unless they are safe. So, the focus of this project on the safety of hydrogen storage materials is obviously very relevant to DOE objectives.
- Understanding and optimizing the behavior of solid-state metal hydride materials in an operational fuel cell environment are important components of the overall Hydrogen Storage Program. This work directly supports DOE RD&D objectives.
- The materials being tested have little chance of commercial use. Although the methods developed may be used for commercial materials, the experiments will have to be repeated.
- This project is focused on the safety of hydrogen storage material systems.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The project's overall goals are appropriate and on track.
- The PI should also consider how material packing in different support structures and thermal management equipment will affect their release and properties, for instance are the materials packed in a honeycomb structure as proposed by Savannah River National Laboratory's (SRNL) ST-011 or in an aluminum mesh such as Van Hassell's project ST-006?
- The project is well thought out and is organized into three tasks: characterize behavior, consequence analysis, and mitigation strategies. The first and second tasks represent a good approach. However, using the mitigation methodolgy in the third task is strongly dependent on materials properties that could vary widely. So, it's recommended that the PI not use this materials-based approach within this project.
- The experimental methods are sound and should continue. The residual gas analyses should also be incorporated into the project to test for gaseous decomposition products.
- A broad-based approach encompassing system design and definition, characterization of material behavior, hazard and consequence analysis, and risk mitigation and accident prevention has been adopted. This provides a solid, nearly end-to-end examination of critical elements that enable the future deployment of metal hydride storage technologies.
- Their modeling efforts are a good contribution that will be applicable to future materials and systems.
- The focus is on identifying mitigating strategies for high-risk scenarios in selected metal hydride materials.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The project shows good progress, especially in developing mitigation materials such as the polystyrene mixes. The PI should determine if these materials will be stable in high-temperature environments. Obviously, these approaches need much more investigation to understand issues such as material compatibility. Such work should be coordinated with SRNL's ST-011 project.
- Some useful results have been obtained. Mitigation of sodium alanate ($NaAlH_4$) was attempted through the use of a polystyrene matrix. The results indicated that the mitigated samples were less reactive than the neat samples. Cycling studies resulted in no loss in activity of the $NaAlH_4$. Results indicate that it might be possible to tailor the matrix. The results are interesting, but it might be useful to try some other matrices for comparison.
- The project shows good modeling results on fire impingement and over-pressure scenarios. However, at this stage of the project, which is about 75% complete, at least preliminary experimental results that test the models are expected. There is excellent work being done on development of new mitigation strategies, such as heat shielding and incorporation in a matrix, for various accident scenarios.
- Matrix material work is especially interesting and innovative, however some important issues about the addition of composite matrix materials should be addressed in greater detail. Specifically, a close examination of structural changes that occur in the matrix when it is heated above the glass transition temperature would be useful to understand matrix-hydride interactions. Specific questions still to be answered include do temperature excursions through the glass transition temperature affect the sorption properties of the hydrides and what is the dependence of the structural morphology of the matrix on the concentration of a cross-linked polymer (i.e., is there an optimum concentration for which the matrix structural integrity is maintained while limiting the volumetric and gravimetric penalties imposed by the addition of the polymer).
- Using mitigation strategies, such as heat shield and polymer matrix, are likely to be applicable to future materials.
- The results given on the metal-hydride and polymer composite materials look promising.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Working in the Engineering CoE, the project team will continue to consult with OEMs to understand the true operating environment of these materials.
- Since this group is a part of Sandia National Laboratories (SNL), they have ample opportunity for collaborations and have a number of partners and collaborators, as expected.
- More visible collaboration with the other related projects would be helpful since it appears there is an overlap with SRNL project's mitigation approach.
- Having close collaborations with experts in partner institutions directly complement and support the SNL's modeling and experimental work. It is recommend to foster closer collaboration with the Engineering CoE.
- The project exhibits a good variety of partners, but there was little information given on the actual contributions of these partners.
- It is not clear how extensive is the interaction with the SRNL safety project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- It is not clear whether a 100-g sample size will be sufficient to truly understand how these materials will behave in a thermal or kinetic event.
- The plans to focus on mitigation and the development and characterization of new materials seem appropriate. This project builds on past accomplishments and is a logical next step. The investigators should do residual gas analyses on the mitigated samples to test for decomposition products.
- Focusing on using the mitigators represents a weakness as they are, again, a strong function of the material properties.

- The future work focuses on optimizing mitigation methodologies and strategies. Given that the project is nearly complete, this is a prudent decision. It is recommended that a more indepth assessment be conducted on the structural and chemical changes that may occur in the polymer-hydride matrix upon temperature cycling of the system. Also, additional efforts directed toward experimental validation of the modeling work is needed.
- The mitigation focus is good. Mitigation strategies should aim for general methods that may be applicable to real systems rather than specific to current model materials.
- This project ends in September 2010. Its future work will emphasize optimizing metal hydride and polymer composites.

Strengths and weaknesses

Strengths
- There are a variety of collaborations and partners that should add strength to this project. The concept of mitigation is a sound one that should yield useful results once an appropriate matrix is identified.
- This is a very good team with strong capabilities.
- The project team is extremely capable and well qualified team. There are innovative mitigation strategies being developed.
- Their modeling activities are strengths.
- The project team's work in developing potentially useful risk mitigation approaches for metal hydrides is a strength.

Weaknesses
- In the mitigation tests, a polystyrene matrix was utilized without the investigators knowing what effect it might have at temperatures in excess of 100 ºC. The investigators should assess the effects of the matrix on the mitigation experiments before going any further. Results of this have been presented at several research conferences, but there is no evidence of any publications in refereed journals this past year.
- The project is starting to take on the materials modification approach in order to improve safety. This is somehow taking the project away from its engineering-based approach.
- Clarification of the project role and what differentiates it from other projects withing the Metal Hydride Center of Excellence researching the safety of handling H_2 storage materials would be helpful.
- Experimental validation of the models is lagging. A greater focus on validation is needed. Presentation of technological challenges and obstacles to deployment of heat shielding and matrix incorporation approaches for material risk mitigation is lacking. A quantitative analysis of changes in hydride and matrix material properties upon temperature cycling is needed.
- The mitigation approaches identified may not be applicable to a wide range of metal hydrides. Polymer decomposition issues should be addressed to a greater extent.

Specific recommendations and additions or deletions to the work scope

- Keep the project. However, it is recommended to refocus the third task on finding engineering solutions rather than materials-based solutions, for instance, delete using mitigators.
- A more complete description and discussion of the obstacles and hurdles faced in deployment of heat-shielding and matrix materials for accident mitigation is needed. Also, a more detailed examination of chemical and structural changes that occur in the polymer-hydride matrix during sorption reaction cycling should be performed.
- It is recommended that the PI develops stronger ties with the Engineering CoE.
- None.

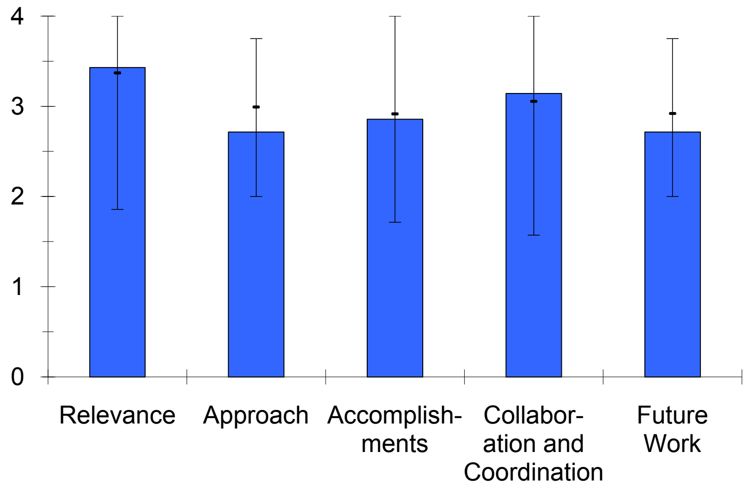
Project # ST-18: A Biomimetic Approach to Metal-Organic Frameworks with High H_2 Uptake

Hong-Cai Joe Zhou; Texas A&M University

Brief Summary of Project

The ultimate goal of this project is to prepare a metal organic framework (MOF) hydrogen sorbent material with both high surface area and high hydrogen affinity. The objectives for 2010 are to: 1) construct MOFs containing mesocavities with microwindows that may serve as a general approach toward stable MOFs with higher surface areas, 2) incorporate entatic-state metal sites into the high-surface area MOFs, 3) design and synthesize porous organic frameworks (POF) for hydrogen storage with high surface area, tunable pore size, and flexibility, and 4) determine H_2 adsorption of POFs doped by metal, such as lithium (Li) and nickel (Ni).

Overall Project Score: 3.0 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The objective of this project is to design, synthesize, and characterize MOFs with active metal centers aligned in porous channels and accessible by hydrogen molecules.
- This project is innovative and aligned to key goals with high risk, which is appropriate.
- This project is targeted at developing a MOF material with high hydrogen capacity at room temperature.
- The project's objectives are well-aligned with the Hydrogen Storage Program and the Hydrogen Sorption CoE (HSCoE).
- Exploring new MOF and POF adsorbents with ambient operating temperatures and high storage capacities is an important research activity in reaching the ultimate performance target.
- In general, the project goals are focused on the hydrogen storage DOE program objectives in relation to rapid discovery and characterization of novel sorbents. The intended focus of this project appears to be appropriately focused on augmenting gravimetric capacity and binding energy, although not enough emphasis or consideration was given to volumetric capacity. Perhaps that is the most challenging target for this class of materials.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The PI needs to consider sorption enthalpy and the practical aspects related to packing and engineering system design. High surface area is only one part of the solution, and all the tradeoffs must be considered.
- The PI is unnecessarily insisting on high surface area materials knowing that this could be done only at the cost of volumetric storage density. He should put some effort on the other half of the project, which is to increase binding energy.
- This project exhibits good guiding principles.
- This project team may benefit from thinking about how the system would work if they accomplished the desired geometry and storage.
- The stabilities of different MOFs being pursued was not discussed.
- The systematic approach of synthesis and structural characterization of the new materials is sound.

- However, the PI should deemphasize aiming for high surface area as a main goal of the project. It should only be thought of as one approach to increase capacity, keeping in mind the likely tradeoffs in material stability and cost. Besides, it is doubtful that obtaining high surface area alone is the right path toward the ultimate storage capacity. Instead, the goal for higher heats of adsorption should be given more effort.
- The approach of this project was described as biomimetic, although it is not clear how these MOFs are biological in nature. The approach and materials seem to be identical to that taken by other MOF-projects and/or researchers (i.e. mixing traditional metal salts and linkers). Most of the prepared materials involve paddlewheel clusters which are already common in the MOF literature, and so the biomimetic spin seems a bit strong. Perhaps the biomimetic spirit of this project should be deemphasized if it is not being truly adopted.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- It would have been useful to look at the delta-H [enthalpy heat of adsorption] for various materials as well.
- Moreover, the main purpose of the project is to show the benefits on hydrogen adsorption of having open metal centers in the adsorbent. Unfortunately, this is not clear after three years of effort.
- The project is making very good progress. Reproduction [of measurements] outside was important.
- The formation of MOFs with the highest surface area is very good; however, it will not allow for acheiving the DOE targets given its low volumetric capacity and low heats of adsorption.
- Though the main idea is to enhance the heats of adsorption using open metal centers, the project seems to be diverging from its main concept.
- The best result thus far is the 7.2 wt.% (percent by weight) hydrogen in PCN-68 (porous coordination network [Zhou group designation]) at 77K and 40 atm.
- The number of new ligands and corresponding adsorbent structures discovered is a significant achievement.
- Important progress was also made toward understanding of these new types of MOFs and physical properties.
- The researchers clearly have a strong aptitude for rapidly preparing and characterizing new MOF materials and corresponding linkers. The characterization also seems appropriately focused on determining hydrogen capacity and isosteric heats. More emphasis and data on volumetric capacity should be included in the future. The PI mentioned obtaining a device to measure density; however, one can easily determine loose powder density in the laboratory for preliminary reporting. This volumetric issue is particularly important for this project since they appear to be interested in preparing MOFs on very expanded linkers, spanning 4+ phenyl rings. It is anticipated that MOFs based on these linkers will not have good volumetric capacity. It should be recognized that the increased surface area and gravimetric capacity is coming at the cost of volumetric capacity.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- Many collaborators are listed but coordination is not clear.
- There are excellent collaborators in quantity and quality, and the collaborations are meaningful additions to the results.
- Others are researchng MOFs, and its recommended to make the interaction among these groups more visble, if it does exist, since they are working on related topics.
- This project has an excellent set of collaborations.
- The collaboration with General Motors (GM) that led to the validation of PI's adsorption measurement is good. However, it was not clear what came out of the rest of the partners. Perhaps, the PI should consider limiting the number of collaborators at this stage of the project. Sometimes, having too many partners has its drawbacks.
- This project does involve a large amount of collaborators both within and outside of the HSCoE. It is also nice to see that the PI has initiated external validation of their results.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The PI expressed a sequential problem-solving approach that dominated the strategy for the future work. Again, the problem requires parallel solutions for various issues. Most importantly is the ability to enhance the sorption energy, increasing the isotherm temperatures. Finally, the plan for doping the MOFs with coordinatively unsaturated metal centers is suitable.
- The future work target should be inline with the new DOE 2015 hydrogen storage targets.
- The 2010 third quarter go/no-go decision points are either well-defined or they appear to have already been met.
- The PI should prioritize incorporating active metals in the new or modified structures over increasing surface area.
- As stated, the next steps should focus on doping of unsaturated metal centers. However, it is not recommended that work continue to focus on the preparation of new MOFs with high surface area, because it is unclear how this will help achieve volumetric targets and/or increase binding. Surface area and gravimetric capacity are not the core challenges with this class of materials. Instead, efforts should focus on increasing volumetric capacity of their existing MOFs.

Strengths and weaknesses

Strengths
- This project shows a good initial proposal to explore the effect of open metal centers.
- The weight percent result is good.
- The project has a good approach.
- It seems to be scalable and recyclable.
- The synthetic capabilities are very good.
- The PI shows innovative approaches to synthesizing new MOFs with high-adsorptive surface areas.
- This project addresses critical factors of the hydrogen storage challenge.
- The project presented strong material synthesis and characterization capability.
- The team is very competent at preparing new linkers and MOFs and the rapid characterization of those materials.

Weaknesses
- The project is slow to reach its goals.
- Similar to other MOFs, the formation of compounds with high surface area was not an issue, and it seems that this is the target. Rather than creating MOFs with high surface area, it is better to focus on the available ones and try to apply the concept of open metal centers to determine how the heats of adsorption could be improved.
- None.
- The project may run into the familiar danger of past mistakes made by other adsorbent related projects. These are, specifically, being content with the temporary achievement of high-adsorption capacities in few materials at 77K, and losing sight of the ultimate target of ambient temperature operating conditions.
- There is too much emphasis on increasing surface area.
- The team seems to be too focused on preparing new materials with larger and larger linkers, yet it is not clear how this will help to get to volumetric targets.

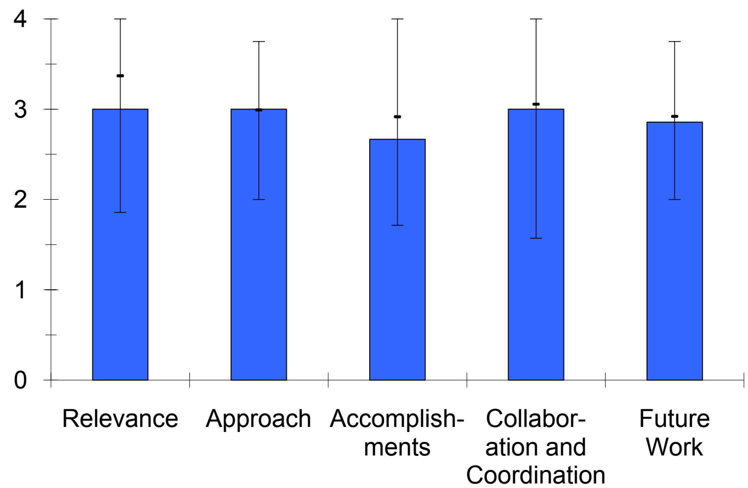
Specific recommendations and additions or deletions to the work scope

- The literature clearly shows that an extremely high surface area led to poor volumetric performance. It is suggested that the PI focus more on developing materials with the open metal sites, even with smaller surface areas, so they can at least measure the effect on hydrogen adsorption.
- The project team should look at the impact of compaction and of impurities.
- Guide materials selection on usable hydrogen (i.e. 2 bar is an empty tank), not total hydrogen.
- To keep the project, it is recommended that the project team devise a clear plan to achieve the targets that includes discovering a MOF with both high-surface area and high hydrogen affinity.
- Since hemoglobin contains iron, perhaps it might be used in the MOFs to increase the heat of adsorption. Other metals such as titanium might also be interesting.
- Aiming for high surface area should not be considered the project goal, but a means to reach a bigger goal.

- The data on additional physical and chemical properties – thermal and structural stability and sensitivity to air and moisture – would be helpful.
- The project team should also consider providing preliminary synthesis cost and scalability information.

Project # ST-19: Multiply Surface-Functionalized Nanoporous Carbon for Vehicular Hydrogen Storage
Peter Pfeifer; University of Missouri-Columbia

Overall Project Score: 2.9 (7 Reviews Received)



<u>**Brief Summary of Project**</u>

The overall objectives of this project are to: 1) fabricate high surface area, multiply surface-functionalized nanoporous carbon, from corncob and other precursors, for reversible H_2 storage; 2) characterize materials & demonstrate storage performance; and 3) optimize pore architecture and composition.

<u>**Question 1: Relevance to overall DOE objectives**</u>

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project at the University of Missouri-Columbia is another effort to develop high surface area and boron- doped carbon materials, mainly from natural resources, such as corncobs, for improved hydrogen storage via adsorption. The objective is to increase capacity and hydrogen-binding energies to facilitate great gravimetric and volumetric capacities at temperatures above approximately 100K. It is similar in scope and goals as most of the projects in the Hydrogen Sorption Center of Excellence (HSCoE) and other adsorption projects with the DOE/EERE Hydrogen Storage Program.
- The project is to synthesize high surface area carbon with surface functionality for superior H_2 storage capacity.
- The project is to manufacture monoliths for conformable, lightweight tanks.
- The design and test tank project is relevant to DOE storage goals. Increasing the hydrogen capacity beyond the the empirical relation, "Chahine estimate" of ~1 wt.% (percent by weight) capacity per 500 m^2/g surface area at 77K, if possible, would be an important contribution to reaching storage capacity goals.
- The project is aligned with major goals, though this could have been more clear in the presentation.
- The base material is free, which is excellent for project's cost.
- The project's objectives are well-aligned with the objectives of the HSCoE .

<u>**Question 2: Approach to performing the research and development**</u>

This project was rated **3.0** on its approach.

- The project is primarily an experimental study of production of carbons with and without supplemental addition of boron (B) additions. Some theoretical efforts to rationalize improvements by these approaches are included along with outside collaborations for characterizations via neutrons and other specialized facilities. A unique aspect of this effort is the use of neutron bombardment of boron-10 (^{10}B) nuclei to promote defect formation for the possible enhancement of hydrogen storage capacities. A reasonably systematic plan for variations in processing and boron-doping strategies is being followed to improve hydrogen storage properties.
- The overall approach is good. It tends to enhance the performance of activated carbon in a practical way.
- Approach is sensible in testing samples prepared with new techniques before moving on to more practical aspects such as forming compacts [monoliths]. External validation of hydrogen capacities of initial materials has proven to be an important aspect in the R&D approach.
- The material study is good so far, but multiple cycles and cycles against pressure are needed.
- It is not clear how much material can actually be processed in a reactor [facility for boron activation by neutrons], so this may result in inherently low volume production.
- This is a solid and reasonable approach of optimizing adsorbent surface area, structure, compost ion, fabrication, and characterization methods to achieve the desired hydrogen storage capacity and enthalpy goals.

- It is far from clear that materials processing via exposure to nuclear radiation is a feasible method for large-scale synthesis.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Missouri has been able to demonstrate excess adsorption gravimetric capacities in the range of 4-7 wt.% (percent by weight) at about 77K, which is inline with similar results reported for other high surface area carbons. They have not yet achieved carbons with surface areas greater than 4,000 m^2/g as initially proposed. Missouri also had some success with their boron-doping procedures to give slightly higher binding energies (> 7 kJ/mole) at the initial stages of adsorption, which is also similar to the work of others. However, a pathway to significant adsorption at room temperature is not currently evident with these materials. The ^{10}B-neutron treatment apparently increases the capacity, but the mechanism is not currently identified.
- The progress is modest, and the enhancement of available materials is not clear. Moreover, there seems to be some contradictions in the results. For example, excess measurements on the "3K" sample show a similar behavior to "MSC-30", which is almost the same capacity with a maximum at around 40 bar, but a few slides later the same sample shows a higher capacity with no maximum and a higher binding energy.
- Also the rate of progress seems to be a little slow, only 30% completed since September 2008, and the remaining tasks will require much effort.
- Technical progress toward the project's goals has been good, especially with the cooperative validation of measurement and analysis of hydrogen storage capacities. More progress will have to be made to meet DOE targets. Neutron irradiation is providing interesting results but requires further analysis to understand the impact on hydrogen sorption.
- It was hard to appraise the actual progress.
- It is not too surprising that the neutron capture did not make the surface area grow, so it will be essential to get to etching experiments .
- They report a good adsorption level on a cheap material, but it needs to be confirmed outside [University of Missouri].
- The project found an increase in heat of adsorption from the addition of boron with a couple kJ/mole better binding.
- In light of the fact that the project is relatively new [DOE: the project was started late in FY 08] , the PIs have accomplished quite a lot on material synthesis, characterization, and modeling fronts.
- The project team has obtained a good agreement on the experimental and theoretical binding energy results on the B-doping.
- It is not clear that all of the data are reproducible.
- It would be very helpful if some of the higher performing samples could be sent to Southwest Research Institute (SwRI®) for independent measurements.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Missouri has collaborated with National Renewable Energy Laboratory (NREL), National Institute of Standards and Technology (NIST), and other facilities to extend the characterization of their materials and independently validate the hydrogen adsorption capacities. They have also worked with outside theory groups. Closer interactions with the theoretical partners from the HSCoE could be useful.
- The collaboration is mainly on characterization and modeling. There is little to move the project ahead.
- Their collaborative validation with NREL is a positive step.
- Their collaborations are suitable and productive.
- The project team appears to have a strong collaborative partnership with NREL, Argonne National Laboratory (ANL) and NIST. However, it was not clear the specific roles of the international partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- It is not clear how future work is planned on room temperature materials.
- The work plan for the remainder of fiscal year 2010 and 2011 is reasonable. More characterization work to understand the role of neutron bombardment on apparent hydrogen-capacities should be included.
- Their work needs to proceed to the next phases of the project
- The future work is appropriate. The comparison of experimental and theoretical heats of adsorption is an important task.
- Etching is important.
- These are very good plans.
- The PIs should be encouraged to strive for reproducibility of their storage measurements.

Strengths and weaknesses

Strengths
- The systematic production and boron-doping of potentially inexpensive carbon adsorbents based on experimental work and theoretical support has been shown. Measurements and characterizations appear to be properly conducted.
- This is a good project with a practical approach for advancing the field.
- The project team appears to be able to functionalize materials through boron insertion.
- The use of low-cost material is a strength.
- While still conducting fundamental and mechanistic studies on the carbon materials, the project team appears to be guided by practical, real-world considerations. For instance, the project reports side-by-side adsorption results at 80K and 303K, which is very encouraging. A second example is the fact the PIs plan on monolith sorbent manufacturing and vessel test design. This is very positive and refreshing.
- There is potential for high uptake in relatively expensive materials.

Weaknesses
- The project has so far been unable to substantially increase either surface areas or boron-doping contents in these materials when compared to carbons from other sources. Improved methods to increase hydrogen-binding energies for larger hydrogen contents are needed if these materials are to have any application above cryogenic temperatures. There seem to be little cycling of the adsorption properties. This is especially important for the neutron-treated materials. This latter point needs to establish whether defects are created that can only bond hydrogen once and not provide enhanced capacities with cycling.
- The project is way behind schedule and needs more focus.
- It is not clear how much higher surface area materials will be achieved.
- There is a potentially high cost for processing.
- There is a high chance of hydrocarbon formation in nuclear-treated material.
- No outside confirmation was shown.
- Perhaps it is the presentation, but this work did not inspire confidence that the data were dependable.
- One of the three project objectives, namely optimization of pore architecture barely got a mention.
- It not clear that the favorable capacities of some samples have been correlated with specific material properties.

Specific recommendations and additions or deletions to the work scope

- The potential use of accelerators to enhanced etching may not be a practical step. Can the enhanced surface area process be achieved by other means?
- It is suggested that a greater effort be put into characterizing the presumed defects formed during neutron bombardment and the formation of new hydrogen-bonding centers.
- Desorption measurements of the doped materials is a must to check their reversibility. Results of the samples “3K” and “4K” need to be confirmed.

- It is recommended that further validation of experimental results be shown through collaborations, particularly on the measurements of isosteric heats of adsorption.
- The PIs need to have a credible third party to confirm the 7.2% result over a few cycles.
- The project team should look into the effect of lithium (Li) created from boron in nuclear-treated material, which may overshadow the boron effects or radiation tracks.
- This project should start addressing material pore structures and their effect on performance.

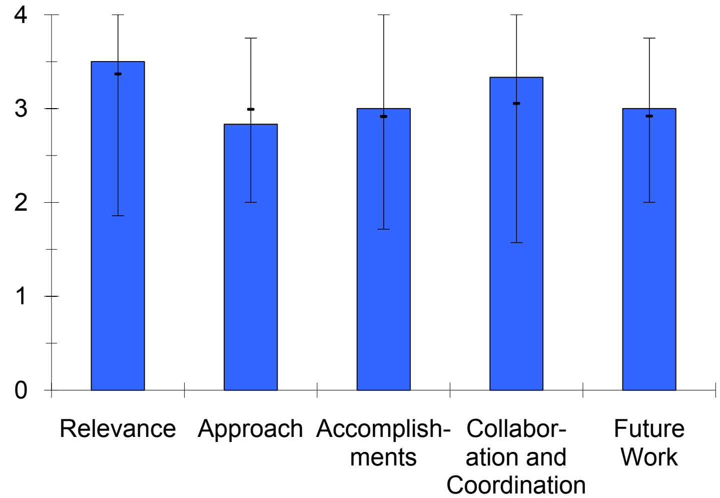
Project # ST-21: NREL Research as Part of the Hydrogen Sorption Center of Excellence

Lin Simpson, National Renewable Energy Laboratory

Brief Summary of Project

The National Renewable Energy Laboratory's (NREL) research in the Hydrogen Sorption Center of Excellence (HSCoE) is focused on key technical barriers in DOE's Hydrogen Program for on-board storage. NREL materials development focused on capacity, cost and kinetics targets, including 1) volumetric and gravimetric capacities; 2) optimize accessible specific surface area and pore size (e.g. decrease tank weight and size); 3) tune binding energies to increase capacities and control operating temperature; 4) improve kinetics for weak chemisorption, rate of hydrogen adsorption; and 5) develop next-gen sorption materials using inexpensive materials and processes.

Overall Project Score: 3.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project is focused on key goals, and the ones that truly drive the system.
- Their attention to costs up front is excellent.
- The project is well aligned with H_2 Program objectives and occupies a leadership role in the overall Hydrogen Sorption Center of Excellence (HSCoE) effort.
- There's no doubting that the HSCoE is a very relevant project.
- NREL's research is highly relevant to the overall DOE materials R&D effort. In particular, they are focused on addressing the key technical barriers associated with sorbent materials – volumetric capacity, optimization of surface area and pore volume, and enhanced binding. Additionally, to overcome these challenges, they are pursuing a wide range of novel materials including boron-doped carbon, spillover in carbons, and new zeolite-templated carbons.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- This center has had a remarkable turn around in its approach and outcome in the past five years. This is the best it has done. The approach is exactly what is needed to methodically develop storage materials.
- The materials being made are a good approach at this point in the program.
- The predictions are a good approach to guiding the more expensive and slower experiments.
- They [NREL] do serve a valuable role as coordinators as well.
- The Center relied too much on theoretical estimation to guide their materials research and was unable to validate these models based on the experimental results.
- For this late stage in the project, the technical approach is too broad and lacks specificity. The technical targets are firm, but the work should focus on a select group of materials instead of introducing new materials.
- Increased emphasis on optimization of hydrogen-binding energy is a good approach.
- The project milestones are weak and appear to be mixed up with deliverables. Publication or final reports are not milestones.

- NREL's approach to sorbent materials discovery is appropriately scoped and covers the primary challenges associated with that class of materials, which are volumetric capacity, binding energy, and spillover validation. NREL is also tasked with a great deal of the project management aspects of the overall Center at which they appear to be very competent, particularly in supplying measurement capabilities for CoE partners.
- It is clear NREL has been developing expertise in validation spillover in carbons. These efforts are applauded given the current lack of understanding and variation in results in the literature.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Specific-area based BC_3 adsorption is good. It is unclear if a higher area is possible but a good option. $LiBC_x$ was especially encouraging, and the PI should show a higher volume proof.
- A spillover capacity of 1% seems reasonable, although it is obviously not quite what is wanted but a believable result. Also, the much faster kinetics are perhaps due to shorter distance from the metal.
- For the BC_x systems, the results of higher uptakes for very low surface area materials needs to be explained. Otherwise, it is rather questionable.
- The analytical measurement capabilities and the "Best Practices" [measurement] document is valuable for future researchers in this area.
- The experimental results on the templated BC_x appear to be interesting.
- The PIs need to work on chemisorptive materials that are important to understanding the spillover mechanisms.
- The project team shows good work on improving the kinetics of spillover.
- A great deal of progress is evident for this project:
  - The characterization (X-ray photoelectron spectroscopy) and understanding of boron-doped carbon is at a high level and shows a great deal of promise. Further searches for materials that couple sp2 sites with high surface area are desirable.
  - The PI's understanding of the synthetic challenges associated with spillover materials is showing progress. It was appreciated that both successful as well as unsuccessful results were shared, and it will be interesting going forward to understand why there are such mixed results for platinum (Pt) on carbons (e.g. validating the role of oxygen functionality).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- This project has been very helpful to other groups along with deriving value from the collaborations.
- There seems to be a low level of collaboration. For instance, there were issues observed for spillover samples reproducibility when prepared by different groups within the Center.
- The project has extensive collaboration with its partners.
- It is very difficult to separate NREL's specific technical contribution outside the leadership role of the HSCoE.
- This project relies heavily on collaborations and is a true resource for other projects both within and outside of the Center with respect to measurement capabilities. NREL has continued to do an outstanding job of extending its knowledge and facilities to other groups. Additionally, as exemplified in their presentations, NREL has developed a great deal of individual interactions with other groups to pursue focused research.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- They are ready to wrap up this project. It is not clear that there is any more they could do.
- It is unsure what was meant by the fiscal year 2010 material development and Center go/no-go activities. There was no specific line for decision makers.
- The future work appears to be organized and focused on logical next steps considering the remaining time left in the project. Given that NREL has a good overall perspective of spillover materials based on interactions

within the Center and its one laboratory experience, it will be very useful to get their feedback in the final report with respect to the future potential for these materials.

Strengths and weaknesses

Strengths

- This project shows strong collaboration and has established access to other researchers and laboratories.
- The flexibility to fail fast and move on to new materials is a strength.
- The Center is finally starting to hit its stride. Unfortunately, this is occurring just as its funding is coming to an end.
- This is a very competent, highly collaborative research team.

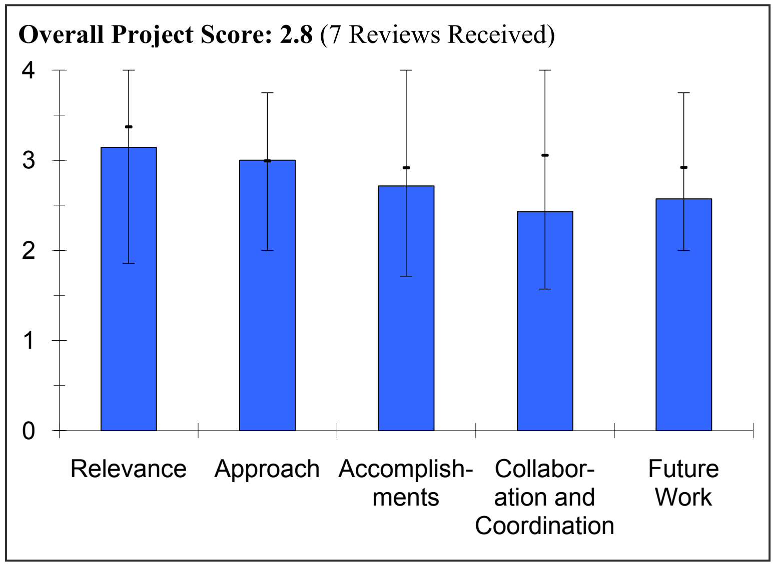
Weaknesses

- The possibility to validate the theoretical estimates led the systems studied.
- There are almost no existing milestones or go/no-go decision points.
- Although an important future plan from last year's slide reads: "Provide materials/systems recommendations for DOE & Hydrogen Storage Engineering Center of Excellence", the project team did not come up with a recommendation of adsorbent materials inspite of specific requests. This is very troubling.
- The following comment from last year's reviewer remains valid: "The lack of a straightforward assessment of the severity and scope of the remaining technical barriers, as well as a statement concerning the extent to which the R&D in the remainder of the project will be able to effectively deal with those obstacles, are weaknesses."
- There is no follow up on the theoretical prediction of the calcium-decorated covalent-organic framework (COF) materials identified a year ago.

Specific recommendations and additions or deletions to the work scope

- It would be good to see more work on the reproducibility of spillover.
- The approach is exactly what is needed to methodically develop storage materials. This center has done an excellent turnaround in their scopes and methods during the past five years.
- This year's presentation is probably one the best given during the entire project.
- The results are still far from the targets. However, the methodology and approach are perfectly suited for further material discovery, if the focus stays on developing room temperature, high-capacity sorbents.
- Among all the centers, this is the most promising area.
- The PI should keep refocusing on understanding the most promising materials systems.
- The final report should clearly contain specific recommendations of promising, downselected materials, or a class of materials, for future R&D.
- The experimental results on the templated BC_x should be verified by Southwest Research Institute (SwRI®) or other independent laboratories.
- Sorbents are an extremely important class of hydrogen storage materials.
- It is recommended that the DOE find a way to continue funding research in this area.

Project # ST-22: A Joint Theory and Experimental Project in the Synthesis and Testing of Porous COFs for On-Board Vehicular Hydrogen Storage
Omar Yaghi; University of California, Los Angeles



Brief Summary of Project

The overall objective of this project is to achieve room temperature H_2 storage in covalent organic frameworks (COF) to meet DOE 2015 targets. The objectives for fiscal year 2009 are to: 1) conduct synergistic work between OmarYaghi (University of California, Los Angeles (UCLA)) and Goddard (California Institute of Technology (Caltech)), 2) build high-throughput preparation setups for COF synthesis using high temperature and pressure; 3) develop chemistry to realize stable frameworks, 4) introduce potential metal-binding sites through the COF synthesis, 5) determine atomistic connectivity of COFs using an ab initio charge-flipping method using powder X-ray diffraction data, and 6) predict adsorption enthalpy of H_2 on various metal sites.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project is designed to support the Hydrogen Storage Program and its goals and objectives.
- Overall, the work plan seeks to help the DOE meet the storage objectives by developing a methodology to synthesize new adsorbents.
- The synthesis and design of new materials is necessary for improving physisorbed hydrogen characteristics and allowing flexibility to develop higher-enthalpy-binding sites to allow higher-than-cryogenic temperature storage. The calculations are used to predict new materials and hydrogen-binding properties, but still there are barriers to synthesizing new materials owing to the complexity of the solvent-reactant energetics. The proposed combinatorial approach might yield fruit here, along with the condensation reactions to produce stable COFs.
- This project is very relevant. Metal-Organic Frameworks (MOFs) working at higher-than-cryogenic temperatures are definitely a huge plus.
- This program examines the development of boron- and nitrogen-doped COFs with the ultimate objective of adding metal sites for improved hydrogen binding. This objective is well within the scope of DOE objectives for hydrogen sorption materials.
- The project is in year 1 , is 20% complete, and has yielded one Journal of the American Chemical Society (JACS) publication, one Materials Research Society (MRS) Bulletin publication, and one Chemical Society Reviews publication.
- This project is critical to the Hydrogen Storage Program and fully supports DOE RD&D objectives.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach is quite similar to that used in the past. It works from a chemistry standpoint but may need an adjustment to reflect the rather modest success with hydrogen storage in MOFs.
- The PIs did not clarify what special advantage is obtained with COFs as compared to what has been studied in previous work.

- The approach of increasing the delta-H and thereby achieve a higher isotherm temperature is not addressed adequately.
- There is still some work to be done to better integrate theoretical and experimental efforts; but to be fair, the project is still in an early stage (20% completed).
- The approach is close to being useful if a closer tie can be made between calculation and experimental validation and/or synthesis. The materials appear to be promising, but this can be said for the calculated properties of so many materials where the synthesis or realization of materials is unlikely. The incorporation of metal ions at sufficient densities and removal of ligands has proven to be an issue for many in this field. It detracts from the storage capacities in numerous ways from surface-area reduction and pore blocking.
- The approach based on modeling and high-throughput preparation is good.
- This researchers are taking a systematic approach to COF synthesis and X-ray characterization, while simultaneously seeking computational input for ideal COF structures and their theoretical hydrogenation properties.
- The systematic synthesis approach includes varying ligand types using a common imine chemistry.
- One impressive development is the use of a relatively new technique in powder X-ray diffraction analysis called charge flipping. Using this approach, the researchers were able to validate the synthesis of COFs comprised of five interpenetrating networks, which is a very complex structure.
- The approach needs to focus more on increasing strong binding sites for maximum hydrogen uptake capacity without losing pore volume.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- It's uncertain why COFs should offer any advantages over MOFs. This is an interesting, basic chemistry project with a very limited potential for practical hydrogen storage.
- Considering the resource allocation to this project, there was a higher expectation for technical progress.
- The project seems to be on track.
- Several areas have progressed in the synthesis areas: 1) high-throughput capabilities, 2) application of the well-known (in the field of crystallography) charge-flipping technique to poor quality X-ray data, and 3) condensation reactions to produce COFs. However, the progress is not without a cost and there is little material presented that addresses the hydrogen adsorption characteristics besides the values obtained form Goddard's calculations, which are prolific.
- Lots of COFs are synthesized, but there is not yet enhancement in binding energy. The PI needs to move to metal incorporation.
- The project is relatively new, and the researchers are off to a very strong start. The importance of systematically developing the COF-linker chemistry, followed by the COF synthesis and X-ray characterization, and then followed by the synthesis and characterization of metal attached COFs (which are recommended by theory before and after hydrogen uptake), is realized by the researchers.
- So far, at about 20% project completion, the COF synthesis, challenges toward X-ray characterization, and recommendations of computational collaborators have been addressed.
- The technical accomplishments to build high-throughput preparation setups and to develop a structural determination technique using the ab initio charge-flipping method seem on track. It is good to see the beginning of a modeling study for optimal-binding energy.
- The synthesis of new COFs through hydrazone condensation needs to be more focused for on-board vehicular hydrogen storage at room temperature.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.4** for technology transfer and collaboration.

- The project's collaboration is limited to just one partner, BASF. Surprisingly, no DOE or academic partners are involved.

- This project is not taking advantage of the rich environment and projects that have been funded and supported within the hydrogen storage program. This is a very critical issue, and if not addressed, it could have significant detrimental effect.
- The synthesis and calculation work seems to be pretty divided,and their work with collaborators toward the same general goal shows little interaction. There is no information on how BASF has been involved so far, though the collaborations from other MOF work is well known. But, given the early stages of materials synthesis and characterization, this may be expected.
- The PI's collaboration with BASF not really clear.
- The researchers are working closely with the Goddard group at CalTech with the idea that Yaghi's group will be able to synthesize challenging to prepare COFs predicted through Goddard's work.
- The project team's collaboration with other institutions are not clear.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Their future work is hardly relevant to the end goal of designing novel, high-capacity hydrogen storage systems.
- While in the late stages of the hydrogen program, the proposed work here is reminiscent of the early part of the hydrogen project. It is not clear how the project addresses issues such as packing density and room-temperature operation.
- The project objective to integrate its theory and experiments is interesting and reflects a desire to address some criticisms of some projects that overemphasize the theoretical development and provide little experimental validation. The importance of studying some materials as proofs of concept that large storage densities could be achieved is understood. Some thoughts should be given to justifying or categorizing those materials in terms of the projected economic viability for transportation applications.
- There is not much provided, but there are logical progressions on material synthesis. It is crucial that the PI puts forth efforts to incorporate or partner with others, so that more characterizations can be performed.
- The proposed future research plan is only a restatement of initial objectives.
- Only one challenge that may arise was not addressed. That challenge is the understanding of neutron diffraction data after H_2 adsorption, given the difficulty in specifying the crystal structure determination of empty COFs.
- The project team's future planning is on track to employ metals to create strong binding sites, including a material design based on theoretical prediction. But, there needs to be more focus for on-board vehicular hydrogen storage at room temperature.

Strengths and weaknesses

Strengths
- This is a strong basic chemistry project.
- The project seeks to estimate the viability of a new class of materials for hydrogen storage.
- The project's way of integrating theory wtih experiments is interesting and reflects a desire to address some criticisms of some projects that overemphasize theoretical development and provide little experimental validation.
- The PIs exhibited good synthesis and materials development directions with some aid of computations.
- The group is excellent at synthesizing new compositions.
- The approach is very systematic and is inline with DOE objectives.
- The institution and the PI are well recognized in the area of this R&D.

Weaknesses
- The project has started to deviate its from original direction, which is designing novel materials for hydrogen storage.
- Although it is important to study some materials as proof-of-concept that large storage densities could be achieved, some thought should be given on justifying or categorizing those materials in terms of the projected economic viability for transportation applications.

- Mechanisms to dope the framework are sketched out, but the chemistry needs to be worked on. While the calculations presented as supplementary information give astoundingly high predictions, there is not much hope of them being validated experimentally. The calculations need validation from experiment and more hydrogen characterization needs to be performed to enable this.
- They need to focus on quantity instead of quality. The synergies between modeling and synthesis are not clear, and it looks like it is proceeding separately.
- The characterization tools seem limited to X-ray and neutron diffraction. Lots of information can be gleaned from fourier transform infrared (FTIR) and nuclear magnetic resonance (NMR) studies, particularly at the stage of H_2 uptake and assessment of its locale.
- Their approach is too diverse.

Specific recommendations and additions or deletions to the work scope

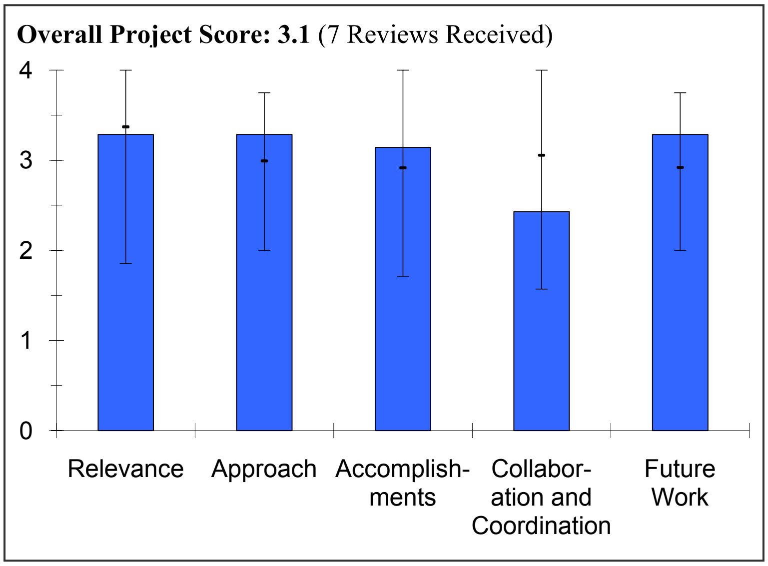
- It is recommended the DOE move the project to the Basic Energy Science.
- In view of the progress made in the past five years and the current trends in the carbon-based sorbent area, this program is out of synch. As a result, this project shows low relevance to the overall hydrogen program.
- It is recommended that the PI revisit the goals and objectives of this project. In view of shrinking resources without a significant change of scope and structure, it is difficult to rationalize the continuation of this project.
- It would be greatly beneficial if the group could synthesize at the least one composition already predicted by their own theory to have high-storage capacity at room temperature, instead of generating a lot of formula compounds. One example is metallated COF102-Li.
- The project team should consider using infrared and/or NMR as tools to assess H_2-adsorbed COFs the would determine the site locale of the H_2 relative to the COF structure.
- It is recommended that, should good candidates become available, large scale synthesis procedures be pursued.

Project # ST-23: New Carbon-Based Porous Materials with Increased Heats of Adsorption for Hydrogen Storage

Randall Snurr; Northwestern University

Brief Summary of Project

The objectives of this project are to: 1) develop new materials to meet DOE volumetric and gravimetric targets for hydrogen storage, including metal-organic frameworks (MOF), polymer-organic frameworks (POF), and the tight integration of synthesis, characterization, and modeling, and 2) increase heats of adsorption as a means to meet volumetric and gravimetric targets at ambient conditions.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The goal of improving gravimetric, volumetric and enthalpy of adsorption (delta-H) are aligned with program objectives.
- This approach is three-pronged with two synthesis efforts (MOF and POF) and theoretical prediction. The combination of MOF and calculation work is useful and is providing knowledge that can be applied towards the DOE goals, especially with higher enthalpy of adsorption sites controlled in MOFs. The POF work is likely to lead to the most scalable processing, but it currently suffers from low volumetrics, capacity, and enthalpy.
- It is not clear how this project addresses the gravimetric and volumetric storage targets. The PIs are clearly focused on increasing heat of adsorption, but the capacities are still extremely low. It seems very unlikely from the material presented that this work will lead to materials that meet the 2015 targets.
- This project is relevant to the DOE's goals of increasing the temperature of physorption materials while maintaining practical hydrogen storage capacity.
- It is necessary to discover and develop new materials to be able to meet the DOE targets and the Northwestern group is combining theory, synthesis, and characterization applied to new concepts and new materials.
- This project is highly relevant toward achieving DOE hydrogen storage programmatic goals, specifically focusing on gravimetric and volumetric capacity and heats of adsorption in sorbent materials, MOFs and POFs, through experimentation and modeling. The concept of using strong-binding linkers and/or introducing metal ions and atoms is not completely novel, but nevertheless should provide a fruitful modeling and/or experimental space.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- This project has just started. [DOE: The project started in late FY2008.] The problems the PIs will face will come from the approach of using high-charge-state linkers (that are desired for the end-product) to attract molecular hydrogen, with the nature of these charge states in order to synthesize the materials in the first place. This is probably reflected in the lower-than-theoretical surface area that the synthesis has yielded to date. It is possible that remnant liquid from the processing will be retained within the structure and be difficult to remove. Moreover, while the charge states of the linkers may serve to improve the sorption at the charge site, subsequent hydrogen will invariably have sorption enthalpies that are smaller. While a MOF with a high average of

sorption enthalpy as a function of loading is desirable, this will probably be too difficult to achieve. It is good that the PI recognizes this though.
- There is strong relationship among the PIs, and the separation of issues leads to a well-rounded approach with more focus on the MOF work than POFs. This is likely because of the ease of calculation of periodic MOFs compared to the short-range-ordered POFs. More emphasis on the latter would be useful.
- The modeling seems to work well. It is very useful to predict isotherms, heats, and diffusivities.
- The investigators have done a good job predicting materials with higher heats of adsorption and following through with the experimental work to prepare and test the materials.
- However, it is not clear how this project will ever meet DOE gravimetric and volumetric targets, even given the most optimistic results.
- More collaboration within DOE program would be beneficial. Good collaboration between synthesis and modeling and more hydrogen storage characterization is needed.
- There are potentially strong linkages between theory and experimentation.
- The combined experimental-modeling approach for materials discovery and optimization is probably the biggest strength of this project. In particular, the modeling activities have proven instructive for the up-selection of promising linker-incorporated metal ions (alkoxides) for experimentalists to focus on. Simultaneously, experimentalists have been productively synthesizing high surface area POFs and MOFs and testing their hydrogen binding and adsorption properties.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **3.1** based on accomplishments.

- The theory and empirical components of this effort appear to work well together, and they have produced a few ideas and results of potential interest.
- The synthesis and validation that delta H can be increased uncontrollably in these MOF materials is an achievement along with the novel zwitterionic systems. The increase in delta H in these systems is presumably from the charge seperation in the framework, but the narrow pore size could also contribute to the high delta H. It would be worthwhile trying to separate these factors, perhaps with calculations.
- For the POFs, the moderate surface areas give rise to a rather low 77K hydrogen adsorption capacity of 1.4 wt.% (percent by weight). The expected maximum excess adsorption would be more like 3 wt.%, yet the available data does allow this determination. Pore size distributions would be helpful to use in interpreting the delta H values since they seem rather narrow.
- The calculations seem robust compared to the literature and experiments. The highlight might be that magnesium (Mg) is a good receptor.
- The project presented nice accomplishments. It has met milestones for heat of adsorption and surface area. However, even while meeting both targets, the H_2 uptake is still very low, approximately 1%, even at 77K. Is there any hope that this approach will ever meet DOE targets?
- Modeling efforts are making progress and pointing to new Mg-alkoxide MOFs with better H_2 storage properties, but these need to be validated through the synthesis of the materials. The PIs have achieved at least two new MOFs and three new POFs. However, all have relatively low surface areas. Novel synthesis routes are promising, but the progress on synthesizing new materials could be better.
- The Northwestern group has synthesized several new MOFs and POFs and achieved the current year's target of improving the heat of adsorption. The new concept of introducing cations is very interesting and seems promising.
- The project team's progress has been somewhat modest so far, but the work is of high quality and this is still a relatively new project.
- Clear progress has been demonstrated on the synthesis of unique zwitterionic MOFs and POFs achieving their 2010 enthalpy milestones (i.e. 10 kJ/mol). These materials appear to be distinguished, in approach and composition, from the many other related MOF projects. For the modeling work, the impact of catenation on binding, and likely volumetric capacity, is very interesting. The alkoxide aromatic predictions are interesting; hopefully, this work can be experimentally demonstrated despite the lack of current successes of lithium (Li) introduction into MOF linkers. The further linking of experimental and modeling efforts is encouraged.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.4** for technology transfer and collaboration.

- Collaboration with the PIs appears good. Partners and collaborations have not been featured in the past year, partly because this is a fairly new project. The lack of materials characterization would warrant they expand their collaborations to get high-pressure isotherms, plus some characterizations from their long-term collaborators.
- There is good collaboration within the group, but no collaborations with DOE H_2 program were shown. Collaborations with other laboratories are getting started. It would be beneficial for the PIs to collaborate more closely with the Sorption Center.
- It is very important to maintain good collaborations with experimental groups to prepare and test predicted materials.
- Collaborations with experts within the program should be increased, particularly with respect to hydrogen storage measurements.
- No formal collaborations exist, but they are utilizing other groups' expertise. There does not seem to be a need to include any other researchers at this stage of the project, but they are likely to do so in the near future.
- There is nothing of significance related to H_2 storage at this point beyond Northwestern. However, the team at Northwestern is already well rounded.
- Given this is a relatively new project, it is understood that some time is required to develop external collaborations, especially since the Sorption Center is ending. It does appear they are beginning to leverage characterization resources at Argonne National Laboratory (ANL) and Universidade Federal Ceara, (Brazil). This project does rely on extensive internal collaborations involving multiple professors at Northwestern. Additional future collaborations with other researchers like Hong-Cai (Joe) Zhou at Texas A&M University might also be logical.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The plans make sense on the basis of the initial proposed work. The problem with the project from a programmatic standpoint is the goal of a high heat of adsorption.
- Capitalizing on the recent results, the direction forward seems well judged, specifically placing more emphasis on the full adsorption range to compare deliverable hydrogen capacities to the 2 bar low pressure limit for fuel cells.
- The project team's future work looks good.
- The proposed future research is inline with the recommendations for speeding up synthesis, characterization, and validation.
- Their theory predicted the introduction of Mg to be beneficial for increasing H_2-content (wt%), and it will be interesting to learn from the next year's experiments if the synthesized compounds meet the expectations.
- The future work seems to be a logical extension of current progress. In particular, it is recommended and listed that the cation incorporation into the MOFs remain a primary focus. This work supports the theoretical predictions and should help to augment hydrogen binding strength. The expansion of modeling efforts to include POFs should also remain a priority to strengthen linkages to experimentalists.

Strengths and weaknesses

Strengths
- There is good collaboration and recognition on the part of the PIs in regards to the goals they can achieve that are within reason.
- The combination of the three PIs and their expertise areas is a strength.
- The project team presented very nice modeling and experimental work. The project is well organized, and investigators work well together.
- The systematic studies are showing steady progress.
- There is a wide range of synthesis capabilities. POFs offer new and potentially tunable material for H_2 storage.

- The Northwestern group is introducing a new concept for materials discovery and for improving existing materials. The approach is solid.
- This is a highly organized, focused, and efficient team. They are showing great progress!

Weaknesses

- A single value for a high heat of adsorption will not address the technological issues. If an initially high value for the isosteric heat decays as a function of loading, then the isotherm behavior will be such that a substantial fraction of hydrogen is retained within the material at the greater than 3 bar pressures that is required for fuel cell delivery.
- They lack the inclusion of practical systems considerations.
- A primary weakness is that there is no clear indication of how they plan to significantly increase the capacity to meet the targets. They need to clearly show what materials properties (i.e., surface area or heat of adsorption) are necessary to achieve the targets.
- This is a very nice basic science project, but it is not well suited for an applied program.

Specific recommendations and additions or deletions to the work scope

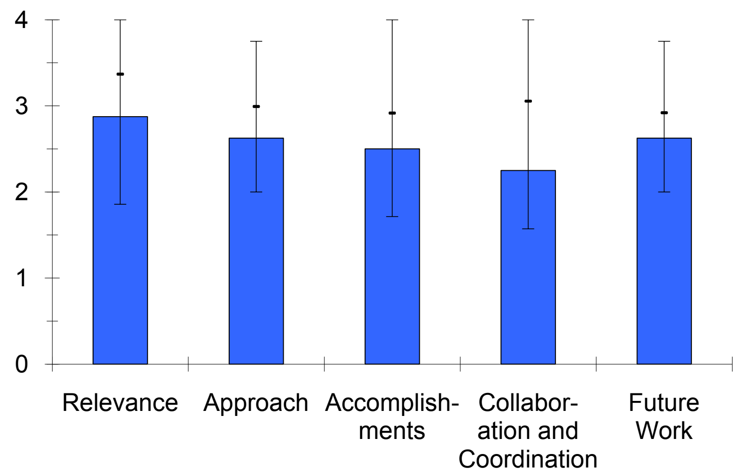
- The project is still relatively new, but they should try to create more overlap in scope and search-space between modeling and experimental efforts.
- The project team needs to have fuller characterization.
- More direct partnering with other DOE projects to validate measurements is needed.
- High-pressure measurements are needed to demonstrate maximum excess capacity of these materials.
- Experiments of full isotherms to high pressure should be performed on current materials.
- In a later phase of the project, a task for validating the experimental results should be added on.

Project # ST-24: Hydrogen Trapping through Designer Hydrogen Spillover Molecules with Reversible Temperature and Pressure-Induced Switching

Angela Lueking, Penn StateUniversity

Brief Summary of Project

Overall Project Score: 2.6 (8 Reviews Received)



The overarching objective is to synthesize designer microporous (d < 2nm) metal-organic frameworks (MOF) with catalysts to enable hydrogen-spillover for H_2 storage at 300K-400K and under moderate pressures. The objectives for the past year have been to: 1) synthesize eight microporous metal-organic framework (MMOF) structures with variations in surface chemistry, pore diameter, pore structure, and surface area measurements, 2) adapt volumetric measurements to enable rapid screening tests (RST) at high pressure towards a go/no-go decision point, 3) validate RSTs against published activated carbon and spillover materials at P<80 bar and 298K, 4) conduct an initial screening via RST secondary spillover tests that shows 2.4 wt% (excess) achieved at 300K and 80 bar relative to a 1.5 wt% benchmark, 5) demonstrate the importance of preparation conditions on uptake with a three-fold enhancement in published literature with proper activation, and 6) explore methods for pressure-induced hydrogen gas trapping.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- Spillover is a controversial methodology that has mixed results from different laboratories, and here $2 million is being invested to shed light on this subject.
- It seems that the project primarily should focus on spillover and improve the lack of knowledge in this area while validating the high value results in the literature.
- Most of the project's aspects align well, especially the 5.5% go/no-go point projected in fiscal year 2011.
- The investigators are adressing key barriers to H_2 storage. The focus on room temperature adsorption is good and appropriate for this project.
- It is unclear how the H_2 trapping work will really be beneficial. Even if a perfect material is found, it is difficult to see how the logistics of charging at high pressure and storing at low pressure will work. Either the onboard tank is built for high pressure where there is no need to store at low pressure, or it performs offboard charging, which is much less attractive.
- The development of new materials is relevant to the program.
- It is not clearly stated, but it is presumed that this project has a focus to improve mass and volume percent.
- It would be desirable to obtain a high-capacity material that operates at room temperature, and the Penn State group is utilizing MMOFs to reach this goal, which is aligned with the needs of the Hydrogen Storage Program.
- A study of the importance of the hydrogen receptor in spillover is of great importance.
- The objectives of this project, concerning the synthesis of spillover MOF materials, aligns well with DOE R&D goals. Given that perhaps the most critical challenge associated with MOFs is their relatively weak binding of hydrogen that leads to cryogenic operation, this project's objectives are very relevant.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- The approach of independent synthesis and validation of spillover results is valuable, and this project is well designed to do so. The experimental and preparation methodologies are in place to make significant progress in the near term. The synthesis of new MOF is of moderate value here but is a little premature given the uncertainties in the spillover process itself. The other aspects of the project are academically interesting but have little chance of achieving DOE goals given the small amounts 'trapped' at cryogenic temperatures. Their approach for MOF work is good but could be more systematic. It would be useful to establish correlations between various functional groups and H_2 uptake and not just focus on capacity.
- What is the role of spillover? How are these materials different than other MMOFs with functional groups? Is the metal just acting as a disassociation catalyst?
- On slide 12, investigators state that "extrapolation suggests greater than 4% at 100 bar". Is it fair to talk about extrapolation here? Don't all of these systems saturate at some surface coverage?
- The main goal of the project is to produce catalyzed MOFs for hydrogen uptake by spillover. Beyond this, the R&D approach to meeting DOE storage goals are not particularly clear.
- The PIs include engineering effects that are good.
- Trying to gain understanding of what factors matter is good, but it is not clear that the specific approach will allow predictive power but only show some generalizations.
- Looking to maximize dispersion; attempt to lower pressure is needed.
- Pressure quenching is of questionable value. The cartridges are not a very likely route.
- To improve the hydrogen adsorption by a spillover mechanism, the Penn State group is designing materials by introducing functional groups. This is a promising approach. However, the second approach for the materials design seems less promising. The H-trapping via hysteretic sorption provides a way to explore the rigidity and flexibility of structural frameworks for trapping hydrogen, but it is questionable how this can improve the gravimetrics.
- While it is good to have a wide-ranging study, casting too wide of a net can lead to confusion rather than clarity. It is recommended that the PIs consider conducting a design of experiments to isolate the importance of, or establish trends across, a few key materials properties by looking at only a subset of possible receptors.
- A nice example of this type of approach was presented in their work already, according to the slides on the importance of preparation conditions.
- The overall approach of pursuing spillover MOFs that surpass current state-of-the-art isoreticular metal organic framework (e.g, IRMOF8) is appropriate for down- and up-selection of materials. It is unclear what the specific rationale is for MOF selection that could be more developed. However, there was good detail pertaining to the approach for maximizing metal dispersion. The screening approach involving a single point isotherm at approximately 80 bar is very useful and efficient, especially given the often slow kinetics for this material class. It is very ambitious to pursue 5.5 wt.% within the second year of the project, which could be slightly softened.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- Several systems have been measured, and literature results seem to be validated. From the synthesis of already-known MOF systems, their preparation for spillover studies has produced some respectable achievements, and the academic aspects of the thermodynamic trapping studies show promise.
- Overall, the results are inline with expectations of material properties and correlate with the best there is in terms of spillover.
- While the quick, one-step throughput has led to efficient down select criteria, the lack of desorption data or evaluation of possible side reactions is a major deficiency. It is apparent in the 2.5 wt.% sample reported the rapid uptake was a completely different mechanism from the other samples. This is usually because of side reactions. The changes in the X-ray diffraction (XRD) data needs to be better controlled and/or explained. The PIs need the full kinetic evaluation of any and all transients.
- What type of temperature regulation is on the high-pressure volumetric system?
- The researchers need to quench after each single dose, pump out headspace, and evaluate the possible by-products on a Temperature Programmed Desorption (TPD) system.
- What is the expected surface residence time of any hydrogen?
- The PIs need some cryogenic (77K) data to establish the side reactions versus physisorption.

- The idea of ball milling or grinding the MOF is not desirable. During synthesis, it is suggested that the different conditions to vary nucleation and growth parameters to get different particle sizes.
- The project team should also report approximate volumetric data.
- It was not clear whether there were any plans for using a bridging material. The investigators synthesized a number of MOFs with various functional groups. Only one sample, on oxygen-functional group modified MOF (MMOF=O), shows promising capacity, but they need to demonstrate reversibility. The XRD clearly indicates that a structural change takes place, so it is not obvious that a second uptake will be the same as the first. Without a second H_2 uptake curve, we can not really be sure. Either way, it does not look like the group will meet 2011 go/no-go of 5 wt.%.
- Seven or eight MOFs were synthesized. Single point hydrogen absorption data measurements were collected on six of these. The benchmark data are far from the DOE goals. The validation of reversible hydrogen storage was not performed.
- The project shows decent progress. The project team made a series of MOFs with different functionality pore size.
- The PIs are looking at a variation in synthesis conditions.
- In reference to 2.4 wt.% at 80 bar in oxygen double bond material, the kinetics are slow and the results about desorption are not yet known.
- This seems to be a very challenging project, but they got results pointing toward the right direction by introducing oxygen as a functional group. This approach increases the hydrogen uptake to 2.4 wt.%, which is still quite modest and slightly higher than for interstitial metal hydrides. What information is there about cycle life?
- While the project is less than a year old, it is expected that progress will pick up in the second year.
- A great deal of technical progress has been made with respect to the rapid synthesis and property screening of spillover MOF materials. At the outset, it was good to see a validation of results from the literature. The primary focus of this year appeared to be on evaluating (approximately 8) MOFs possessing diverse surface chemistry, pore diameter, and structures. Based on the final uptake data, some structure-property trends were identified. What was not analyzed yet was the impact of processing, such as ball milling, on the MOF materials themselves. What is the impact of processing on the structure of the materials? This suggested investigation is already listed in the future work, which is a great next step. The idea and practical benefits of trapping are not yet clearly understood. The PIs might consider deemphasizing this work unless practicality is demonstrated.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- The MOF synthesis and spillover work is naturally well tied but removed from the theoretical input by Milton Cole. Cole does, however, tie in with the synthesis of flexible MOFs and calculations of the thermodynamic functions. There is no evidence of further collaborations other than these three aside from what is planned in the future.
- The project team needs to set up a collaboration for independent validation of data and/or look at downselected materials for multi-step, high-pressure systems. They should consider National Renewable Energy Laboratory (NREL), Southwest Research Institute (SwRI®) and/or National Institute of Standards and Technology (NIST) would be possible options. Validation is very important to reach the 5.5 wt.% point to determine go/no-go.
- It is unsure with whom they are collaborating. It looks like all collaborations are pending or ongoing discussions. The project could benefit from collaborating much more with the sorption community.
- Their collaboration activities are mostly focused on modeling. Validation through collaborations with other laboratories is highly recommended.
- The project's number of collaborators is low, but the team is planning more.
- There are some collaborations ongoing with future plans to increase the network.
- This project relies on collaborations among three institutions, although the specific roles of these partners were not given in great detail. Additionally, with respect to collaborations outside of this project, it appears that some are being developed, for modeling for instance, which should benefit this project. This project is still relatively new, so some understanding is given to the time it takes to develop these collaborations.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- There is continuity in the future plans, but the focus on the DOE goals needs to be more concentrated. Spillover should be the emphasis, while the other aspects are reduced in priority. Given the description of how sample conditioning affects spillover, there is a great need to examine this along with the factors affecting kinetic uptakes.
- Apparatus modification that will allow for desorption studies are also necessary and needs to be addressed in the near term.
- There is no clear roadmap to meeting the targets in the future work plan.
- A more systematic study is necessary to incorporate predictions, materials design and synthesis, characterization, and feedback to modeling. This approach would require more effective use of collaborations.
- Getting validation of the measurement techniques through a collaboration with NIST is an important step.
- The future work appears to be relevant to the project's goals, but the outlined approach is vague.
- This project is good but needs the benefit of outside collaboration with experts in MOFs.
- The future work presents a great extension of the current results. In particular, the investigation and impact mixing methods and surface chemistry should be very instructive for further definition of structure-property relationships. The suggested future work involving modeling and/or techniques would be additionally valuable, where spillover could be characterized *in situ*. The PIs should deemphasize their work on trapping unless practicality can be demonstrated and understood.

Strengths and weaknesses

Strengths

- The individual capabilities of the PIs bring a lot to the project, but it needs to be focused in one direction. The approach to validate spillover is important, and attention needs to be placed on understanding the processes involved.
- The insight of the PI and the ability to make a range of materials for evaluation is a strength.
- The project team has done nice work on the synthesis of MOFs with different ligands and functional groups.
- The team's engineering aspects in trying to get at the mechanisms and important parameters is considered a strength.
- They are a very capable team who are making great progress, especially with the synthesis and characterization of spillover MOF materials.

Weaknesses

- The diversity of interest of the three PIs is pulling the research in three different directions and unequally at that.
- There is a lack of multi-cycle data and in any evaluation of side-reactions in the current data set presented.
- The project will not likely meet its go/no-go point since only one sample shows a capacity greater than the benchmark. There is no clear path identified for meeting targets.
- The project is lacking significant validation of results through cycling and collaborative validation measurements.
- The team showed little understanding of MOFs.
- It is not clear how a higher-gravimetric capacity will be obtained. The project has a go/no-go decision in the third quarter of the second year on reaching more than 5.5 wt.% at 300K-400K. If this is not achievable, this approach may be too challenging for the project to continue.

Specific recommendations and additions or deletions to the work scope

- They need a design of experiments, not just variation in the parameters.
- There should be more efforts in determining the controlling factors for spillover and additional work on identifying the barriers to improving the uptake and desorption kinetics.

- The PIs should focus on spillover on known materials and their characterization. They should reduce their other efforts at this point in time, especially since the 77K and 87K uptakes are so low that there is little hope of pressure and temperature swings reaching DOE goals.
- The PI needs to set up a collaboration to validate absorption data and assist with the adsorption and desorption evaluation of the materials used for a faster downselect process.
- A more systematic study of how various functional groups affect storage capacity would be useful. If a high capacity material is not found in this effort, they may be able to learn something (e.g. empirical trends) that could be applied to future work.
- It is not certain that the trapping work is appropriate for an applied program. The science is interesting, but it's not clear how this type of material would work in a real system.
- Validation of results should include H_2 sorption measurements on blank samples under the same conditions as spillover and hysteretic adsorption results. The materials should be cycled to confirm reversible hydrogen storage and not simply hydrogen uptake through other reactions.
- The project team needs to gain a deeper understanding of MOFs, perhaps by entering the research community who focus on MOFs and collaborating with more of them.
- It is recommended that less, or no, emphasis be spent on the approach with H-trapping, and instead focus on the addition of functional groups on the MMOFs and the other tasks they listed in their future work slide. It is also recommended to add on a task for durability (cycle life) of the upselected materials.

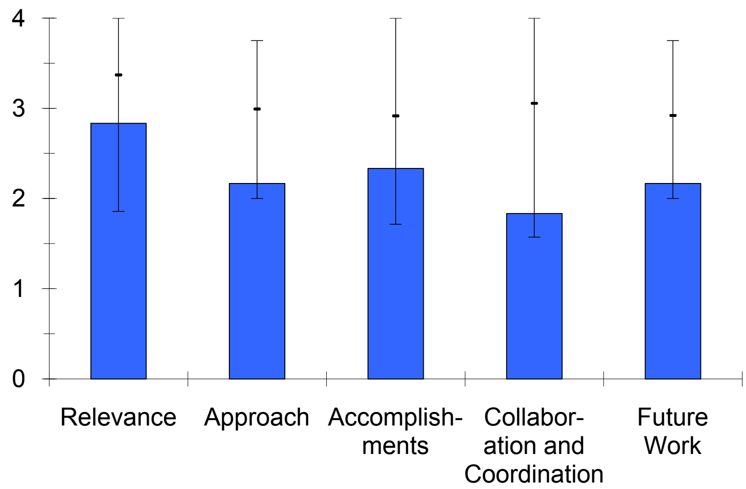
Project # ST-25: Polymer-Based Activated Carbon Nanostructures for H_2 Storage

Israel Cabasso; State University of New York-esf at Syracuse

Brief Summary of Project

The overall objective of this project is to develop and demonstrate reversible nanostructured polymer-based carbon on hydrogen storage materials with materials-based volumetric capacity of 50 g H_2/L, with potential to meet DOE 2010 system-level targets. Tasks are to: 1) perform precursors processing, including material development, modification and characterization; 2) perform nanostructured carbon preparation, including high surface area activated polymer-based carbon, analysis morphology, and production scale up; 3) investigate hydrogen storage, including physisorption and chemisorptions; and 4) perform hydrogen storage testing.

Overall Project Score: 2.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The synthesis of microporous carbons is of relevance to the program, as it addresses the problem of volumetric storage in materials of this type.
- This project successfully develops high surface area polymer-based carbon which, with an appropriate doping, will combine physisorption and chemisorption of hydrogen with an improved hydrogen binding energy. The material processing can be scaledup, inexpensive, and approach some of the DOE targets.
- Nice focus on materials exhibiting a combination of high gravimetric storage capacity and durability (cycle life).
- The overall value of investigating inexpensive adsorbent materials is relevant and supports the DOE RD&D objectives. The explanation of the path to the target through improved surface area or pore size provided a good vision of the project focus.
- The program is focused on development of high surface area polymer-based carbons. The PI showed reasonable storage at cryogenic temperatures. However, some of the positive results are somewhat surprising, and the PI made no attempts to explain these results even when asked.

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- The approach of the PI makes sense, but aspects of the work defy any rationale in motivating the need for the assessment of the materials that have been synthesized. For example, the reason for the addition of lead zirconate titanate (PZT) was done without any strategic explanation given. While some important empirical discoveries have been achieved accidentally, many things are unclear, such as:how the PZT was incorporated; the difference in surface area after incorporation; the amount of PZT added and whether the addition of this PZT was used as part of the weight percentage (wt%) normalization of the gravimetric uptake.
- The approach to make the carbon material is good; however, it requires better understanding of its morphology; e.g., connectivity of the pores.
- It is not clear why investigators doped with PZT.
- It was not clear what they are trying to do with the Monte Carlo calculations. Slide 16 suggests titanium and magnesium open up the structure, but it doesn't look like there has been any attempt to make this material.

- The project approach was unclear. It was difficult to gain confidence in the results since the approach needed further definition. During the AMR presentation, it was explained that the best results were from material that was 4 years old, but this information was not linked to the project approach or other materials. The volumetric capacity calculation needed further development and explanation to gain confidence in the values presented.
- The results seem to show that the PI has fairly good control over the morphology and chemistry of the carbons created.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- No measurements of carbon that the reviewer is aware of have ever shown excess sorption values of over 6 wt%. Data as measured by the Gas Technology Institute (GTI) appears to show 7 wt% uptake at around 50 bar pressure. This is an astonishing value for a pure carbon. Unfortunately, slide 11 shows a collection of data from different laboratories on different materials, so it is difficult to assess the utility or accuracy of these results. The only data the reviewer does trust are those from Quebec, showing the expected 5.5 wt% uptake seen in most carbons. Data from Hiden systems could be inaccurate.
- The plot on slide 19 is a misrepresentation of the uptake as it is data taken at one bar pressure. What this data actually show is the enhancement of adsorption enthalpy at low pressure. The PI cannot extrapolate data taken at one bar to determine the overall uptake of a material. While the initial slope of the isotherm with enhanced enthalpy will be sharper, the ultimate uptake at the surface excess maximum invariably depends on the surface area.
- In slide 12, the PI notes a 46 g/L volumetric density, but this is a deceptive number, the derivation of which has not been explained. The implication is that this is the density that might be achieved by using this material in a tank. What the PI has omitted is the fraction of bulk density by which this material can be compressed. After that, a gas law contribution needs to be worked out for the rest of the tank volume. PIs should not be allowed to present a volumetric density unless this analysis has been worked to completion and an actual fraction of material bulk density is presented.
- The progress was made in testing and in reproducibility of production. Very good numbers were obtained for gravimetric capacity at 77K. Progress in theoretical simulation gives some indication on prospects of having higher temperatures adsorption using transition metals.
- Very large volumetric values—hard to believe for uncompressed powders.
- Results show very high capacities, but for samples that were 4 years old. It's not clear what new materials development work has been going on for the past four years, whether the new materials show lower capacities, or whether there were no new materials developed over the past four years.
- All of the diatomic hydrogen uptake testing was done by collaborators. It is not at all clear what the investigators actually did with their funding.
- The major accomplishment is that at 77K, some of the materials investigated here show high surface area and up to 7 wt% materials hydrogen content, which is close to meet the DOE system gravimetric target, however the materials' capacity must exceed the system target. Kinetics is, however, not evaluated, or perhaps not reported here.
- The concept of introducing a carbon alloy is interesting and the simulations show promise; however, the preliminary experimental results only show a lower gravimetric capacity and it's unclear how they will proceed from here.
- It was difficult to assess the accomplishments of this project since the presentation needed further explanations of the items in the project summary. The comparison of excess gravimetric hydrogen uptake isotherms from various sources was a useful assessment.
- The volumetric capacities look (surprisingly) good?! There needs to be an explanation for this high capacity in such a low-density, high-surface area material. The PIs acknowledged that they did not measure these capacities directly, but rather inferred from density. Given that these numbers are so surprisingly large, a direct measurement would be necessary (or at least welcome).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- The extent to which collaborations are noted in the presentation are only relevant as they indicate measurements performed at other laboratories. The nature of any real interaction that may have occurred is not clear.
- There was increased collaboration, especially in the area of testing and establishing independent measurements.
- There are no clear collaborations within the DOE Hydrogen Program. Investigators are working with a United Kingdom (UK) group for uptake measurements. The project would benefit from more collaborations with other sorption groups within the DOE program.
- The collaborations listed seem to be for validation of results and for obtaining precursors, but there doesn't seem to be any scientific exchange or discussion with other research groups.
- The collaboration partners seem to be limited and could be improved.
- The previous reviewers suggested that this project should have collaboration (or at least some contact or communication) with others who are working with polyetheretherketones (PEEK). It appears as though the PI is not familiar with other similar efforts in the field.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.2** for proposed future work.

- The PI apparently does not recognize that some enhancement of sorption enthalpy of the melem-based material has already occurred. Not having presented an actual value, the rest of the goals of their research, while sensible in principle, leaves the reviewer to wonder what approach the PI will take in addressing these values. In addition, it is not only the initial enthalpy value that needs to be addressed, but the range of isosteric heat over the sorption range (as other PIs have presented). There is, potentially, great interest in materials of this type. The execution of this work in determining the thermodynamic quantities of interest, given the present list of collaborators and the questionable high pressure data that has been produced so far, represents a huge disappointment, given where this research could go.
- The proposed future work in improving the binding of hydrogen is not very clear. It is also not clear if, with the methodology of the project, future improvements of pore/surface area parameters can be achieved.
- The future work plan is appropriate.
- It would be useful to measure sorption enthalpies (ΔH) for these materials.
- It would be interesting to learn the strategy for obtaining adsorption at elevated temperatures of the PEEK materials.
- The next steps appear to be headed in a correct overall direction, but it is unclear if any of these items can provide enough improvement to achieve the acceptable binding energy. An assessment of the potential for improvement would be useful in the future work plan.

Strengths and weaknesses

Strengths

- Sorbent materials are of interest and show some interesting thermodynamic data.
- This project successfully develops high surface area polymer-based carbon, which, with an appropriate doping, will combine physisorption and chemisorption of hydrogen with an improved hydrogen binding energy. The material processing can be scaled-up, inexpensive, and approach some of the DOE targets.
- Investigators demonstrate high gravimetric capacities and durability.
- The project strength is the focus on the potential of inexpensive adsorbent materials. Also, it appears the project has attempted to make progress with little or no funding for this year.

Weaknesses

- PI does not recognize the fundamentals of the sorption process and hydrogen sorbent interactions.
- This reviewer doesn't see good ideas behind the selection of doping materials. It is not clear if future progress can be made in improving the morphology of the carbon material.

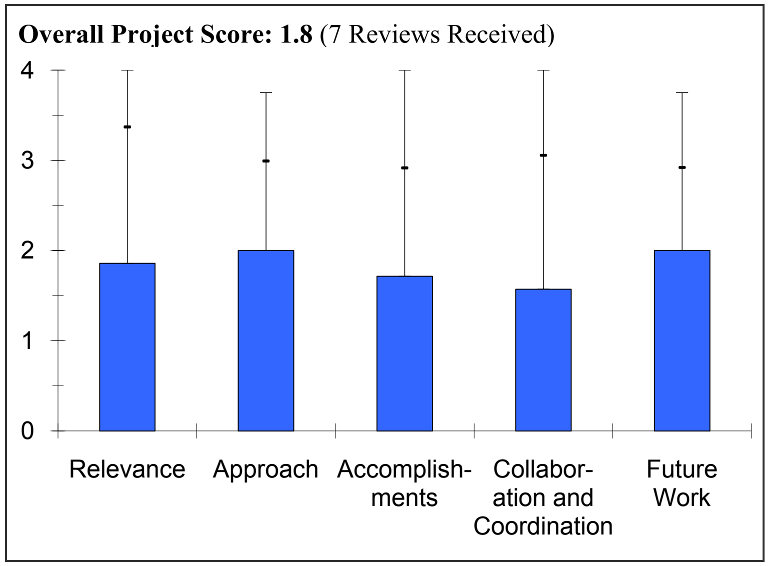
- Reported gravimetric and volumetric capacities seem very high and do not seem to be consistent with other results in the literature. It is not clear what is so exceptional about this material to account for the unusually high capacities.
- The project weakness is the inability to provide the clear analysis execution.

Specific recommendations and additions or deletions to the work scope

- Good characterization of the doping material (state, distribution, homogeneity) is necessary.
- Need to get sorption enthalpies (ΔHs) for all materials.
- Much improvement is needed to clearly identify the project plan. A more systematic study is needed to (1) use modeling to identify promising materials, (2) develop those materials, and (3) test the diatomic hydrogen storage properties. Instead investigators have developed a model for one material and then prepared a different material.
- Independent validation is necessary and should continue.
- No recommendations, this project has ended. [DOE note, this is an active ongoing project that has not ended.]
- A recommendation is not to add any scope, but concentrate the project's effort in developing and explaining the analysis techniques. The project should also provide a potential for improvement assessment to direct the next steps.

Project # ST-26: Capacitive Hydrogen Storage Systems: Molecular Design of Structured Dielectrics
Robert P. Currier; Los Alamos National Laboratory

Brief Summary of Project



The objective of this project is to use applied electric fields to facilitate high hydrogen adsorption/loading under more economical ranges of temperature and pressure, with controllable uptake/release dynamics, and with moderate thermal management requirements. Features of the project include: 1) materials will be tailored to porous substrates with controlled dielectric response; 2) an electric field is applied across the porous substrate; 3) the field produces controlled, localized, dielectric response in the substrate; 4) localized polarizability should enhance hydrogen binding at those sites; and 5) upon discharge of what is effectively a "capacitor," the displacement field is removed, and, in turn, the energy binding diatomic hydrogen dissipates.

Question 1: Relevance to overall DOE objectives

This project earned a score of **1.9** for its relevance to DOE objectives.

- The objectives of this work are to use 10 to 20 keV potentials in a capacitor configuration in order to improve the normally weak electron correlations effects that promote adsorption of molecular hydrogen onto a surface. A 10 kJ/mole enhancement for the 2500 mole (or 5 kg) quantity implies a 25 MJ energy, which would amount to ~10% of the energy content of hydrogen. While not a bad number *per se*, some of this analysis, as well as the rationale for the 10 to 20 keV potential, would be in order. A gas in the presence of potential fields that are this high will invariable result in arcing and, conceptually, it seems puzzling that this work was funded. Given the overall 2,500-mole hydrogen storage requirement of this program, it is not clear that even in the absence of arcing, that the potentials suggested come close to the 0.1eV/molecule value required for enhanced sorption.
- This project at LANL speculates that the hydrogen adsorption capacities of metal-carbon systems might be enhanced by the presence of local, strong electrical fields from currently to-be-determined sources. There is minimal theoretical justification provided that sufficient bonding effects will be possible at practical field strengths or that the materials will remain stable in the presence of these fields. However, if significant hydrogen can be stored in the presence of an electrical fields, then the loss of the field would presumably give instantaneous desorption of the bound hydrogen, potentially generating high pressure in the storage vessel, which could rupture.
- It is quite surprising that this is even being funded given that: 1) it is impractical for a tank to have such high electric fields applied across it; 2) given the powder nature of most adsorbents (current technology), there is no way that you can polarize everything in a beneficial manner; and 3) any conductive path will neutralize the field.
- This is not a relevant technology, but is interesting on an atomic scale for academic purposes.
- This project is very relevant. Increasing the binding energy of metal-organic frameworks (MOFs) by additional physical binding is definitely a plus and is worth exploring.
- The project is relevant to the DOE storage goal of increasing physisorption storage capacity at room temperature.
- The project needs a conceptional device design and performance estimate to determine relevance.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- Again, the overall concept of this work is puzzling. The researchers themselves recognize from slide 6 that avoiding breakdown potential needs to be avoided. In any practical system, this will be impossible to do.
- The project consists of three main aspects: 1) development of a theoretical framework for the feasibility of enhanced hydrogen adsorption by localized metals in an organic host; e.g., MOFs, 2) preparation and dielectric characterizations of potential MOF compounds that could exhibit increased adsorption in the presence of strong electric fields, and 3) design and fabrication of a test system to perform initial feasibility demonstration experiments. A go/no-go decision point is scheduled in fiscal year 2010, but has not yet occurred. The test equipment is apparently not yet ready to permit any evaluations.
- The methodology and approach is actually reasonable. The combination of calculations and experiments are usually a good indicator, but the emphasis on detailed calculations to find the right material is a bit premature, given the fact that we do not really know if there is any improvement in well-known systems.
- Continuing from the previous sections, this is not a relevant technology for on-board storage.
- The approach is based on theory and experiment.
- The concept that applying a voltage-modifying, localized polarizability of high surface area materials should enhance hydrogen binding at those sites is interesting and attractive if possible. However, the R&D approach has been spread across diverse aspects of the whole storage concept. There does not appear to be enough focus on proof of concept testing that an applied electric field could actually cause significant changes in hydrogen storage capacity.
- The approach is generally unique; however, several key items need to be addressed given the high cost of this project.
- This project needs modeling components to predict device performance-even rough calculations. The project also needs to estimate the weight and size of a device and an upper limit on how much storage benefit one would get.
- Need an estimate of field (V/cm) needed to get desired increase in adsorption energy. What dielectric constant is needed?
- It is not apparent that investigators have really examined the criteria for success in this project. The team needs to set success criteria that would be needed for the technology to be relevant for improving hydrogen storage. Simply "increasing adsorption" does not equate to creating technology that will enable hydrogen vehicles.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.7** based on accomplishments.

- While there is a popular trend to view framework structures as a panacea to this program's goals, the implementation for the use of such a material in this application is not apparent. Even if as the PIs point out that linkers in the frameworks they have selected are more polarizable, to what extent is the localized polarization going to enhance uptake? This is a back-of-the-envelope calculation and should have been presented. Also, the synthesis of these materials is often imperfect, leaving remnant solvent in the framework structures and it is not clear how the collaborators who are responsible for this project will deal with this issue.
- Modeling results providing support for the concept were presented and several MOF compounds were apparently prepared while some materials were screened. A test system has been prepared, although initial experiments were not yet reported. Presumably, delays in funding to LANL on this project in early FY 10 has delayed their efforts.
- Given that the presentation is only a few slides in addition to what was presented last year, there is very little to judge this aspect on. They were rather slow on not only moving forward to do real experiments and get results and have very little direction on what kind of MOF would be most useful. The next go/no-go decision will be a crucial marker, and they do not seem to be close to proving if the concept works.
- Relatively fair progress given the funding delays and cuts. Presenters went into time-consuming details and efforts before establishing the validity of the concept, which was not preferable.

- Progress was made on determining dielectric response of test materials. Materials appear to have dielectric properties similar to other capacitor dielectrics. More focus is needed on demonstrating that hydrogen capacity can be enhanced (even marginally) for any of these materials by applying high voltage.
- Not much progress has been made, partially due to funding issues, but also due to the complexity of this project requiring a new measurement system setup.
- It appears that some very complex calculations are being done, but simple ones are not mentioned, such as: the field strength needed to attain 10-15 kJ/mole adsorption energy for a hydrogen molecule; given that number, whether there are dielectrics known that could be used in a device; how much the device would weigh; or how big it would be.
- Testing with carbon dioxide and argon show minimal effects. Need predictions from those results for hydrogen.
- The project team should consider breakdown voltages for MOFs.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.6** for technology transfer and collaboration.

- What is the division of labor amongst the collaborators? While the PI mentions ties through the "Chemical Center of Excellence," there is apparently no working relationship that the PI has with the CoE members. While LANL identified several outside organizations with some past collaborations, there was no indication that they were involved in this project. Also, there was no indication of interactions with any of the DOE-EERE partners involved with hydrogen storage technology.
- There seems to be some sample producing collaborations, but not much that is integral to pushing the research forward. A more detailed presentation indicating how the collaborators interact could help improve this perception.
- The project has yet to be effectively established. They really need to interact with some adsorption experts; they could save lots of effort and time.
- Collaborations show a lot of strength in the area of materials development. More collaboration is needed with institutions that have expertise in precise hydrogen sorption measurements.
- No visible collaboration seems to exist outside of obtaining materials for their system.
- Partners listed, but no indication of their level of involvement in the program is given.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- There are a number of "cart-before-the-horse" plans that the PI has proposed over the next 2 years. Because of the difficulties the reviewer has with the overall concept of this project, any work toward attempting to execute the work as outlined in the proposed work seems to be of rather limited value.
- Among all the tasks described in the future plans for this project, the most critical should be experimental tests on current samples to see if there are enhancements of hydrogen adsorption in the presence of strong electrical fields. Preparation for the go/no-go decision is paramount.
- The FY 10 go/no-go decision must be adhered to.
- The project really needs to primarily prove concept in a simpler way.
- Addressing validation of enhanced capacity has been identified as the first topic for future work. The majority of effort should be focused on this. Understanding the effects of adsorbed impurities on the electrical properties of the test materials by comparison with baked-out samples is lacking.
- It is recommended to relate the project to the well-to-tank efficiency earlier on, in order to establish feasibility. Scoping and clear goals are needed to guide future work. For example, if increased adsorption is demonstrated under a set of conditions, investigators should extrapolate data to estimate conditions required for the technology to be commercially relevant.

Strengths and weaknesses

Strengths

- None.
- A potentially innovative concept if something can be demonstrated via laboratory tests and not just theoretical rationalization. Search for specific types of MOFs with metal atoms that might provide the desired effects.
- Sample environment and measurement capabilities are interesting and could be useful to answering some of the overall questions concerning the premise.
- Novel approach.
- Good collaborations with materials developers. If concept is valid, then it will open up a whole new approach to hydrogen storage.

Weaknesses

- The PI has vastly underestimated the potentials that are required for this work. Even working with the potentials that the PI has assumed will be adequate, these potentials under modest pressures will result in gas breakdown with arcing.
- There does not appear to have been any detailed assessment of the electric field strength necessary to provide for the behavior desired. The theoretical arguments are not convincing as presented at the AMR.
- There are no plans to observe, atomistically, the effects of electric fields. They need diffraction or clear well-known techniques to prove that this works over all the sample and is not a zero-sum game where the field can aid adsorption in one direction but hurts it in the opposite.
- Difficult to implement in practice.
- Too much focus on supporting science (modeling, synthesis, safety, and materials properties) without sufficient evidence that the basic principle is valid. More collaboration with experts in accurate hydrogen sorption measurements needed.
- Efficiency concerns were not addressed.
- Hydrogen molecule potential dissociation not addressed (technical hurdles).
- Safety issues were not addressed.
- There is a lack of focus and clear goals that would enable commercialization.

Specific recommendations and additions or deletions to the work scope

- This project needs to be scaled back in a substantial way. If the PI wants to prove the concept, this needs to be done with less exotic materials.
- The project should focus on preparing for feasibility demonstrations in the laboratory as quickly as possible, spend less effort on theoretical predictions, and perform tests.
- Concept of the benefit of capacitance electrical energy storage and re-adsorption of hydrogen with regenerative braking should be validated with simple calculations.
- Energy loss in maintaining a large differential voltage for a vehicular storage system may be a serious problem. This should be addressed with simple, small-scale measurements on current test materials.
- Decomposition of MOF and other materials in the presence of hydrogen at a high voltage may make these materials impractical for reversible hydrogen storage. Simple voltage cycling of samples in a hydrogen environment could be performed with material characterization (X-ray diffraction) to evaluate whether materials will withstand such cycling.
- Without illustrating the feasibility potential very soon, at least based on estimation results and current data, it is recommended to delete this project.
- Scoping work.

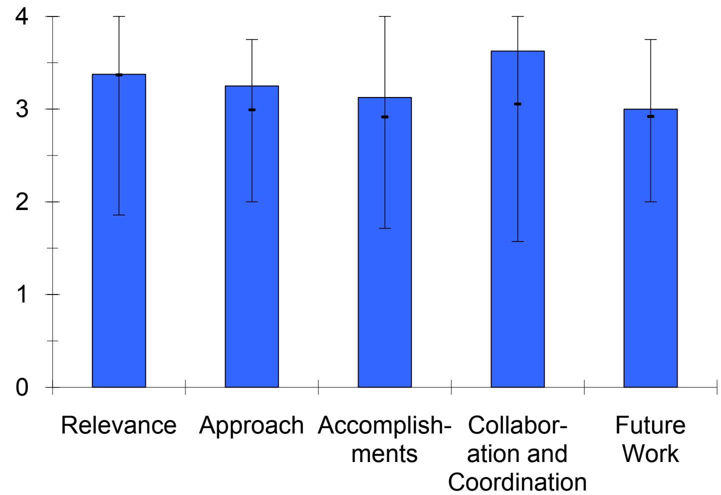
Project # ST-27: Tunable Thermodynamics and Kinetics for Hydrogen Storage: Nanoparticle Synthesis Using Ordered Polymer Templates

Mark Allendorf; Sandia Natioinal Laboratories

Brief Summary of Project

The overall project objective is to achieve tunable thermodynamics for hydrogen storage materials by controlling nanoparticle size, composition, and environment. The key goals for fiscal year 2009 are to: 1) demonstrate and downselect infiltration methods, 2) measure desorption kinetics for simple and complex hydride nanoparticles, and 3) benchmark density functional theory and atomistic nanoparticle models using Quantum Monte Carlo (QMC) to quantify the effect of nanoparticle size on the enthalpy of reaction ($\Delta H°_d$) and develop a compositional tuning method.

Overall Project Score: 3.2 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Destabilizing of complex hydrides that are usually too stable to release hydrogen under moderate pressure and temperature is a key issue. This project strongly supports the Hydrogen Program.
- The project is relevant to DOE objectives and explores tunable thermodynamics for hydrogen storage materials by controlling nanoparticle size, composition, and environment.
- This project seeks to develop a fundamental understanding of the behavior of hydrides confined within nanoporous templates. The work is motivated by early research, which shows that for some hydrides, nanostructuring stabilizes the hydrides while in others, nanostructuring destabilizes the hydride. The researchers are taking a very systematic approach to scaling hydrides within the range of 1-15 nanometers (nm). This understanding is well within the DOE scope for improved kinetics. However, the ability to scale-up the most promising nanoconfined structures in a cost effective manner remains at question.
- One of DOE's objectives is seeking materials that will absorb and release hydrogen under specific temperature and pressure conditions. The proposed research in "tuning" the thermodynamics is extremely relevant to this objective.
- This project clearly supports the Hydrogen Program and DOE research, development & demonstration (RD&D) goals and objectives for fuel cell vehicles. It is compactly focused on one central theme—using nano-confinement techniques to destabilize metal hydride-type hydrogen storage materials. In this sense the project directly addresses the central issue for effective utilization of metal hydrides for on-board fuel storage.
- Prohibitively slow sorption kinetics and large reaction enthalpies, especially in many complex hydrides, are serious obstacles to the successful incorporation of those materials in a practical storage system. Understanding the role of nanoparticle size on the thermodynamics and kinetics of hydrogen sorption reactions using the novel approach employed in this project is an important component of the DOE RD&D program and is directly relevant to DOE needs and objectives.
- The project's objectives are well aligned with the DOE Hydrogen Storage Program.
- The AMR rating criteria for relevance is clearly not appropriate for this particular project; its numerical rating unfortunately suffers from that fact. It does not quantitatively and directly discuss the DOE targets and the potential for meeting them, and should not be expected to. It is a very fundamental project aimed at testing the speculations and calculations that low nanometer particle sizes can result in thermodynamic

destabilization of stable hydrides like magnesium hydride, lithium hydride, etc. If successful, this would be an interesting and valuable scientific finding, but it would not be expected to result in practical engineering systems in the near to medium term.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- It is significantly important to identify the "size effect" using a computational method before designing the size of scaffold for hydrogen storage materials to be placed. In the experimental part the project is ready to use a wide range of pore sizes for the research.
- The approach of the project is to use different templates such as metal-organic frameworks (MOFs), covalent organic frameworks (COFs), zeolitic immidazolate frameworks (ZIFs), and block copolymers to create nanoparticles by infiltration, reduction, and stabilization. The experimental synthesis work is supported by theory (density functional theory (DFT), Quantum Monte Carlo (QMC), and nano-prototype electrostatic ground states (NanoPEGS)) and characterizations.
- The approach involves synthesizing the pore structure, confining the hydrides within it, and characterizating the materials before and after desorption of hydrogen. This is a very solid and systematic approach.
- In particular, the systematic variability of size has led the researchers to determine, that for lithium borohydride, particles below a 7-nm size, have desorption behavior that is very different relative to particles above that size.
- The approach of infiltrating metal hydrides into novel organic frameworks is innovative and well conceived. The assumption that nanoparticles could be contained in the pore structure of MOFs, and that this may affect thermodynamics, is logical.
- The project is indeed well designed, feasible, and integrated with other related efforts. The approach has both depth (detailed study of specific destabilization methods) and breadth (looking at a variety of promising confinement media). The quality of the synthesis, testing, and characterization science is very high.
- This project uses ordered polymer frameworks as nanostructure-directing agents for generating hydride nanoparticles. This is a novel approach that is allowing the team led by the Sandia National Laboratories (SNL) to probe the effects of nanoparticle size on sorption thermodynamics and kinetics in a systematic way. To my knowledge, this is the only project within the DOE program that is investigating these effects using particles with well-controlled sizes.
- Although the approach is innovative, there are limitations, especially with regards to unwanted interactions between (some) hydrides and the ordered polymer template. This can deleteriously affect the sorption reaction characteristics. Also, the stated objective of the project is to “achieve tunable thermodynamics…by controlling nanoparticle size…” However, in this reviewer’s view, it is far more likely that reaction kinetics will be altered. Separating those effects in reactions employing very small sample sizes is a challenging experimental endeavor.
- The approach is unique in that it aims to improve the thermodynamics and kinetics of previously known chemical hydride materials by encapsulating nano-size particles in the pores of carbon templates such as MOFs.
- The use of polymer templates for making ultrafine hydrides by infiltration techniques is an interesting and new approach.
- The theory on nano-sized hydrides has long been in confusion; it would be good if this project could resolve that problem once and for all. Having said that, there is relatively little thermodynamic-based diatomic hydrogen desorption/absorption testing relative to theory and other non-thermodynamic measurements.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- A wide range of scaffold have been readied. Using scaffolds some differences have been observed comparedto the materials without scaffolds, but it seems to be kinetic phenomena and not destabilization of materials. In other words, control of thermodynamic properties has not yet been shown.
- Template materials providing a 1- to 20-nm range of pore sizes were prepared. Infiltration with metal hydrides was studied for a number of hydrides. Formation of infiltrated hydrides and their destabilized behavior was demonstrated. Application of modeling (DFT and QMC) was critically evaluated.

- There was no clear delineation between work done prior to this funding year and during this funding year. However, assuming ALL of the work presented was a part of this funding year, the results are many.
- Specifically, the research group has investigated the stability of MOFs (used as templates in some cases) and has revealed a difference in the behavior of copper-based MOFs used relative to zinc-based MOFs (which degrade at desorption temperatures for the hydrides).
- The research group has transmission electron microscope (TEM) tomography results, which show the presence of hydrides throughout the pore structure; i.e., filling of the 1- to 15-nm pores with hydrides).
- There has been some significant progress toward achieving the goals set forth in this project. It is noteworthy that the hydrides are compatible with the MOFs but not with ZIFs. It is also noteworthy that some of the hydrides have shown reduced desorption temperatures when in the MOFs. Some kinetic data has been presented, but nothing in the way of thermodynamic data has been presented.
- Excellent progress has been made toward goals and objectives. Examples include the following:
    - Templates in the 1- to 15-nm size range have been synthesized.
    - Confinement of particles as small as 1.5 nm diameter has been archived.
    - Observed enhanced desorption in sodium aluminum hydride/MOF vs. bulk.
- Solid results have been obtained on preparation of appropriate templates (especially MOFs, block polymers, and resins) with well-controlled pore size. Good results have also been obtained from the modeling of high symmetry clusters and from understanding the limitations of the modeling approaches for understanding nanoscale hydride behavior in ordered templates.
- The successful demonstration that a metal hydride is compatible with an MOF template upon infiltration is an important first step toward the development of controlled nanostructures. However, the degradation of ZIFs with hydride incorporation underscores the sensitivity of the template materials to successful formation of the hydride nanostructures. Also, serious caution must be exercised in interpreting results that may be compromised by the formation of chemically tenacious adducts that have proven to be problematic during solvent-based infiltration of aerogels with hydride compounds.
- Results obtained on sodium aluminum hydride nanostructures formed by melt infiltration into cylindrical pore carbon seem to be similar to results on much larger particles formed by infiltration into aerogels (HRL Laboratories and United Technologies Research Center (UTRC) work). The questions remain to if there a measurable pore size dependence and are these particles really small enough to show a change in thermodynamics, and, if so, how the can surface effects be ruled out.
- The synthesis and infiltration technique of the hydrides in the nanoporous carbon templates has been demonstrated.
- The project's hypothesis is verified by the lower desorption temperature observed in some systems.
- With the project nearly 60% completed, uncertainty seems to dominate still.
- Various theoretical approaches are giving contradictory or negative results. It seems there is little time left in the project to complete the theory, much less accomplish experimental confirmations.
- The one experiment purporting to show nanoparticle destabilization (sodium aluminum hydride, slide 10) really shows kinetic improvements, I believe, not thermodynamic destabilization. Sodium aluminum hydride is already thermodynamically unstable at room temperature. Positive fine particle effects on kinetics have been shown in many, many papers.
- To their credit, investigators have shown a great versatility in making templates and infiltrating them with hydrides.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- Collaboration between computational and experimental people is excellent.
- The project has an extensive network of first-rate collaborators, both in computational modeling, experimental work, and characterization.
- The project is a collaboration among six different institutions. This broad collaboration permits the avaibility of specialized characterization tools (including neutron scattering) and computational research (including DFT and QMC).
- More reuslts from the nuclear magnetic resonance (NMR) and neutron characterization "branches" of this project should have been included within the presentation. This is an area suggested for future improvement.

- There is an impressive list of active partners in this research who are making significant contributions.
- Collaborations seem appropriate and well coordinated. Roles and responsibilities are clearly established. The project leaders reached all the way to Germany to bring in unique expertise. Research on the synthesis of confinement media is at a very high level. The overall approach to characterization is also impressive.
- There is excellent synergy between theory and experimental work. Significant contributions have been demonstrated by all team members. This is a well-coordinated research effort.
- There is a good collaborative plan in place.
- There are several very well-qualified collaborators, but they seem to be mostly experts in theoretical approaches and non-hydrogen experimental measurements. It is not clear who is doing the work, if any, on measuring thermodynamic properties (pressure-composition isotherms (PCT) and van Hoff analyses).

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The size criteria that is the border of the bulk and cluster has been clarified for some of materials. The next step will be to prepare the appropriate sample smaller than the criteria in a scaffold.
- Many experiments were proposed for future study—including a delineation of size effects at the smallest range, i.e., between 1 and 5 nm. Researchers suggest differences in behavior as size scales from 1 to 5 nm, but more experiments are needed to verify this.
- The proposed future work is reasonable. They plan to continue with the synthesis of the hydride and MOF combinations and to do the thermodynamic and kinetic studies.
- The future plans clearly build on past progress and are sharply focused on the pivotal technical barriers for metal hydrides. The individual tasks are neatly connected. The project is designed to either succeed, or at least determine with confidence, what the performance limits are going to be for metal hydrides.
- Future plans address the important issues that have been raised thus far in the project. The go/no-go decision in 2009/2010 for the compositional tuning effort is appropriate. It will be important to focus on cycling and the extent to which reversibility can be demonstrated in the nanostructures compared with results obtained in bulk materials.
- Future plans should include a quick assessment of the proposed system to meet the DOE hydrogen storage targets.
- Though not specifically addressed by the project team, it is clear from the results that there is a huge trade-off for their proposed approach; namely, improved desorption kinetics at the expense of drastic reduction in storage capacity. There should be a go/no-go decision point for the calculated theoretical limit in storage capacity of an ideal system.
- The remaining work proposed for the remainder of this project seems like more of the same.
- Future plans (slide 17) say nothing about experimental determinations of thermodynamics. The section "dehydrogenation thermodynamics and kinetics" on slide 17 talks only about kinetic measurements. "Tunable thermodynamics" (contained in the title) should be at least half of the "dual approach" of this project.

Strengths and weaknesses

Strengths

- The project firstly identifies the border of bulk and cluster by calculation. Then, it synthesizes the clusters in various scaffolds following the computational achievements.
- The project develops a new type of material based on tightly dispersed nanoparticles of hydrogen-absorbing phases. It has the potential to overcome kinetic and thermodynamic limitations of these phases to achieve characteristics desirable for hydrogen storage applications.
- The researchers use a very rational approach to tune the properties of hydrides via nanostructuring. This is commendable—results are many and impressive.
- The project team consists of a group of well-qualified scientists with the expertise needed for the project.
- The project has strong leadership, creative approaches, and world class investigators.
- The project has an extremely capable experimental and theory team. A novel experimental approach is being employed that has the potential to provide in-depth understanding about important research issues concerning the effects of nanoscale structure on thermodynamics and kinetics of metal hydride sorption reactions.

- Hydride encapsulation synthesis capabilities have been demonstrated.
- There is good collaboration in material characterization and accompanying computational methods.
- The PI and collaborators have good theoretical expertise.

Weaknesses

- At present, real destabilization has not been found. It is uncertain if the methodologies to synthesize the cluster are under precise control of the size as the computation predicts.
- The project needs better TEM and electron energy loss spectroscopy (EELS) work for direct verification of dispersion of phases and their crystallographic characteristics. Not sure what "Wulff construction" is. If it is a shape of minimal surface energy facets, the PI should state whether it determines stability. the reviewer questions if "Wulff construction" a macroscopic concept and therefore not applicable to nanosize particles.
- There seems to be some confusion as to whether thermodynamics are tuned or if kinetics are varied by nanostructuring. To date, the researchers have yet to sort out this issue. One of the major barriers is the synthesis of enough nanoconfined material for measurement via Sievert's apparatus (or similar) to yield thermodynamic data. The difficulty which arises in attaining enough nanoconfined materials for the purpose of thermodynamic measurement also leads this reviewer to question the ability to scale-up the approaches to commercially viable and cost effective techniques (if a system is identified to be promising for meeting DOE targets).
- The integration of NMR and neutron scattering techniques are not complete.
- No progress has been made on the thermodynamics. An effort should be made to determine if the materials absorb and release hydrogen reversibly while infiltrated into the MOFs.
- None are apparent.
- A more complete description of obstacles and barriers to achieving controlled nanostructure dimensionality is needed. In these experiments it has been challenging to discriminate between kinetic and thermodynamic effects. A comprehensive research effort that provides a compelling pathway to understand these differences is needed.
- The trade off in hydrogen storage capacities of proposed infiltrated systems not addressed at all. This can potentially be deterimental to the project's success.
- Diatomic hydrogen measurements are too weak and limited, so far, to confirm theoretical predictions (the PI admitted this during the question and answer part of the presentation).

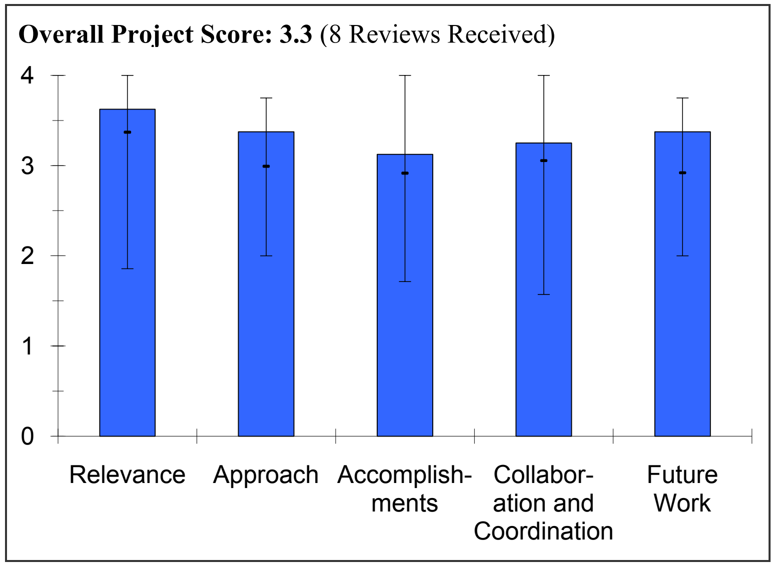
Specific recommendations and additions or deletions to the work scope

- The original idea is excellent, but how to make it real seems to be a little bit weak. There have been a lot of the similar kind of attempts using various materials, however, to make nano particles smaller than a few nm is critical.
- No recommendations for additions or deletions—only a suggestion to more fully integrate the existing tools; i.e., NMR and neutron scattering) with synthesis work.
- Stay the course. I wouldn't change a thing.
- Greater emphasis on discrimination between kinetics and thermodynamics effects in hydrides should be incorporated in nonporous materials. Focus on drawing a definitive conclusion about the changes in sorption reaction properties as a function of particle size.
- It may be useful to consider supercritical fluids as solvents (as in work by Jensen, et al.) to facilitate hydride infiltration. That approach could avoid the formation of stable adducts which can confuse interpretations of results.
- Carry out a quick theoretical calculation of combined physi-sorbed and chemi-sorbed capacities of idealized systems to determine the potential of the proposed approach to meet DOE targets in order to justify further work on this pathway.
- Make pressure-composition-isotherms (PCT) measurements to quantify thermodynamic changes, if any (the PI strongly promised to respond to this need).

Project # ST-28: Design of Novel Multi-Component Metal Hydride-Based Mixtures for Hydrogen Storage
Christopher Wolverton; Northwestern University

Brief Summary of Project

Three materials classes: chemical, metal/complex, and physisorptive have been divided into DOE Centers of Excellence (CoE). The overall objective of this project is to combine materials from these distinct categories to form novel multicomponent reactions. Systems to be studied include mixtures of complex hydrides and chemical hydrides; e.g., $LiNH_2 + NH_3BH_3$, and nitrogen-hydrogen-based borohydrides; e.g., $Al (BH_4)_3 (NH_3)_3$. These types of combinations have only recently begun to be explored, but initial results look very promising.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Computational approach to develop novel hydrogen storage materials is one of the ideal ways, but it is very challenging.
- The project is relevant and may be critical to ultimate DOE objectives–finding light reversible hydrides.
- This project does an excellent job of computationally narrowing down reaction pathways in combined boron and nitrogen containing hydride mixtures. However, the computational work is very far ahead of the experimental synthesis work. This may be because the start date (a little over 1 year ago) has been only enough time for the computational branch to develop a system of proposed reactions to be pursued experimentally.
- This project does fully support the Hydrogen Program and DOE research, development & demonstration (RD&D) goals and objectives. It offers the possibility to identify and test new hydrogen storage material concepts with the potential to meet on-board storage system performance targets.
- This project is exploring a new class of multi-component, complex hydrides for reversible and non-reversible hydrogen storage applications. It directly complements work that has been performed at the Chemical Hydrogen CoE (CHCoE) and the Metal Hydride CoE (MHCoE). Specifically, the project explores compounds from different CoE material categories. This comprehensive, cross-cutting study is exploring novel material mixtures in a new way. The project is well-aligned with DOE RD&D objectives for hydrogen storage.
- The project is relevant in that it aims to understand the physical and chemical as well as the charging and discharging properties of multi-component hydrogen storage systems made up of various hydride systems.
- The project aims to meet DOE objectives and barriers such as weight, volume, and rates. In fact, the focus is mostly on weight, with nothing on volume.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Experts working on this project utilized the most advanced software for exploring novel hydrogen storage materials.
- The project approach combines powerful and tested computational methods with experimental verification of the validity of the theoretical predictions.
- The researchers have developed a systematic approach to combine borohydride and ammonia containing systems (and to explore many compounds and their associated reactions). The researchers also suggest that

another level of tunability can be achieved by changing the cation on the system (and the researchers have moved to include calcium cations).
- This project is indeed sharply focused on the key technical barriers for on-board hydrogen storage. The basic approach is to combine candidates from the three distinct categories of hydrogen storage material types (chemical, metal/complex, and physisorptive) to form novel multicomponent reaction media. The hope is that some special thermodynamic and/or kinetic synergies will unfold that will bring hydrogen storage capacity and diatomic hydrogen uptake and release performance into the range needed to meet on-board storage system targets.
- This project employs both experimental and computational modeling methods to explore novel multi-component reactions of (primarily) light metal complex hydrides and nitrogen-based borohydrides. The large array of reactions that can be assessed using these starting materials, as well as the potentially high gravimetric and volumetric storage capacities that can theoretically be achieved, make these compounds especially interesting candidates for reversible and non-reversible hydrogen storage applications.
- The experimental studies and theory work are strongly coupled, and a well-formulated plan is in place to converge on the most promising candidates. The division of effort (computational prediction, measurements, and kinetics/catalysis) provides a good way to efficiently identify and test new systems. However, as with most complex hydrides, the problem of prohibitively slow kinetics remains a serious challenge. Only limited attention is given to this critical problem.
- This reviewer is not clear what the scientific basis behind the project's approach is. It appears like the PIs are hoping to find success by mixing X and Y, even though they are aware X or Y separately is not the material of choice!
- The approach is very sound, namely the use of various theoretical calculations to predict new reactions and materials that might result from mixing two or more classes of hydrides.
- The intent to follow up on theoretical predictions with experimental confirmations is a critically important part of the plan.
- The project, at least as planned, is an attractive combination of theory, experiment, and original equipment manufacturer (OEM) needs.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **3.1** based on accomplishments.

- Thanks to prototype electrostatic ground state (PEGS), that does not need crystal structure data for performing density functional theory (DFT) calculations, computational results can predict novel materials with their structures. The presentation showed the splendid agreement of their computation results to experimental ones nicely.
- Significant progress was made in predicting new compounds and reaction paths, and finding their energies.
- The work on catalysts is not very clear to the reviewer.
- Many computational results were presented, and the most promising reactions were narrowed down. However, the experimental results development seems to lag behind the computational developments. This should be corrected for next year.
- Progress has been significant considering the amount of work done and the nature of the findings to date. The coupling of theory and experiment is carried out very nicely in this project. What has not yet happened is finding the loadstone combination of materials that actually and demonstrably overcomes the barriers that the project is addressing.
- This project is off to a good start. Computational screening has identified a large number of multicomponent materials from several material classes whose predicted thermodynamic and storage capacity properties make them potentially viable storage candidates. The experimental effort is obviously just ramping up, but some interesting preliminary results have been obtained.
- Although they are undoubtedly recognized by the project team, three important issues must be addressed: 1) in many related systems, even though the predicted and measured thermodynamic properties seem to suggest that the materials might be ideal, slow sorption kinetics limit the system to operation at high temps and pressure; 2) in the nitrogen systems, ammonia generation is always a concern,; and 3) reversibility and high capacity cycling are often problematic in systems comprising multi-step reactions. Solid plans should be in place to address these issues.

- There is a well defined combinatorial discovery approach for a difficult area of research.
- The project has identified metal amidoborane systems with reasonable theoretical capacity and enthalpy.
- Several new and potentially interesting hybrid materials were predicted by theoretical calculations.
- Unfortunately, the experimental side of the effort seems to be lagging behind the calculation side. There were only a few experimental confirmations attempted, usually just temperature scans without formal dehydriding capacities, thermodynamics, or kinetics obtained.
- Most of the materials studied were based on mixtures of hydrides, borohydrides, and amides with significant potential for the generation of impurity gases; e.g., boranes and ammonia. Impurity problems were acknowledged with only vague paths suggested for mitigation.
- The new possibilities are certainly impressive in terms of gravimetric hydrogen capacities, but not a word was said on the volumetric capacities.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The distinguished scientist who developed PEGS is an "outside collaborator," but without PEGS, this project cannot stand.
- Collaboration with experimentalists seems to be ideal.
- The project will need to add a strong synthesis and experimental partner.
- The project has an extensive network of collaborators, both in computational modeling and in experimental work and characterization.
- The research project is a synergy between experimental and computational groups with the focus on developing new hydrogen storage materials and on enhancing kinetics via catalysis of those materials once developed. This is a very strong strategy.
- Many computational results were presented and the most promising reactions were narrowed down. However, the experimental results development seems to lag behind the computational developments. This should be corrected for next year.
- Partnering or collaboration within this project is well organized and appropriate, as are the links to the Hydrogen Storage Engineering CoE. The roles and responsibilities are clearly established and coordination of research tasks; e.g., theory and experimental validation, appears to be going along nicely.
- Close collaboration among highly qualified experimentalists and theorists is evident. The “in-depth automotive perspective” is an important element of the overall technical interaction.
- The roles of the partners are well defined and complementary.
- The effort seems to be a valuable collaboration among theoreticians, experimentalists, and OEM users. However, it is not clear if and how the collaborations are functioning. For example,it is unclear if the OEM participant is providing input to support the belief that these newly predicted compositions have reasonably near-term value.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- The way of thinking is very clear and the leadership of the PI is excellent.
- This reviewer would like to see not just the lowest energy reaction; higher energy reactions cannot be discarded because they can be realized experimentally for kinetic reasons. Attempting to understand the kinetics of the proposed reaction is very desirable.
- The compounds of interest must be synthesized and the reaction pathways explored. The experiments will verify these reactions using infrared (IR) spectroscopy and X-ray diffraction. This is a good plan for future work.
- The future plans build logically on what has been accomplished to date and remain sharply focused on the key barriers to on-board hydrogen storage. These plans include the following studies:
  - Extend computational search for promising reversible reactions in a lithium-calcium-boron-nitrogen-hydrogen system.

  - Characterize storage properties/reactions of $(NH_4)_2B_{12}H_{12}$.
  - Extend catalyst studies to $(NH_4)_2B_{12}H_{12}$.
  - Continue computational search for 1) novel borohydride/amine compounds and reversible reactions, 2) mixed metal borohydrides, and 3) ammonia borane reaction products.
- The future plans are well formulated, and they provide a good pathway to achieving project goals. However, primary emphasis seems to be on identifying and testing candidate materials having satisfactory thermodynamic properties. Less attention is being paid to kinetics issues (slow kinetics in these systems seem unlikely to be solved by catalysts alone). It is recommended that future plans include more serious computational and experimental efforts on the kinetics problem. In general, the principal obstacles and barriers that are currently perceived should be articulated, and a fairly detailed plan for how to deal with them should be in place.
- Proposed future research is reasonable. It should be made clear that the needed experimental verifications of predicted materials will be accelerated.
- Because this approach involves intermediate stage calculations and predictions, it would seem that it would be more useful in predicting kinetics, not just end-point thermodynamics and weight percentages. This potential future work effort seems to be missing. (During the question and answer part of the presentation, the PI expressed doubt that can be accomplished at this time).

Strengths and weaknesses

Strengths

- Utilizing the PEGS program, novel hydrogen storage materials can be predicted. Collaboration between computational and experimental scientists is ideal.
- The project combines powerful state-of-the-art computational methods with experimental verifications.
- Validity of the approach is validated by a number of successful discoveries.
- The research plans take a systematic approach to examining a large number of systems and reaction pathways.
- The computational results presented are impressive.
- The project incorporates a knowledgeable PI, experienced scientific partners, and a reasonably novel approach (something not tried before).
- The project has a strong team that has all of the requisite computational and experimental capabilities to make good progress on this challenging project. The project uses novel ideas and provides good opportunities for exploring a new parameter space of materials.
- The computational methodology used is excellent.
- Excellent theoretical capabilities.

Weaknesses

- The developer of PEGS is outside of the project.
- The project should consider other reactions than just the lowest-energy reactions. More experimental validation work is desirable. It is not clear what to do when the phases are liquids or amorphous. The lack of kinetics studies in this project seriously undermines the power of the predictions made.
- As said before, the experimental validation appears to lag, somewhat, behind the computational group. This will likely correct itself in year 2 as the computational group delivers promising compounds for the experimental groups to explore.
- None that are obvious.
- Insufficient emphasis on kinetics issues. Slow kinetics have been found to be a serious problem in related complex hydride systems. A cursory study of catalytic effects seems unlikely to provide the guidance needed to satisfactorily address the kinetics challenges.
- There is too much reliance on computational modeling.
- Experiment lacks modeling work.
- System complexity and reversibility challenges are not adequately addressed.
- Project does not have go/no-go decision points.
- The researchers are not following the theoretical (calculational) work with more immediate experimental hydrogen confirmations, which are preferred.

Specific recommendations and additions or deletions to the work scope

- The scientist who is still developing PEGS, and therefore needs good supports, should be included as a project partner or given financial support from the project. As far as this reviewer's understanding, the PEGS program has not been opened to the public, but only a small number of users are currently able to use it.
- This project will benefit greatly if it can organically align itself with an experimental or synthesis project in metal hydrides such as project ST-032. There are some strong synergies between these two programs.
- No recommendation for additions or deletions. The project can be improved by developing supporting experimental data (as already planned by the researchers).
- Try to steer away from reaction paths that end up with final products like boron that are hard to regenerate.
- Combinations that mix physisorbers with chemical and/or metal hydride storage materials should be considered.
- Future plans should focus more strongly on enhancing sorption kinetics. Also, unwanted generation of ammonia in nitrogen-hydrogen containing compounds should be addressed more thoroughly, and strategies for inhibiting ammonia formation should be explored (the statement that “ammonia liberation could be reduced by optimization of composition” is too general—a more detailed description of the approach is needed).
- Verify that the new low temperature hydrogen release is not the result of a simple disproportional reaction of sodium amide. Consider running temperature-programmed desorption mass spectrometry (TPD-MS) on sodium amide alone under the same conditions.
- More in-house or partner measurements on dehydrided diatomic hydrogen purity and a greater focus on mitigating that problem is needed.

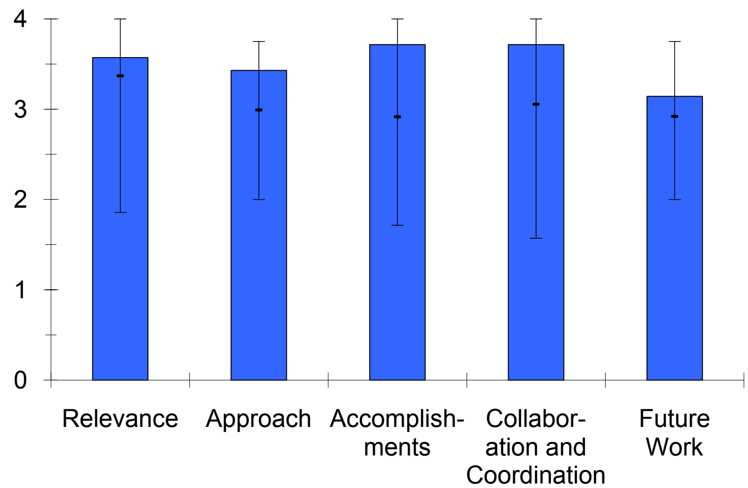
Project # ST-31: Advanced, High-Capacity Reversible Metal Hydrides

Craig Jensen; University of Hawaii

Brief Summary of Project

The overall objective of this project is to develop a new class of reversible complexes that has the potential to meet the DOE 2010 kinetic and system gravimetric storage capacity targets. Current investigations include: 1) magnesium borohydride nano-confined carbon aerogels; 2) reversible dehydrogenation of high capacity borohydrides at low temperatures; and 3) development of a method for the hydrogenation of lithium hydride/aluminum to lithium aluminium hydride at moderate conditions in unconventional solvents.

Overall Project Score: 3.6 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The project is exploring new reversible metal hydride (MH) materials, which is a critical pathway for the program.
- The reduction of cost and the increase in kinetics and high capacity materials all allign with the goals of the program. The underlying concern for cost is very good.
- This project strongly supports the Hydrogen Program and DOE research, development & demonstration (RD&D) goals and objectives. The work is focused on the types of materials that (arguably perhaps) have the best chance of meeting the on-board hydrogen storage targets. The emphasis on new confinement approaches and new solvent embodiments separates this work from what has been done in the past with the same families of storage materials.
- The project is fully relevant to the DOE Hydrogen Program mission.
- The project is generally in line with DOE objectives and targets. Weight and reversibility at reasonably mild conditions is emphasized. Some nominal volume calculations would have added to the project's usefulness.
- The project is relevent to the discovery and optimization of novel on- and off-board reversible metal hydrides. This project really tries to tackle the key challenges associated with materials as opposed to going after "low hanging fruit."

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- The approach is based on the PI's vast technical expertise, network, and experiences.
- Multipronged program with several ways to succeed.
- Scaffolding methods are good for kinetics, but are not so good for mass and volume.
- The magnesium borohydride work was excellent, no one had done that before.
- Regeneration using supercritical fluids is a standard (low-cost) approach to other problems and is a good way to proceed.
- The University of Hawaii project is now honing in on a select few of the more promising storage materials coupled with some novel utilization concepts. Specifics of their approach include:
    - Magnesium borohydride nano-confined in carbon aerogels.
    - Reversible dehydrogenation of high capacity borohydrides at low temperatures.

  - Development of methods for hydrogenation of lithium hydride/aluminum to lithium aluminum hydride under moderate conditions in unconventional solvents.
- Excellent approach.
- The effort is chemistry oriented and aims to improve a very diverse range of hydrides. One might argue the effort is too scattered, but, because of the PI's extensive collaborations, this approach works out very well.
- The approach, in the most general sense, appears to be the utilization of novel chemical approaches for the exploration of reversible and non-reversible metal hydrides. While this very general approach is being successfully applied, there is not a clear thread that links the various storage systems. It was great to see an increased focus on select storage systems verses previous years.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.7** based on accomplishments.

- This project has consistently provided new materials and approaches.
- The project has gotten a lot accomplished in several areas. It was able to get magnesium borohydride into a scaffold, but the big problem, reversibility, was not solved. They achieved 4.28% total, including the scaffold and a huge temperature reduction. However high-pressure and temperature settings did work for reversibility, which was a first for that compound.
- Dimethylether (DME) was used as a liquid with a catalyst to regenerate lithium aluminium hydride. They looked at energy of regeneration, a very nice thing to have done.
- Progress toward meeting goals and objectives was impressive in the past year. Some key results are listed below:
  - Achieved high (60 wt %) loadings of magnesium borohydride in carbon aerogel.
  - Demonstrated that nano-confinement of magnesium borohydride improves dehydrogenation kinetics, but does not change re-hydrogenation pathway.
  - Reversible hydrogenation of magnesium diboride to magnesium borohydride achieved for 12 wt% hydrogen.
  - Reversible partial dehydrogenation of magnesium borohydride achieved under mild conditions.
  - Obtained fully charged titanium-doped lithium aluminum hydride in major yields from direct hydrogenation of titanium-doped lithium hydride/aluminum in liquefied dimethyl ether at room temperature under 100 bar of dimethyl ether.
- The rehydrogenation of metastable alanates in liquified dimethyl ether is a creative approach and the results obtained are very good.
- Very good accomplishments.
- Much has been accomplished in this diverse project, including several old materials made more practical in terms of synthesis and reversibility.
- Unfortunately, the synthesis of aluminum hydride in supercritical fluids (the main intent of the project originally) did not work out, but the PI has abandoned that and has moved, credibly, in a number of useful alternative directions.
- The full reversibility of magnesium borohydride has apparently not been accomplished below the severe conditions of 400˚C and 900 bar. Given the borohydride cluster problem, it was a good try that may set the stage for success some day with this interesting material.
- The synthesis of lithium borohydride from lithium hydride and aluminum-titanium in dimethyl ether is a very positive development.
- Overall, very good progress is being made for a variety of storage materials and reactions. This progress also broadly covers the areas of kinetics, reversibility, regeneration, and discovery. In particular, the most valuable and interesting results are demonstrating reversibility in magnesium borohydride, successfully incorporating metal hydrides into scaffolds and looking into regeneration for lithium aluminium hydrides.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- This project has always maintained a very diverse network of collaborators.

- Work performed with several partners. Partnerships, in part, drove the work.
- The PI is perhaps the most connected researcher in the whole Hydrogen Program. The project is internally well coordinated and well integrated into the MHCoE. Roles and responsibilities are clearly established. One does have to wonder how the 16 partners listed on slide 2 actually participate on a year-to-year basis, but slides 23 and 24 do clarify the importance of collaboration in this project.
- The collaboration is visible.
- The project has excellent and broad collaborations both nationally and internationally.
- The many and excellent collaborations within this project have been the key to its relatively good value.
- Very strong examples of collaboration spanning industry, academia, and even with international entities. The project clearly leverages external analysis techniques and also serves as a resource to others.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- The project is in its final stages. There are limited opportunities to do major work.
- Reviewer suggests that added aluminum be tried with lithium aluminum hydrides.
- The future plans do build on past progress and still clearly address overcoming barriers. But it is unclear what will happen to the work on the nano-confinement of magnesium and aluminum hydrides. It seems that only the borohydride cycling studies and the hydrogenation in non-conventional solvents work will continue. Perhaps knowing what the budget for this project will be in 2011 and beyond would answer this question.
- It is necessary to refine the well-to-tank analysis for lithium aluminum hydride recharge in solvent by Argonne National Laboratory especially given that this system is included in the engineering CoE plan. For example, it is proposed that the cyclic stability is included in these calculations as the results become available experimentally; i.e., recycling of the titanium-aluminum.
- In the short time remaining, future plans look fully adequate.
- Given the impending end of this project, the cited future work is about as good as one can do.
- Mapping of all the materials studied on the DOE weight-volume plot would be useful in the final report. Include both theoretical capacity and actual reversible capacities achieved.
- Future work is a logical extension of current progress in each area. For borohydrides, focus is on optimizing reversibility and understanding tradeoffs. The regeneration work on lithium aluminum hydride shows that this project is focused on the practical aspects of implementing this material.

Strengths and weaknesses

Strengths
- There are strong chemistry underpinnings that drive the work.
- The team is experienced.
- Unique approaches are employed.
- Concern for cost is a background to choices.
- The project incorporates a knowledgeable and dedicated PI, a broad-based team of collaborators, and lots of experience with what won't work in the realm of on-board hydrogen storage.
- Worldwide collaboration and very good progress.
- The project explored numerous promising hydrides. Many potentially useful systems have been identified.
- Good, innovative chemistry.
- Ability to change directions as needed.
- Extraordinary collaborations.
- Very competent group focused on the fundamental challenges with metal hydrides; e.g., kinetics and off-board regeneration).

Weaknesses
- None are obvious.
- In the past, the scope was too broad at times.
- None significant at this late stage.

- Project scope is a bit scattered, but it technically is demonstrating success in each area.

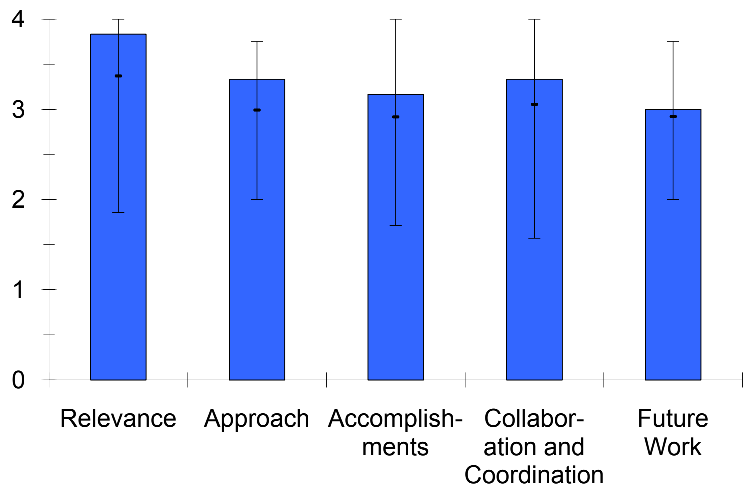
Specific recommendations and additions or deletions to the work scope

- Again, reviewer suggests that added aluminum be tried with higher titanium loading in the lithium aluminum hydride work.
- Good to see super critical carbon dioxide out of the picture. IThis reviewer felt it never had a chance of working from day one.

Project # ST-32: Lightweight Metal Hydrides for Hydrogen Storage
J.-C. Zhao; Ohio State University

Brief Summary of Project

Overall Project Score: 3.3 (6 Reviews Received)



The overall objective of this project is to discover and develop a high capacity (> 6 wt%) lightweight hydride capable of meeting or exceeding the 2015 DOE FreedomCAR and Fuel Partnership targets. Objectives for fiscal year 2009 were to: 1) study magnesium borohydride, especially by synthesizing and studying the stability of magnesium dodecaborate (an anhydrous compound not obtained), and 2) study aluminoborane compounds AlB_4H_{11}, AlB_5H_{12} and $AlB_6H_{13}$for suitability for hydrogen storage. Objectives for fiscal year 2010 are to: 1) study the absorption and desorption kinetics with and without catalysts to improve the reversibility of AlB_4H_{11},and other aluminoborane compounds, and 2) study their structures and kinetic mechanisms.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- With a sharper focus on high capacity reversible material development, this project is now the only major experimental project left in the metal hydride (MH) field.
- This project is well aligned with the Hydrogen Program and DOE research, development & demonstration (RD&D) goals and objectives. The work of this project directly explores materials that have the potential to meet the DOE 2015 hydrogen storage targets. Research is focused in part on selected aluminoboranes that have actually passed a recent go/no-go assessment.
- The project is relevant to the mission of the DOE Hydrogen Program. It addresses lightweight borohydrides.
- As part of the MH Center of Excellence (MHCoE), the project is well aligned with the overall objective of the CoE and Hydrogen Program. It aims to discover and synthesize novel, lightweight materials to meet DOE targets.
- The project aims to achieve some critical DOE targets, in particular gravimetric hydrogen-density and reversibility. Some additional information on the volumetric density of the developed materials would have been useful.
- The project is clearly focused on the rapid discovery of new metal hydrides and definitely supports the DOE RD&D plan.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- There is subtle but important focus on high potential material candidates. It is recommended that this project augment their work with an ongoing theoretical project, specifically ST028 (Wolverton).
- The approach does address technical barriers in an appropriate manner; the project appears to be well designed, feasible, and integrated with other efforts within the MHCoE. Is it possible that some of the unanswered questions (like the structure of AlB_4H_{11} and the "yellow" material) could be resolved by electronic structure type computational methods? Application of methods like energy dispersive spectroscopy or electron microscopy might also add useful characterization information.

- The project's approach is sound with the specific goal on synthesis of new AlB_xH_y complex materials. The project appears to be well structured and focused on a systematic approach to synthesis and characterization of new materials.
- For each new material under consideration, the PI should carry out a quick assessment of the thermodynamic limitations of material regeneration before spending too much effort on hydrogenation.
- Based on some old unappreciated literature, the project has nicely moved into some interesting aluminoboranes. These materials are not being studied anywhere else within the DOE program and offer new opportunities.
- The effort consists of complimentary synthesis, pressure-composition isotherms , and numerous other characterization techniques.
- The work is a bit scattered and haphazard at times, but still seems to move nicely.
- The approach for the project that was given was specific for this year (on aluminoborane compounds), unless some sort of up selection of these compounds was performed. Some level of continued materials discovery of new compounds is encouraged but should be balanced with the focus on particular promising compounds as is done here; i.e., should be balanced.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The research team has indeed made some significant progress toward meeting goals and accomplishing objectives and has produced some evidence of success in overcoming one or more barriers to on-board hydrogen storage. Key examples are listed below:
  - AlB_4H_{11} showed reversibility under mild conditions; endothermic desorption and thermodynamic reversibility also observed.
  - Cycling AlB_4H_{11} didn't change apparent amorphous structure; i.e., no formation of borides that would be hard to reverse.
  - Synthesis of new phases continues; one new phase is undergoing characterization.
- This reviewer would be inclined to give a higher rating when the "unknown" structures have been worked out and the performance characteristics fully evaluated.
- Identified a pathway to synthesis of AlB_4H_{11}. Showed some reversibility.
- Prepared a new aluminoborane with unknown structure.
- Significant findings made in synthesis of the new AlB_4H_{11} and its derivatives.
- The various aluminoboranes are showing good gravimetric capacity, rather low desorption temperatures, and in some cases partial reversibility (AlB_4H_{11}).
- Overall, the results are many and promising from thermodynamic and kinetic perspectives.
- One shortcoming is significant diborane in the diatomic hydrogen released. It needs to be determined with further mechanistic studies, if there are hopes of reducing the desorbed diborane to acceptable levels.
- The recent work on ammonium-substituted borohydrides offers new possibilities. Ammonia contamination of the diatomic hydrogen will offer challenges.
- This reviewer would have liked the progress report to have included some volumetric capacity calculations to help judge if these new materials will likely meet that DOE target. (During the question and answer part of the presentation, the PI answered this question positively. The high volumetric hydrogen densities of these materials should be included in the final report).
- The technical progress is always very strong for this project. It is clear the PI is very focused on hydrogen storage and is capable of rapidly synthesizing and characterizing new metal hydrides. The compounds that are being explored are out-of-the-box concepts and not being pursued in other projects. The progress on AlB_4H_{11} is thorough and promising with a detailed level of understanding.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- It is recommended that this project augment their work with an ongoing theoretical project, specifically ST028 (Wolverton).

- The collaboration/partnering in this project seems for the most part to be appropriate, well organized, and properly coordinated. It is possible that some additional expertise in areas like molecular structure calculations, other types of spectroscopy; e.g., Raman spectroscopy, electron spectroscopy, and/or electron microscopy, might help answer lingering questions about compositions and structures.
- The project is part of the MH CoE and appears to be well connected to other contributors and resources; however, it was not clear from the slides and presentation what the roles of others are, and especially what results came from where, and etc.
- Collaborations are excellent.
- Many good national and international publications and presentations.
- The project is highly collaborative, leveraging external resources for characterization with partners such as the California Institute of Technology (Caltech), the Jet Propulsion Laboratory (JPL), etc., and synthesis knowledge with partners like the Oak Ridge National Laboratory (ORNL).

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The future plans do build on past progress and are generally focused on overcoming barriers to on-board hydrogen storage. However, these plans are stated in a rather general way that has a "more of the same" tone to it. This reviewer would like to see some quantitative performance targets for future results spelled out so that one can better gauge future progress.
- Description of future work is somewhat vague and need to be more specific.
- Future plans sound good.
- There is a need to realize the difficulty of gaining mechanistic understanding of reversibility due to the amorphous nature of these aluminoboranes.
- The approach is good, given the limited time left for this project.
- Work is a logical extension of current progress, however a question is if they intend to stick with aluminoboranes or continue with a more broad materials discovery effort.

Strengths and weaknesses

Strengths
- Strong synthesis background.
- The project is working on a class of materials that does have a chance of meeting DOE RD&D performance targets for on-board hydrogen storage, is staffed by a team of knowledgeable scientists, and high hydrogen content materials are being considered.
- The project has strong synthesis capabilities.
- Good chemistry and materials science knowledge and experience is being used in this project.
- The project uses different materials from most other DOE projects.
- Very strong synthesis and characterization expertise is being used in this project.

Weaknesses
- The project may be missing out on useful input from some informative characterization methods that are not currently part of the project. Consideration needs to be given to the question: what are the most informative ways of elucidating the composition and structure of amorphous materials. It is not clear whether reversibility will be achieved.
- As a result of the nature of this particular class of materials, the research is very fundamental and the probability of finding a successful storage material is, unfortunately, very low.
- No substantial weaknesses.

Specific recommendations and additions or deletions to the work scope

- The project will benefit from the PIs developing a more organic working relationship with a theoretical project within the hydrogen program.

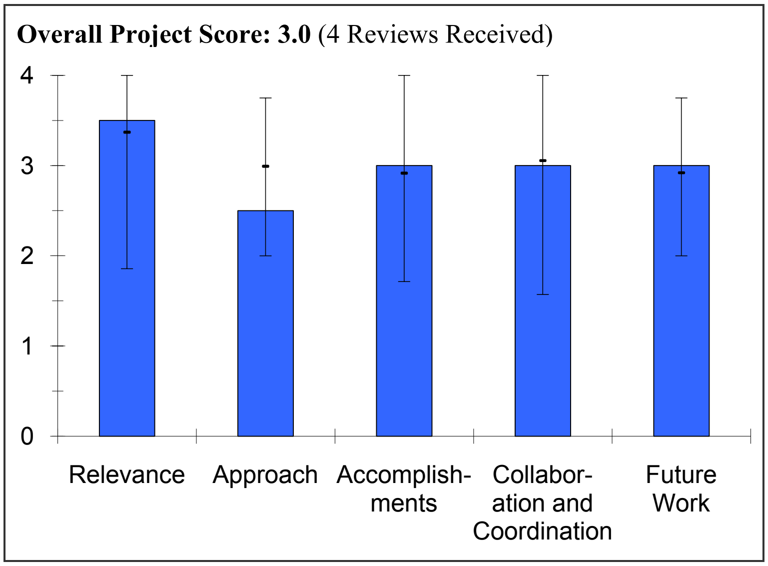
- One of the interesting features of the work going on in this project right now is that they are running into amorphous materials with encouraging performance properties. This poses the question if amorphous state of matter the way to go for high capacity, easily reversible hydrogen storage.
- Highly recommend the project adequately address the following non-trivial issues and potential show-stoppers for each newly synthesized material as early as possible: the degree of reversibility, safety, synthesis reproducibility, material purity, charging/recharging kinetics, synthesis, complexity, yield and cost. This reviewer would include a little calculational work on volume to better show the potential advantages of the aluminoboranes.

Project # ST-38: Hydrogen Storage by Novel CBN Heterocycle Materials

Shih-Yuan Liu; University of Oregon

Brief Summary of Project

Objectives for this project are to: 1) focus on cyclic diatomic hydrogen storage materials containing carbon, boron, and nitrogen; 2) investigate a new approach to diatomic hydrogen storage that complements the materials currently under investigation; 3) couple exothermic diatomic hydrogen desorption from boron-nitrogen with endothermic diatomic hydrogen desorption from coated carbon (CC) in a cyclic system to address reversibility for on-board hydrogen recharge; and 4) foster a strong collaborative effort with feedback loops between theory, synthesis, catalysis, and charge/discharge characteristics measurements.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project supports the Hydrogen Program's goals and objectives. It may generate a number of novel materials for hydrogen storage applications.
- The project is aimed at achieving some key goals: capacity and low spent fuel recharge energy.
- Generally high, although near-term original equipment manufacturers (OEMs) will not likely use off-board chemical hydride systems.

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- The approach is similar to that proposed by other researchers working on hydrogen storage in organic materials. However, in this case, the research objectives are quite different.
- Making lower free energy of reaction (ΔG) materials with reasonable capacity is a good route.
- Theory guide to synthesis is a good idea and a good approach.
- Finding a combined endothermic/exothermic materials system that yields a slightly endothermic net reaction for automotive use is an interesting and valuable approach. However, the materials proposed are overly endothermic to release the diatomic hydrogen off with >100kilajoules per mole (kJ/Mol) and overly exothermic with >100kj/mol for spent fuel regeneration.
- Also, 25+kcal for the enthalpy of reaction (ΔH) is terribly unrealistic – nearly helf the energy stored in the hydrogen is consumed. Also, the material is currently not liquid over enough of the temperature range.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Progress has been outstanding considering the limited funds.
- Very interesting chemistry is used.

- Materials developed may have applications in a variety of areas adjacent to hydrogen storage, for example, non-conventional fuel cells.
- Overall, it is a very good project; however, the progress made is a bit slow for 1.5 years at $400,000 per year.
- The project did find a reversal path, but the yield may not suffice.
- The synthesis path is not good enough, and they know this.
- A higher percentage of hydrogen material with a methyl group was made, but there was no data on how it works.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Tom Autrey and Dave Dixon are the appropriate partners for this effort; the collaboration with D. Dixon at Alabama appears to be of value.
- This project would benefit if it could continue to be part of a materials CoE in the future. This is difficult chemistry—it seems unlikely that one PI can take on all of the tasks necessary to complete this project.
- Current collaboration is reasonably good. However, I would recommend involving an industrial partner.
- Seems like collaboration with the CoE was helpful.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Liquefying the materials at subambient temperatures will be critical in making this a useful material for automotive purposes. The liquefying additives must not reduce the material capacity significantly since the starting material is only in the ~7% range.
- My impression is that the PI intends to do direct rehydrogenation on board. Both the hydration and rehydration steps are in the 100-150 kj/mol area—is direct hydrogenation really that feasible? This seems more likely to be an off-board material unless the PI can prove that the exothermic reaction worked.
- A basic research component, a new materials discovery effort, could be added to the task list.
- Plans need to be made.

Strengths and weaknesses

Strengths
- It is an interdisciplinary project that combines elements of materials science and organic chemistry. It has good chances for success.
- The project exemplifies and understands the need for liquid form.
- The project will be aimed at higher capacity materials in time.

Weaknesses
- An industrial partner could make valuable contributions to the project by evaluating scalability of the developed materials.
- Energy is unrealistic.
- Not clear if there have been any studies of hydrogen uptake or release for any of these materials. Data of this type do not appear in the presentation slides.

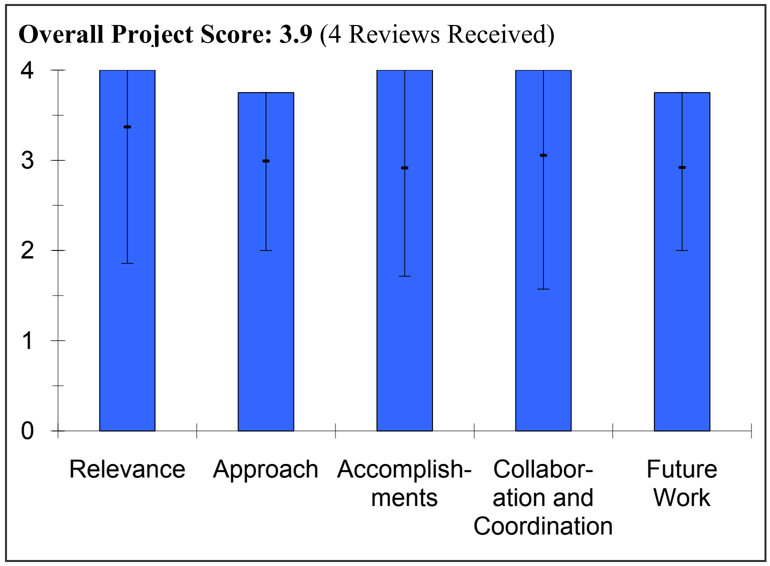
Specific recommendations and additions or deletions to the work scope

- Move to understanding kinetics of release of hydrogen.
- Need to understand if parent can be decomposed without polymerizing to know what the real mass percent is possible.
- Above all, work on a compound that has a ΔH in the 7-12 kcal range.

Project # ST-40: Chemical Hydrogen Storage R&D at Los Alamos National Laboratory

Anthony Burrell; Los Alamo National Laboratory

Brief Summary of Project



Objectives for this project are to: 1) complete demonstration of the spent fuel regeneration process and provide data for preliminary cost analysis of the new Los Alamos National Laboratory (LANL) spent fuel regeneration process; 2) develop liquid ammonia borane (AB) fuels and increase the rate and extent of hydrogen release; 3) develop and demonstrate heterogeneous catalysts and continuous flow reactor operation; 4) identify and demonstrate new materials and strategies for near-thermoneutralhydrogen release (with free energy of reaction (ΔG°) = ideally no less negative than approximately -0.8 kcal/mol); and 5) develop materials and processes to minimize gas-phase impurities and demonstrate adequate purity of the hydrogen stream.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The project aligns with diatomic hydrogen storage objectives. Materials studied have the potential to meet the volumetric and gravimetric targets. The project is focusing on relevant issues, such as impurities generated during hydrogen release and the regeneration of spent AB.
- This project focuses on the efficient release of hydrogen from AB and related compounds and the development of improved strategies for regenerating spent fuel materials. AB and analogs, especially in liquid form, are promising candidates for meeting DOE objectives for a non-reversible, chemical hydrogen storage system. The LANL effort for the optimization of hydrogen release and efficient off-board regeneration of spent AB and related materials is highly relevant and fully supports the DOE objectives.
- The project has definite potential for high levels of hydrogen storage in liquid form.
- Unusually complete consideration of almost all DOE storage objectives: weight, volume, regeneration cost, gaseous impurities, hydrogen release rates, thermal management, etc.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- They have been focused on the technical barriers. They have discontinued work in areas such as alkyl ABs to focus on more productive areas. Focus could have been a little more on a cost effective spent fuel regeneration scheme, which is still the main hurdle to overcome.
- A clearly stated and well-formulated approach has been adopted to address the important issues of complete hydrogen release at reduced temperatures and efficient off-board regeneration of spent AB.
- A comprehensive materials development and reaction characterization approach with LANL collaborators is being successfully employed to improve AB hydrogen release properties through addition of ionic liquids and catalysts, and to identify other related compounds; e.g., alkyl-AB mixtures, having potentially improved performance.
- By thoroughly addressing the issues of efficient “one-pot spent fuel regeneration,” fast hydrogen release, and impurity inhibition, LANL has made excellent progress in making liquid AB and related compounds serious

contenders for practical on-board hydrogen storage. Likewise, through their collaboration with The Dow Chemical Company (DOW), a cost analysis is underway to identify all cost-critical spent fuel regeneration pathways and to explore the best ways to reduce system cost.
- Excellent approach for both hydrogen generation and spent fuel regeneration processes.
- The effort was very broad and considered many aspects of AB storage, usage, and spent fuel regeneration; each aspect was studied in substantial detail.
- Excellent combination of AB chemistry and practical experiments. Good engineering orientation.
- Several built-in go/no-go decision points and detailed decision procedures were included in the project approach, which was appropriate.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **4.0** based on accomplishments.

- Excellent work on improving hydrogen release rates using catalysts and ionic liquid additives (collaboration with the University of Pennsylvania (UPenn) and other Chemical Hydrogen Storage Center of Excellence (CHSCoE) partners). Although optimization is still needed, demonstration of an efficient "one-pot" off-board spent fuel regeneration approach is a breakthrough result. This approach is considerably less complex than previous regeneration schemes, and it seems to offer a good pathway for achieving high efficiency regeneration in a high capacity processing environment. Reduction of hydrazine costs seems to be the most serious challenge in the implementation of the LANL regeneration process. However, excellent work is underway to explore cost-reduction options and the development of alternative reactants.
- Good work on cost analysis is being performed by LANL and CHSCoE partners (principally DOW). This is an important component of the project; it is essential to providing a "reality check" at this advanced stage of the project.
- Confirmed the expected copious amounts of impurities (diborane, borazine, and ammonia), but developed a carbon-based adsorbent purification method that will clearly produce pure enough diatomic hydrogen for a proton exchange membrane fuel cell (PEMFC).
- Overall, this project is an exemplary combination of science and engineering that is effectively addressing important issues in the development and deployment of a practical chemical hydrogen storage system.
- The project has demonstrated that the regeneration of spent AB is possible. LANL has devised several regeneration schemes and significantly simplified the regeneration scheme with use of hydrazine and ammonia. This scheme was demonstrated with several different spent fuel forms, which suggests that the technique will work independent of the scheme used to reduce impurities and foaming.
- The metal AB systems and ionic liquids are both very promising.
- The one-pot spent fuel regeneration system is a major improvement that significantly simplifies the AB regeneration.
- Cost of hydrazine for the one-pot regeneration process is an issue for the overall fuel cost.
- A staggering amount of data! Highly productive effort for the level of funding.
- Clearly showed that some forms of AB could be engineered into a vehicular storage system.
- Identified and demonstrated spent fuel regeneration processes, especially the LANL hydrazine process. Showed price of hydrazine has to be lowered before cost target can be met.
- Many other nice details.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **4.0** for technology transfer and collaboration.

- Collaboration within this CoE has been outstanding.
- Excellent cooperation between LANL and CoE partners (especially with UPenn on AB-Il systems and DOW on cost analysis). In addition, LANL and CHSCoE partners have in engaged in fruitful interactions with other Centers (especially the Metal Hydride (MH) CoE and the Engineering CoE). LANL is providing superb leadership and coordination of materials development and off-board spent fuel regeneration work.
- Excellent collaborations.

- One of the best examples of good collaboration within any of the DOE activities.
- Much of the collaborative success comes from the efficient and open operation of the CHSCoE.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.8** for proposed future work.

- Project is ending.
- Good plans are in place to conclude the project and transfer technology to the Engineering CoE. Unfortunately, with the conclusion of the CoE, it is unlikely that the concepts studied thus far will be developed to the extent needed for complete and seamless technology transfer. However, it is hoped that work can continue at even a reduced level on optimization and validation of AB-Il-catalyst systems and on complete, end-to-end demonstration of the one-pot regeneration cycle at reduced cost.
- This project is 100% completed, but the final results are very promising and should be continued if at all possible.
- This evaluation is not fully applicable because the project is imminently ending. However, there are some loose ends that remain.
- PI has convincingly argued to continue long enough to complete the details. This reviewer would agree.

Strengths and weaknesses

Strengths
- Good collaboration between experiment, modelling, and engineering analysis.
- Highly quailified research, development, and engineering team has been assembled at LANL and at collaborating CoE institutions. Focused effort on improvements in hydrogen release in AB, AB derivatives, and related materials is showing considerable promise. Likewise, using well-coordinated efforts to improve spent fuel regeneration processes will complete the overall engineering cycle and bring this technology closer to practicality.
- Extremely strong chemistry approach and team.
- Excellent fundamental science and engineering.
- Very close to meeting DOE targets and practical application.

Weaknesses
- Project is ending.
- None.

Specific recommendations and additions or deletions to the work scope

- Strongly recommend that DOE find a way to provide continued support for this activity.
- Project should be continued.
- Try to continue (renew) in some limited form.

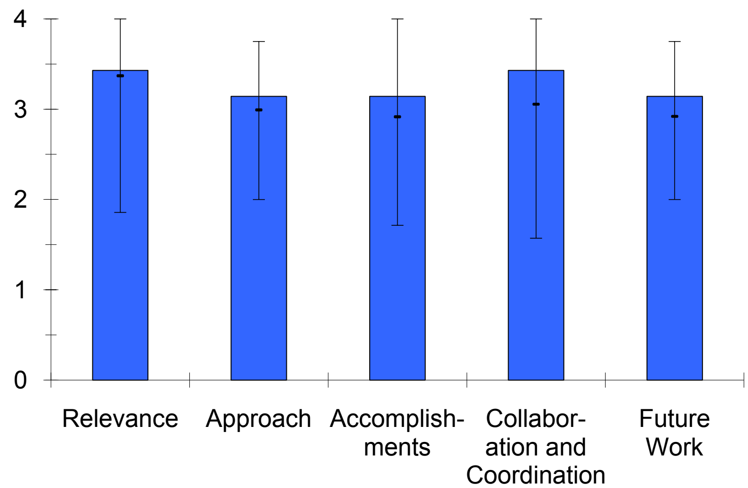
Project # ST-41: PNNL Progress as Part of the Chemical Hydrogen Storage Center of Excellence

Jamie Holladay; Pacific Northwest National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) develop materials and methods for low temperature release of pure hydrogen from chemical hydrides with potential to achieve DOE targets, 2) demonstrate high efficiency methods for large scale synthesis of chemical hydrogen storage materials, 3) develop high efficiency off-board methods for chemical hydride spent fuel regeneration with potential to achieve DOE targets, and 4) support collaborators through expertise in chemistry and characterization to determine the kinetics and thermodynamics of hydrogen release and regeneration of spent hydrogen-storage materials.

Overall Project Score: 3.2 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This broadly based project focuses on critical problems in efficient hydrogen release and off-board regeneration of spent chemical hydrogen storage materials. There is excellent synergy among research, development, and engineering efforts at PNNL and collaborators in the Chemical Hydrogen Storage Center of Excellence (CHSCoE).
- The focused efforts on theory, assessment of material characteristics (mainly ammonia borane (AB) and related compounds) and development of novel, yet straightforward engineering strategies and approaches directly address DOE research, development & demonstration (RD&D) barriers as well as performance and cost targets.
- The project is part of the CHSCoE and is well aligned with the objectives of the CoE, namely to develop materials and processes for low temperature release of hydrogen that can meet DOE targets.
- Specifically, the project appears to aim for large scale synthesis of AB and the regeneration of the thermodynamically difficult spent fuel from AB storage material.
- Aimed at higher capacity and efficient recycling with low impurities.
- Mostly aligns with DOE goals and targets. The Pacific Northwest National Laboratory (PNNL) is studying some materials that do not have the potential to meet storage goals—new metal amido borane does not meet the 6 wt% goal.
- Materials are not ready for commercialization, but they do show interesting properties.
- Solid AB has a very high hydrogen storage capacity.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- The project is keenly focused on the technical barriers associated with successful deployment of an AB-based storage system. A methodical and systematic approach has been employed to address vital research and engineering issues, including: 1) elucidation of mechanisms of hydrogen release from AB, 2) development of practical methods to achieve rapid and efficient hydrogen release, 3) reduction of impurities that deleteriously affect system performance, and 4) demonstration of innovative methods for first-fill and off-board regeneration of spent fuel.

- The approach on solid AB and derivatives is complementary to the LANL effort on liquid AB systems. The approaches adopted in these two large R&D activities plus directed efforts in CHSCoE partner institutions are well coordinated and comprehensively address the important barriers associated with this promising system from both on-board and off-board perspectives.
- The project incorporated a theory/calculation-guided experiment approach, deep exploration of the details of reaction, and process reaction engineering, which was helpful.
- Metal amidoborane (MAB) spent fuel regeneration seems unlikely, but may be possible.
- Work is appropriately focused on the technical barrieres of reducing impurities and on spent fuel regeneration.
- There is a good mixture of theory and experiment.
- The CoE eliminated non-competitive materials.
- Solid AB has issues with borazine and ammonia impurity generation along with the hydrogen.
- Solids handling may be problemmatic for vehicular applications.
- The approach of the scale up synthesis of the material appears sound.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- There has been approximately $2 million spent for the following results:
  - Worked down cost of first fill AB production from basic materials to $9/ kg. In part, by redesigning to a flow process.
  - Showed metal changes the reaction path of AB greatly, offering a new route with possible avoidance of impurity streams such as the borohydride, B6H6. However, it does make ammonia at 10 times.
  - Undisclosed endothermic MAB (2 kilocalories (kcal) and 85°C release).
  - Complete boron recovery in plain AB spent fuel regeneration scheme.
  - Progress was made but not completed on low cost route for spent fuel regeneration.
- The project has shown progress in reducing impurities through catalysis, but borazine release was still high from AB; the project has also demonstrated high boron recovery during AB spent fuel digestion.
- Important new results were produced in 2010 on: 1) impurity identification and mitgation in AB dehydrogenation reactions, 2) characterization of AB-metal hydride mixtures which show decreased impurity levels and less foaming than AB alone, and 3) demonstration of a new ammonia synthesis route for more efficient AB first-fill and AB spent fuel regeneration. Also, intriguing new results on characteristics and mechanism(s) of hydrogen release and impurity from metal amidoborones were produced.
- Borazine and ammonia release from AB and metal-amidoboranes remains a problematic issue. However, good results were obtained in 2010 on mitigation strategies involving additives/catalysts to facilitate reduction of impurity concentrations.
- In the metal-AB (M-AB) work, the efficient regeneration of M-AB from spent fuel by simply separating the metal in a conventional AB spent fuel regeneration cycle remains a serious outstanding challenge. At this stage of the project, solid plans should be in place to facilitate metal-AB spent fuel regeneration; e.g., can a variant of the hydrazine-mediated approach adopted by LANL be used?
- One overarching issue is that solid systems are generally not as desirable as liquids for fuel transport and handling in the reaction system. A more detailed examination of approaches for efficient use of solid fuels is needed.
- The team has done a good job addressing issues affecting AB systems; however, many problems remain.
- Significant progress has been made in fiscal year 2010.
- New endothermic proprietary MAB material is a significant development, albeit the hydrogen capacity is below 6 wt%.
- New steps were identified to lower the cost of solid AB.
- The moderately high volume synthesis of the M-AB storage material (even at the 20-gram level) is significant.
- Given the inherent huge spent fuel regeneration challenge of these chemical hydrides, the project results are significant.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- There was a good number of very useful collaborations.
- There are good collaborations, both within the CoE and with outside institutions.
- The PNNL technical efforts are enhanced by extensive and fruitful collaborations with CoE partners and external institutions. There is a good division of effort and the R&D effort is well managed and coordinated.
- Good work was accomplished with CoE partners.
- The CoE "Lessons Learned" slide indicates that trust between some of the CoE partners (unidentified) was a problem.
- The project appears to have plenty of collaborative opportunities; however, I assume that managing the high number (15) of institutions may have been a challenge and may not have been as effective as having less partners would have been.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- This project is 95% complete—the future plans are not commensurate with that timeline; however, they do suggest remaining work that would be needed if CoE activities would continue.
- Time seems insufficient, but they have a good plan.
- Although the work on hydrogen release from M-AB compounds is interesting and potentially important, very little attention is given to the important issue of M-AB spent fuel regeneration.
- Approaches and concepts for efficient distribution and handling of solid fuels are needed.
- Not applicable. The CoE closed.
- The project is ending, this project is essentially over.

Strengths and weaknesses

Strengths
- Highly quailified research, development, and engineering team has been assembled at PNNL and at collaborating CoE institutions. Focused effort on improvements in hydrogen release from AB, AB derivatives, and related materials is showing considerable promise. Likewise, well-coordinated efforts for improving spent fuel regeneration processes are completing the overall engineering cycle and are bringing this technology closer to practicality.
- Includes mix of theory and experiment.
- High hydrogen capacity and simplicity of solid AB.
- The demonstration of high volume synthesis of AB is significant.
- The down selection of materials shown in the summary table for diatomic hydrogen is useful.
- PNNL continues to set the bar very high. Overall, they have done an excellent job with the Chemical Hydrogen Storage CoE.

Weaknesses
- Insufficient emphasis on M-AB spent fuel regeneration and concepts for distribution and handling of solid fuels.
- Impurity and rate issues with hydrogen release.
- The difficult nature of the chemical hydride materials.

Specific recommendations and additions or deletions to the work scope

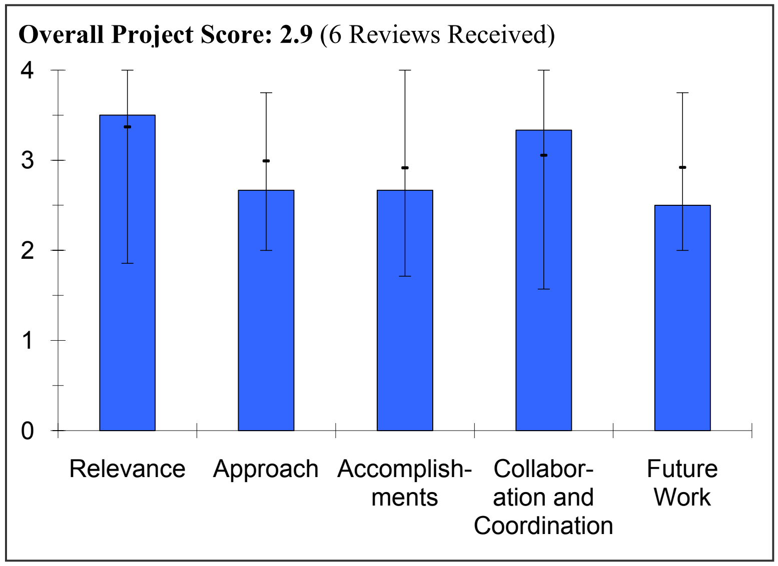
- Some formalized method to hand things off to the Engineering CoE would be beneficial—overlap or transfer of some staff would be the most efficient way.

- The researchers should take a closer look at efficient M-AB spent fuel regeneration schemes and effective methods for solid fuel handling and distribution.
- Reviewer recommends that DOE find a way to provide continued support for this activity.
- The project team should address the presence of contaminants such as ammonia and borazine quickly, as it can be a show stopper.
- The project team should spend more effort on resolving the spent fuel regeneration issue and not on scale up synthesis.

Project # ST-44: SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Metal Hydride and Adsorbent Systems
Ted Motyka; Savannah River National Laboratory

Brief Summary of Project

Objectives for this project are to: 1) collect and assimilate property data for metal hydrides and adsorbents, 2) collect operational data for storage vessel configurations, 3) develop general format for models, 4) assemble and test models, and 5) develop an "acceptability envelope" of media characteristics based on 2010 and 2015 DOE technical targets.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Generally high—a systems-level understanding is required to understand how to build an automotive grade tank.
- This project meets the target of DOE.
- This project is consistent with the objectives of the Hydrogen Program and fully supports DOE research, development & demonstration (RD&D) objectives.
- All project aspects align with the Hydrogen Program and DOE RD&D objectives. However, in order to give an "outstanding" rating, I would have to assume that parallel considerations for chemical hydrides are covered in another part of the Hydrogen Storage Engineering Center of Excellence (Engineering CoE) (which it turns out they are).
- Project approach is designed to achieve the ultimate goal of the Engineering CoE.
- This is an exercise in futility since no viable metal hydride or otherwise hydrogen storage material capable of meeting the DOE diatomic hydrogen storage material targets for on-board vehicular use is available. It is, therefore, unclear why this activity should continue. DOE dollars would have been better spent on funding smaller projects that research new hydrogen storage material concepts.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- Good to introduce novel concepts—what about integrated concepts like the aluminum mesh being used by Van Hassel's group (project ST-006)? Such concepts serve as both heat transfer mediums and material stabilizers.
- The size of the tank is unclear in the model, for instance is it only for 100g or a larger 5- to 6-kg tank, or is it 1 kg as some slides indicate. How will they predict the performance of a full scale system was not clearly explained nor how they validate the model if the CoE doesn't construct a bigger system.
- The model of thermal management seems to be investigated. However, real hydrogen storage tanks using hydrogen storage materials have various issues including expansion and contraction of materials with hydrogenation and dehydrogenation, swelling of materials, the amount of powder in the vessel, and so on. This project excludes these significantly important issues from modeling.
- Selection of near- and mid-term metal hydride (MH) candidates for engineering development is sound.
- Compiling metal and adsorption hydride date is critical to modeling efforts.
- Developing an "acceptability envelope" of hydride properties provides a great pathway for the downselection of materials.

- The approach is generally good. I like the "acceptability envelope" aspect of this project, but this type of overall performance analysis should be applied uniformly to all of the storage concepts under consideration by the Engineering CoE, i.e., will the Pacific Northwest National Laboratory (PNNL) and the Los Alamos National Laboratory (LANL) be using the same approach for chemical hydrides.
- There is no detailed schedule given that would enable one to gauge at what point in the program plan model validation would start.
- The PI's approach lacks the core premise of having a viable hydrogen storage material available. Without that, the sophisticated engineering analysis, model building, CFD work, etc., is unlikely to produce a viable system-level hydrogen storage tank to DOE specifications. Why is it a good idea to model an activated carbon (AX-21) or metal-organic framework- (MOF-5) when if they are not going to deliver at system level if they fail at the material level.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Good.
- At present, only methodologies were shown.
- Resources devoted to sodium aluminum hydride may be wasted since sodium aluminum hydride has no chance of becoming a viable diatomic hydrogen carrier due to very low system capacities that will not meet DOE targets.
- Development of the kinetics, heat, and mass transfer models are on target.
- If the numbers were understood correctly, well over a million dollars has been expended on this work since the start of the project. The accomplishments from this work are mainly: 1) the gathering, evaluation, interpretation, and assimilation of data into models; 2) the development and application of the "acceptability envelope" to metal hydrides; 3) the development of a model for adsorbents; 4) the validation and testing of the metal hydride models; and 5) the development of optimization studies of the vessel configuration for sodium aluminum hydride. Much of what was presented was called "baseline" analysis. Hopefully there will be much more to show for this kind of funding expenditure in future years, including some concrete technical findings that push prototype system development work in the right direction.
- Progress on technical accomplishments appears to be on track; however, no detailed schedule was provided. The "refined" model for a physisorption storage system needs further improvement when compared with the measured thermal profiles.
- Having stored (or aiming to store) 1 kg of hydrogen in 12 minutes is not really an accomplishment. Progress has been minimal. PIs have too much on their plate.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Good.
- The project contains some distinguished experts of the field, but others are missing that should be included.
- Excellent collaborations among the many CoE partners.
- Appropriate, well integrated, and seemingly effective collaborations with other institutions; partners appear to be full participants and are well coordinated within the various tasks.
- Very strong team established.
- There are many entities involved–perhaps too many. It is not made clear how closely these groups work together. No example of close collaboration that resulted in a tangible improvement was given.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- This effort is part of the CoE and includes working with original equipment manufacturers (OEMs).

- The issues that have been mentioned in the "technical accomplishments" section should be investigated. For that purpose, construction and testing of prototype tanks should start very soon.
- Proposed future work is very good.
- The future plans read like more of the same in terms of approach and emphasis. This reviewer doesn't have a warm, fuzzy feeling about bang for the buck where this project is concerned. The participants in this project should work toward a strong showing in 2011 by deeply engaging the issues to produce concrete go/no-go recommendations for future technical developments—baselining isn't going to be enough.
- Need to establish a clearly defined test matrix of candidate storage materials.
- Building hardware and software models around MOF-5 and MaxSorb as the hydrogen storage materials appears to be an academic exercise. It is unclear how this activity can result in an acceptable system-level hydrogen storage tank because they are starting from a storage compounds that do not meet DOE's programmatic goals for on-board diatomic hydrogen storage.

Strengths and weaknesses

Strengths
- Modeling of thermal properties with candidate materials are under way with collaboration of excellent experts.
- There is strong collaboration.
- The partners have excellent understanding of the technical work.
- The project is well organized.
- The project has a well defined architecture in the modeling hierarchy toward addressing the Engineering CoE goals.
- There is a very strong team contributing to the project.
- Successful completion of model development will establish the theoretical boundaries of expected performance from any given storage system design and storage media.
- Researchers and scientists involved in this Engineering CoE are capable individuals with resources available to them.

Weaknesses
- Only thermal properties, such as thermal conductivity, is taken into the consideration. However, there are a numbers of issues to be considered to design tanks utilizing hydrogen storage materials.
- None.
- The project is looking only to "preliminary prototype designs" by the end of Phase I. I would like to see an emphasis on results that are more substantive than just "preliminary".
- Project activities have a high potential for replication of effort, past and present.
- While no detailed schedule of project tasks was given, the success of the project is likely to be constrained by time unless media property data become available very early in the project schedule.
- Software models and hardware designs are based on a yet-to-be-discovered hydrogen storage material with unknown characteristics and thermophysical properties. The lack of cost considerations is a real issue.

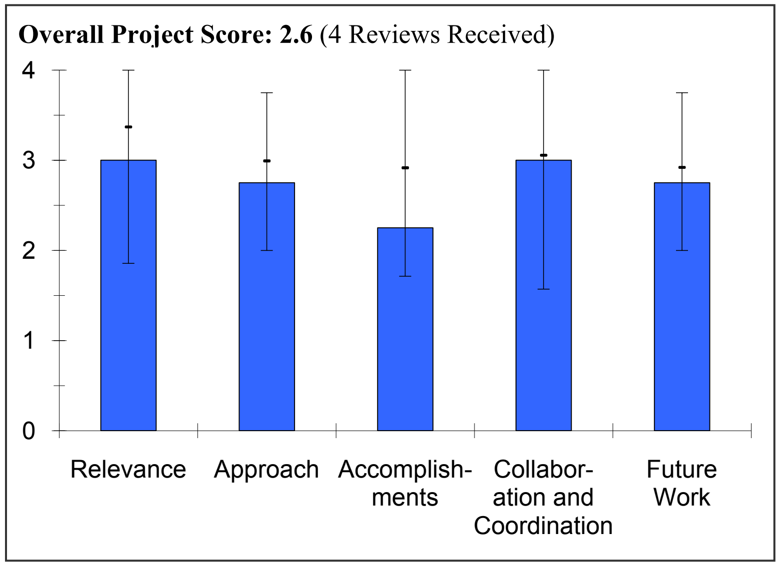
Specific recommendations and additions or deletions to the work scope

- Every factor that influences the tank performance should be investigated. To that purpose, testing with the real tanks is indispensable. The size of a 100-g tank seems to be too small.
- Step up the pace of work—raise the ambition level.
- Prepare a detailed schedule of tasks and milestones.
- Reduce and revise the scope of work.

Project # ST-45: Key Technologies, Thermal Management, and Prototype Testing for Advanced Solid-State Hydrogen Storage Systems

Joseph Reiter; NASA Jet Propulsion Laboratory

Brief Summary of Project



Objectives for this project are to: 1) provide management tasks in support of the Hydrogen Storage Engineering Center of Excellence (Engineering CoE), 2) serve as technology team lead for thermal insulation research and development, 3) perform insulation material testing and validation, and 4) perform metal hydride prototype testing and evaluation. The purpose and focus of the Jet Propulsion Laboratory (JPL) effort is technology management, including: 1) assessment of current state-of-the-art and fitness evaluations of existing technologies; 2) identification of technology gaps in regards to system requirements and operational demands; 3) assessment of impact of technology gaps on system developability; 4) selection of candidate approaches to device design and implementation for gap mitigation; 5) technology development, hardware design and analysis for selected technologies; and 6) continuing assessment and feedback of emerging technologies.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project meets the goal of the Hydrogen Project.
- This project is an integral part of the Engineering CoE and is only 20% complete (one year). The tasks to be performed by JPL are relevant to the goals and objectives of the Engieering CoE and, as such, are relevant to the DOE Hydrogen Storage Program. The primary technical contribution will be in the areas of high and low temperature insulation. JPL will also provide "technology management" for CoE activities.
- Heat transfer is a critical parameter for many hydrogen storage systems and with other components of hydrogen fuel cell systems. Having accurate, reliable, and VERY current data on engineering materials should be helpful to the researchers and designers of these systems.
- This project has some specific roles within the Engineering CoE and thus has implicit connection to the DOE goals and objectives.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- There are three tasks in the project, but they are a little bit dispersed.
- JPL will use its extensive experience and background in thermal insulation to contribute to the Engineering CoE objective. The proposed approach building on this capability appears to reasonable and adequate.
- The combination of literature and Internet surveys and future material properties testing should help fill any gaps in the open literature, if there are any.
- Given JPL’s long history in practical metal hydride technology devices, its initial activity seems rather limited compared to the overall budget allocated.
- This effort is mainly a management role in the area of “enabling technologies.” It manages the cryoadsorbant engineering effort.

- JPL's only technical role, at least in Phase 1, is in the area of "thermal insulation," applicable mainly to cryocontainment.
- Later in Phases 2 and 3, JPL will be involved, more appropriately, in prototype design and construction.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- Task 1 has been completed very quickly.
- Technical accomplishments after the first year (20% complete) of this project appears to be rather limited, involving literature surveys and data base development. Given their experience in similar thermal insulation systems (for space applications), it could be assumed that this data would be readily available at JPL.
- So far, the database is mostly a literature study.
- Accomplishments, so far, have been very limited. Spending has been rather low for the first year. It is not clear if this is a result of DOE interim spending allocations, or the effort is behind schedule.
- The main management effort seems to be not underway yet.
- JPL has appropriately gathered commercial insulation information and provided a useful database for the Engineering CoE (delivered this month).
- Thermal performance is not quite started, but the equipment is nearly in place.
- In spite of the slow start, to quote the PI, "fiscal year (FY) 2010/2011 will be an extremely busy period!" (slide 19).

Question 4: Technology transfer/collaborations with industry, universities, and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The California Institute of Technology (Caltech) seems to be a major player in this project, and other people strongly support them.
- JPL appears to be collaborating with the other Engineering CoE partners, especially in the system architecture development area.
- Ideally, the matrix nature of the organization is very effective, but there can be breakdowns with slight variations of the normal, especially in communication or availability of resources.
- The Engineering CoE provides nice collaborations. It is not clear from the presentation how well they are working at this early point.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- FY 10/11 will be very busy. Plan and schedule should be rearranged to average the duties of the project.
- Future work plans seem to be consistent with overall Engineering CoE goals and objectives and with CoE schedule for system design and development.
- Continue as planned within the directives of the Engineering CoE.
- One might question the rather large effort on thermal insulation, being that it is applicable mainly to cryostorage. Is this not being covered partly in the long-standing Lawrence Livermore National Laboratory (LLNL) cryocompression project (ST-03)?
- This effort is apparently what the Engineering CoE needs.

Strengths and weaknesses

Strengths
- Caltech has experience and expertise in testing.
- Extensive experience in thermal management subsystems.
- Experience in technology management and system architecture definition.

- Potentially developing a large data base of thermal properties of relevant materials, combining literature studies with physical measurements.
- Past JPL experience on hydrogen-storage prototype systems.

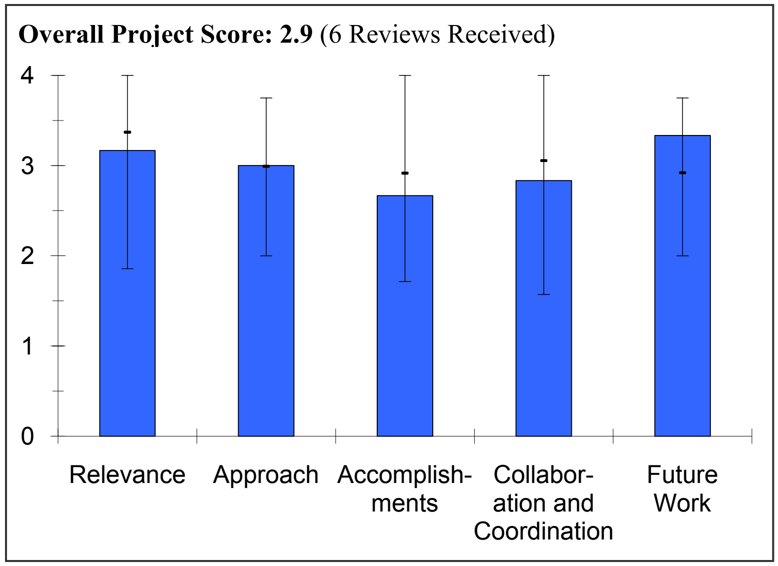
Weaknesses

- Task 1 and 2 are separate subjects to Task 3.
- None apparent at present.
- It may be difficult to make the materials database more attractive to a researcher/developer than their own literature and Internet search study. The team should work hard to make the database very user friendly, practical, and complete, or this part of the project expense may be negated by hydrogen researchers using the Internet more than this product. Most likely the key element of the database will be the most current materials, which means that updates are critical.
- There are questionable roles in Engineering CoE Phase 1. There is no question about the need for JPL in prototype Phases 2 and 3.

Specific recommendations and additions or deletions to the work scope

- Good preparation for Task 3 is critical because Task 3 supports the entire project. The success of Engineering CoE depends on the activity of this task.
- JPL should be encouraged to move to the design and testing approaches addressing the specific design requirements for thermal management of the specific systems identified by the Engineering CoE. General testing and "re-validation" of insulations identified in the literature survey should be limited to insulations that can meet the requirements specific storage system designs.
- Since the insulation materials will be used in vehicles, it will be valuable to include the toxicity, respirability, reactivity, and other hazardous characteristics to the database.
- Establish a working relationship with LLNL to make use of their vacuum insulation expertise and experimental cryocontainment activities.

Project # ST-46: Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage
Kevin Drost; Oregon State University



Brief Summary of Project

The objective for this project is to use microchannel technology to: 1) reduce the size and weight of storage, 2) improve the charging and discharging rates of storage, and 3) reduce size and weight and increase performance of thermal balance of plant components.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The project addresses the problems of system weight and volume, charging and discharging rates, and thermal balance by applying well-established microchannel technology. In this engineering approach, the use of microchannels addresses the enhancement of heat and mass transfers in storage systems.
- This work is relevant to the Hydrogen Program and DOE research, development & demonstration (RD&D) objectives.
- Microchannel technology has the potential to significantly reduce system weight and volume.
- The project "Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage" is relevant to Hydrogen Program and supports DOE RD&D objectives. This microchanneling technology may reduce size, weight, and charging time for hydrogen storage.
- Microchannels are useful and can reduce size and weight in certain applications. Microchannels have been oversold in other applications. It is not clear if microchannel geometry will help in these applications where it would be difficult to place the microchannels in the material of interest, and coating the material of interest on a microchannel surface (heat exchanger) could increase the weight.
- The project does properly support DOE goals by attempting to decrease storage weight and volume and increase tank discharge rates by improving heat exchange and internal flow.
- The PI's description of the rationale and relevance of this activity to the ultimate objectives of the Engineering CoE is clear, succinct, and well established. The only weakness is the lack of attention to cost factor; i.e. how expensive these systems are and how cost consideration would affect the choice and features of the Microtechnology-based Energy and Chemical Systems (MECS) design, etc.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach of the project is to develop models, design and predictively evaluate components, fabricate proof-of-principle test articles, conduct proof-of-principle tests, and use the results to validate the predictive models.
- The approach is well planned in three phases. The project is currently in Phase 1.
- The approach to use microchannel technology to reduce barriers to heat and mass transfer is good. They are focused on optimizing the performance of a single unit cell; i.e., an individual microchannel, and then "number up."
- The approach taken here "is microchannels are the solution, now look for a problem in the diatomic hydrogen storage Engineering CoE to apply them to." This is generally not as effective an approach as defining the problem and then looking for an appropriate solution.
- Go/no-go decisions should be based on system requirements.

- It is not clear how the microchannel insert will improve heat transport or diatomic hydrogen transport in the diatomic hydrogen storage tank. For heat transfer, the limiting rate will be the heat transfer between storage material particles. The microchannel does not appear to address this. The poster states that "metal hydride powder will be patterned with micro-channels to investigate effect on diatomic hydrogen gas distribution throughout reaction volume." Decrepitation will degrade the microchannels if they are formed in the powder or pressed storage material. Decrepitation will also make it difficult to control the contact between microchannel plates and the storage materials, which will affect heat transfer.
- The microchannel combuster/heat exchanger seems more promising.
- This project is under the Engineering CoE and will hopefully provide novel heat exchanger designs.
- The approach is to use small channel heat exchangers to improve heat and mass transfer, which is logical.
- The contractor will also attempt to design and build a microchannel combustion unit to preheat oil for the hydride desorption process. This is especially useful for high temperature (high enthalpy of reaction (ΔH)) hydrides where supplementary high grade heat input will be required.
- Intuitively, cost may be a challenge. Manufacturing cost estimates will come later in the project.
- The PI's approach is reasonable, but it does not address the central issue of not having a viable candidate material for hydrogen storage. Also, the approach to dealing with cost issues has not been fully addressed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- A tank insert integrated diatomic hydrogen distribution and heat exchanger plate was constructed. Calculations of multiphase Navier-Stokes equations for mass and energy transport between gas and hydride phases were performed. Preliminary modeling for combustor with surface reactions is ongoing, with different adsorption, surface, and desorption reactions identified. A lightweight combustion heating system for desorption of hydrogen from metal hydrides was evaluated and found feasible.
- This is a new project. Technical accomplishments are limited by the fact that it is still in an early stage of development.
- The progress was dampened by funding problem but completed the identification of the highest value applications of microchannel-based technology, including completion of design and fabrication of tank insert unit cell test apparatus.
- Reviewer is not sure how the tank insert concept was determined to be the top priority since no modeling was presented to show the potential of microchannels in this concept, such as can it improve things by an order of magnitude, a factor of two, or by 10%. If this work started in February 2009, not much has been accomplished over this time.
- The model validation for pressure drop appears to be a one-point validation—this is inadequate. The project should validate over several flow rates to get the shape of the curve and ensure proper flow rate dependence.
- Hydrogen combustor/heat exchanger work seems more useful since they have provided a quantified weight benefit.
- The project has had a slow start (8% spending in the first 15 month). It is unclear if this a DOE funding delay, or the fault of the contractor. Nonetheless, a number of technical accomplishments are cited. Most seem to be on paper.
- Slide 10 (Technical Accomplishments) states reduced size and weight, and improved rate for both the tank and combustor, however it is unclear if these results are experimental or calculations. In either case, the results are not given or obvious if they are new results or reiterations of the proposal hopes Or if they are the calculation results of the General Motors (GM) study (slide 22).
- The reactor test cell (slide 13) is interesting and innovative.
- The modeling work seems to be going well.
- It is unclear from the accomplishment slide 21 if the experimental facility is in place, or just designed.
- The PI has carried out a considerable amount of good work that, in general, is of value to many applications including hydrogen sorption devices. The work on the MECS-based tank insert, MECS-based integrated combustor/heat exchanger, and the lightweight microchannel combustion system is invaluable and holds great promise.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- There is an extensive list of partners and collaborators, which includes national laboratories and major U.S. automobile manufacturers, and members of the Engineering CoE team.
- Work is done solely by Oregon State University (OSU).
- The project needs more collaborations with members of the Engineering CoE team in terms of selection of materials for hydrogen storage.
- Collaborations appear to be in place within the Engineering CoE.
- The collaborations within the Engineering CoE are good, at least on paper, but it is unclear if they are working well.
- The GM collaboration looks like a good example.
- Good collaboration with other team members is clearly evident.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The proposed future work is mostly focused on completion of different tasks, such as simulation of optimized tank insert and integrated combustor/heat exchanger (μCHX) unit cell, as well as experimental validations.
- The proposed future work is sound.
- The proposed future work related to the complete simulation and testing of the tank insert unit cell including complete design, manufacturing cost estimate for tank insert, and complete design and testing of the microchannel heat exchanger are good based on past progress.
- Proposed future work addresses appropriate issues.
- Plans seem logical and fit well with Engineering CoE needs.
- Proposed future work is well described and logical.

Strengths and weaknesses

Strengths
- The project tries a new engineering approach to improve performance of a system, with an anticipated reduction in the size and weight of storage and improved charging and discharging rates.
- The PI has excellent experience with microchannel technology.
- The Microproducts Breakthrough Institute (MBI), a unique product development laboratory operated by OSU and the Pacific Northwest National Laboratory (PNNL) is well suited for the proposed work. The PI brings more than 15 years of experience to this project.
- The project has an innovative heat and mass transfer approach that needs to succeed in order to reach the refueling target time.
- The OSU team is well versed and fully equipped to carry out the work delegated to them by the Engineering CoE.
- The project's engineering technology and theoretical modeling seems to be already in place.

Weaknesses
- There is no external collaboration.
- The corrosion issue related to materials of microchannel by hydrogen storage materials is weak.
- The approach is "a solution looking for a problem."
- The concept looks expensive.
- No cost considerations were included.
- Strategies for dealing with hydrogen storage material containment; i.e., problems such as expansion and contraction, decrepitation, etc., are lacking.

<u>Specific recommendations and additions or deletions to the work scope</u>

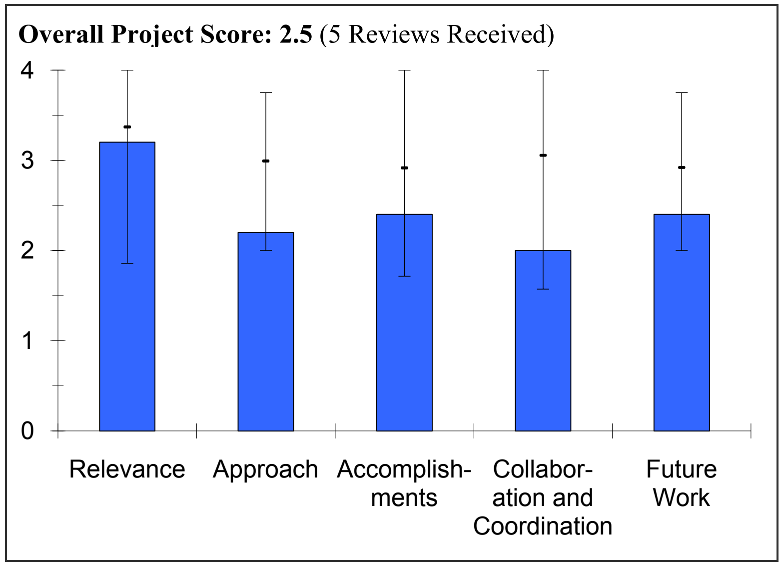
- The project team needs to do a better job of showing details of potential for microchannel technology to help specific problems in a storage device. How much will it enhance heat transfer? How much smaller or lighter can the heat exchanger be? Modeling should be able to provide some estimates.
- Get manufacturing cost estimates soon.
- Add cost considerations and materials containment issues to the scope of work.

Project # ST-47: Development of Improved Composite Pressure Vessels for Hydrogen Storage

Norman Newhouse; Lincoln Composites

Brief Summary of Project

The objectives for this project are to: 1) meet DOE 2010 and 2015 hydrogen storage goals for the storage system by identifying appropriate materials and design approaches for the composite container; 2) maintain durability, operability, and safety characteristics that already meet DOE guidelines for 2010 and 2015; 3) work with Hydrogen Storage Engineering Center of Excellence (Engineering CoE) partners to identify pressure vessel characteristics and opportunities for performance improvement; and 4) develop high pressure tanks as required to enable hybrid tank approaches to meet weight and volume goals and to allow metal hydrides with slow charging kinetics to meet charging goals.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- High-pressure tanks that are cheaper are critical to the success of the FreedomCAR Partnership goals.
- The project relates to development of improved composite pressure vessels for hydrogen storage, which is essential to the Hydrogen Program, but this project is dependent on what material one uses, and, as such, needs close coordination with the hydrogen storage material's developer. The coordination seems lacking.
- This project is very relevant to the DOE program objectives. Lincoln Composites brings a great deal of experience to the Engineering CoE. A pressure vessel will be required for materials-based systems.
- Storage costs are an important element with respect to enabling cost effective on-board hydrogen storage and therefore hydrogen vehicles.
- This is yet another exercise in futility since no tank-worthy hydrogen storage material that meets DOE hydrogen storage material targets for on-board use is available. It is, therefore, unclear why this activity is needed or should even continue. Again, the most important parameter, cost, has been mysteriously left out of the equation.

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- The approach is unclear. The PI did not clearly indicate what the primary driver of the project was (cost, safety, weight, etc.). Only after much interrogation was cost revealed as the primary driver. It is clear that the improvements will only be incremental in nature and will not achieve the team's overall targets.
- Project targets are unclear—the PI simply highlights team targets, which, we all know, compressed tanks will not achieve. The PI needs interim milestones and targets to highlight what incremental improvements for which he is aiming.
- There needs to be clarification of what the materials are.
- The approach to performing the work is well designed and the technical barriers are addressed, but integration with other efforts needs improvement.
- The approach is to establish a baseline tank design, evaluate potential improvements, evaluate design concepts, and project the ability to meet go/no-go criteria. The pathways being investigated are alternatives to current fibers, boss materials, and liners, as well as methods to toughen resins.

- There is no indication that including hardware in the tank to enhance heat transfer to the storage media will be investigated.
- The project has balance of design and material improvement approaches.
- The approach given is weak because no solid metrics were provided by which to gauge the progress and value or contributions of this effort. By now, there is only a few months before the first go/no-go decision point in October of this year; no clear direction is evident that holds even the promise of getting us closer to the DOE system-level hydrogen storage targets. [DOE note: the first go/no-go decision point is scheduled for mid-FY 11 not October of 2010.]

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- The project lacks a sensitivity analysis of the relative gains in improvement; e.g., cheaper fibers. What the fibers are (only provides relative strength), and why there identification is this a secret was not provided. There are only a few fiber suppliers and most of the specifications are available publically. Lower safety factors should be addessed as well.
- The project started at a slow pace because of funding issues.Due to this problem, the progress is not very good. Materials issues and their characterization need to be addressed more critically. For example, why do other tests on fiber D if it has poor strength? If 7075 T73 is not good for bosses, look for other alternatives like titanium alloys for better specific strength and corrosion resistance.
- Tanks with alternative fibers have been fabricated for testing. The use of aluminum boss material is being investigated as are alternative liner materials. Different resin hardeners have been selected for testing. A stress rupture project is being considered with other collaborators. The purpose of the project will be to examine the increased data base for stress rupture of carbon fiber tanks with the intent to argue for a reduced safety factor.
- Baseline data needs to be presented in a format that allows for comparison against DOE targets. A better description is needed regarding how each work area; i.e., fiber, boss material, resin changes, and liner work, will potentially contribute toward improving metrics related to DOE targets. For example, what impact on product cost (in $/kWh) will a three-times more expensive liner material have?
- The PI describes a fair amount of structural development and tank testing but gives no clear indication of why they are doing what they are doing or to what end. The PI was not present during his poster presentation, and my discussions with the engineer manning the poster did not produce any insights as to what their contributions to the Engineering CoE were. He basically referred me to the PI, who was nowhere to be found!

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Collaboration exists with the Engineering CoE, but it is not clear who the direct collaborating partners are (slide does not show) within the CoE.
- Collaboration with other efforts for material selection needs improvement.
- It does not appear that there has been a lot of interaction with the other members of the Engineering CoE. Guidance from the Engineering CoE, particularly concerning in-tank hardware requirements, is needed and could influence the paths being examined by Lincoln Composites.
- Project partners are identified, but it is not clear how they are participating or contributing.
- The PI gives the following as an indication of his collaborative effort, “collaborating on technical papers with John Khalil (United Technologies Research Center)—Lead, Kevin Simmons (Pacific Northwest National Laboratory), and Daniel Dedrick (Sandia National Laboratories).” No information was given as to the nature of the collaborative work and what was accomplished as a result of it.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Continue work, but the approach needs to be defined.

- Not clear how lessons learned will be transferred back to Engineering CoE, and, if they will, be implemented in a tank build.
- The project plans look good based on the progress, but selection of the right hydrogen storage material is essential to the success of the project.
- Future work is only briefly described. No schedule is provided to show how the information from this project will support the Engineering CoE in preparing for the Phase I decision point.
- Future work is clearly identified, though more clarity regarding schedule is needed.
- The proposed future work given is very vague and general, as if it was copied down from their original proposal. It contains no metrics that they intend to meet and where they fit in with the overall Engineering CoE objectives.

Strengths and weaknesses

Strengths
- The institution and the PI are well known for composite materials development.
- Lincoln Composites has much experience in composite tank design and fabrication. They are an industrial partner and manufacturer of high pressure composite cylinders with existing product lines.

Weaknesses
- Expertise related to the role of materials to fatigue and corrosion is lacking.
- There does not appear to be a sufficient guidance from the Engineering CoE leaders regarding Lincoln's contribution to Engineering CoE's effort.
- The project does not sufficiently identify how the efforts, individually or in aggregate, are addressing barriers to reaching DOE targets.
- The value of their contribution to the success of the Engineering CoE is predicated on if a viable hydrogen storage material can be found that benefits from composite tank designs (at system level) without cost burdens.

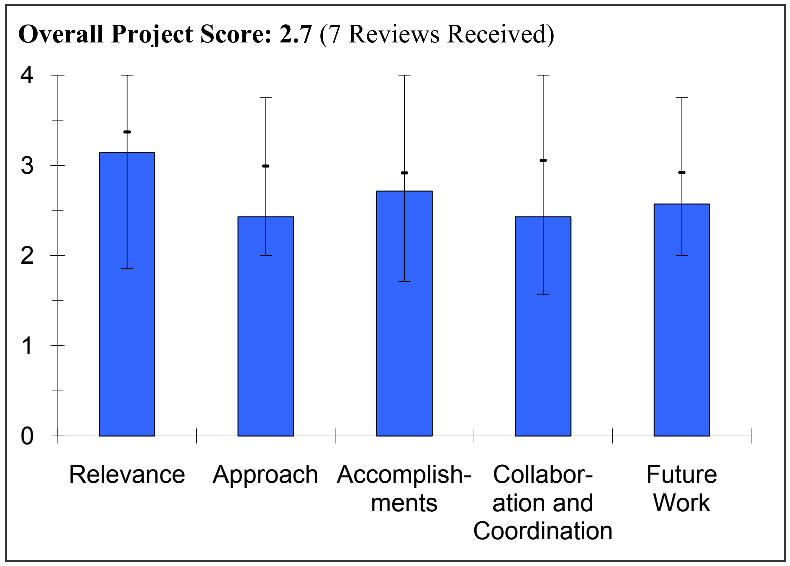
Specific recommendations and additions or deletions to the work scope

- Consider the implications of including heat transfer hardware in the tank.
- Revise the scope of work and give a solid rationale for why their contribution is needed or necessary.

Project # ST-48: Hydrogen Storage Materials for Fuel Cell Powered Vehicles

Andrew Goudy; Delaware State University

Brief Summary of Project



The objectives for this project are to: 1) identify complex hydrides that have the potential to meet DOE's goals for storage and demonstrate the optimum temperature and pressure ranges under a variety of conditions; 2) improve the sorption properties of systems that have been identified as good prospects for hydrogen storage; 3) determine the cyclic stability of new materials and develop strategies for improving reversibility; 4) perform kinetic modeling studies and develop methods for improving kinetics and lowering reaction temperatures, thereby reducing refueling time; 5) extend the studies to include other complex hydrides that have greater hydrogen storage potential; and 6) develop a viable storage system using flow, reaction kinetics, and thermal modeling, followed by system design, fabrication, and performance evaluation.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project is relevant to the Hydrogen Program's goals and objectives.
- The project involves a lot of metal hydride (MH) storage alternatives that have shown some promise yet have significant shortcomings. However, as models they may be useful.
- The project is in good alignment with DOE goals.
- The project is attempting to develop hydrogen storage materials of high capacity based upon magnesium borohydride and lithium borohydride destabilized systems.
- The materials chosen for this project currently operate at high temperatures (200°C-450°C), which does not enable them to operate with a fuel cell. However, it is necessary to study new materials systems and to understand the cause of the obstacles to be able to overcome the barriers.
- The project is well aimed at DOE targets and needs: weight and volume, desorption temperature, kinetic rates, etc.
- The project is focused on complex hydride reactions and, specifically, on capacity and kinetics, which are two of the major obstacles in this area.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- The approach is adequate for the goals of the project.
- The project would benefit by revising some of the model compounds that have been selected.
- Work should focus on mixed borohydirdes—other hydride systems are well focused. The need for studying a magnesium hydride system is unclear; it has been studied comprehensively in the past by numerous researchers.
- Is mechanical alloying the best way to make these materials?
- The researcher chose to use sodium alanate and magnesium hydride as "baseline materials" for comparison with the materials they are developing as guided by theory, which is reasonable. Magnesium hydride has been extensively studied in the literature and numerous additives/catalysts have been investigated, thus, focus should

not be on developing this material further, but on new materials discovery and development of new materials systems. Moreover, the borohydride related systems are "complex hydrides" and magnesium hydride is not, thus magnesium hydride and the complex hydrides are likely to behave differently.

- The approach has evolved over recent years and has taken better and more practical directions. That is, the PI has made good use of past data to move in new, more promising, directions.
- The acquisition of pressure-composition isotherms (PCT) is especially appreciated; most DOE projects do not make enough effort to get this important information.
- As much emphasis should not be put on magnesium hydride, even as a model system. There is so much historical data on magnesium hydride. Most of it suggests the system is not very amenable to very much thermodynamic modification. Kinetics is a different story, but also highly overworked.
- The choice of model system is not at all clear, both in terms of the system chosen, and in terms of the rationale for making this choice. Initially, it is stated that magnesium hydride is the model system, which does not seem like a particularly good choice and no rationale is given for why this was chosen. The history of this topic is very confusing; it is stated that in 2006, the model system was sodium aluminum hydride, and then in 2008 it was changed to lithium borohydride/calcium hydride, but in the initial slide of this presentation, the model system is stated as magnesium hydride. Further, it was then suggested that the choice of magnesium hydride is somehow related to the observation of reversibility in magnesium borohydride, but the connection between these two compounds is not at all clear.
- The PI chose six reactions based on magnesium borohydride to study for reversibility but it is not clear how these reactions were chosen, or how did the PI know what the expected products should be for these reactions. It does not seem as though there is a logical progression here.
- Some of the stated objectives of the progress appear to have almost no progress. It does not appear as though these items are being seriously addressed in the current project. There are essentially no results on these important points: 1) "develop a viable storage system using flow, reaction kinetics and thermal modeling, followed by system design, fabrication and performance evaluation"; 2) "perform kinetic modeling studies and develop methods for improving kinetics and lowering reaction temperatures, thereby reducing refueling time"; and 3) "determine the cyclic stability of new materials and develop strategies for improving reversibility."

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Although the project's objectives are not met, it produced interesting fundamental results on the solid-state chemistry of metal borohydrides.
- This reviewer recommends the PI to take a closer look at solid-state transformations that generate polyboranes.
- Technical accomplishments have been good, even though only partial reversibility in all of the studied systems has been shown.
- Activation energies for lithium borohydride/calcium hydride stabilized with titanium (III) chloride seem quite high.
- Hydrogen capacities after cycling are significantly diminished from the magnesium borohydride systems.
- The lithium borohydride/calcium hydride system was investigated with respect to the additives' effect on desorption temperature and activation energy, and they report a beneficial effect from using titanium (III) chloride. However, there is no attempt to explain the results, which is important to further advance the materials development.
- Six magnesium borohydride based systems were also investigated this year, based on theoretical guidance. Although three of them show reversibility, they are all high temperature systems with slow kinetics. It is recommended to perform a thorough experimental analysis of these three systems to be able to explain the reaction mechanism. It is also necessary to team with other research groups and use multiple tools, including X-ray diffraction, spectroscopy, neutron, synchrotron, and *in situ* studies.
- The third theme presented is on magnesium hydride and magnesium nickel hydride based systems with additives to enhance performance. It seems like the researcher is disconnected from other on-going efforts on the hydrogen program and there has been a few papers published from the Metal Hydride Center of Excellence (MHCoE) on magnesium hydride + titanium hydride mixtures in the past few years, thus, it seems less relevant to duplicate the efforts in this project. However, the results obtained are different as the MHCoE group found

that nanosized powders improved performance. It is recommended that Goudy, et al., also consider nanosized hydride mixtures as a complement to the additive study.
- Much useful and interesting data have been generated.
- The results on destabilized magnesium borohydride is interesting and potentially useful, especially the discoveries of partial reversibility at mild conditions. Again, the PC isotherms are especially valuable in understanding destabilization.
- The technical progress seems quite minor, given the length of time this project has been ongoing, as well as the committed resources. It is not even clear who is actually working on the project, and who is merely a (no-cost) collaborator. For $500,000 per year, there should be many PIs, students, and postdoctorates working on this project, and it is not apparent who is doing what with all this money.
- The PI stated that magnesium borohydride is reversible at less than 200˚C, but then showed no proof (or a reference) to this fact. It sounds very surprising and contrary to what others have seen in the field, and some evidence seems really necessary. Under technical accomplishments, the PI states, "we have determined that several destabilized borohydride systems based on magnesium borohydride can absorb hydrogen reversibly starting at temperatures less than 200˚C." This is a very important and provocative statement, but the PI didn't provide any evidence to this effect (specifically, that any material can 'absorb' hydrogen at temperatures below 200˚C. All the data shown is for desorption.It is expected that the work on magnesium hydride is probably not new. This material has been extensively studied, alloyed, etc., over the last 30 years. The PI should definitely do a better job of justifying the choice of magnesium hydride and placing this work in the context of the (considerable) literature in this field. For instance, the PI seems to think that these additions are "catalytic" and will reduce the desorption temperature. But, this presumes that magnesium hydride alone is not desorbing at the thermodynamic transition temperature, which it likely is. It is well known that the enthalpy of reaction (ΔH) for magnesium hydride is consistent with a thermodynamic desorption of about 300˚C, which is observed. It is not clear why one would add catalysts to decrease this temperature. One should add catalysts to increase the 'rate' of desorption. A lowering of the temperature would be an indication of a change in thermodynamics; i.e., a change in reaction, but not a catalytic effect.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.4** for technology transfer and collaboration.

- Surprisingly, no DOE research centers or national laboratories listed among the collaborators.
- Sufficient collaborations.
- Collaborative activities might be strengthened somewhat.
- It appears that there is a lack of coordination with other on-going efforts, and this needs to be improved in order to avoid duplication. Goudy, et al., would benefit from teaming with other institutions that have the research instruments needed to perform a more in-depth study of the materials under development.
- There are a few good collaborations; as indicated, the PI is looking for more.
- The very practical work being done in this project would be useful to the Hydrogen Storage Engineering Center of Excellence (Engineering CoE) objectives and activities. It is strongly suggest some formal collaboration be developed with this large group.
- Why is this project not a part of the MHCoE? It was started in 2006, so it seems as though it could have easily been incorporated (even informally) into the MHCoE. Some collaborations with other groups working on these topics could be quite useful.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Future work proposal is reasonably good. The PI may think about dedicating some additional resources to studying the solid-state transformations of borohydrides.
- Future work is vaguely described.
- Modeling of kinetics has been envisioned for a long time, but as far as I can see, has not been done, and is proposed again.

- There is no need to concentrate on magnesium hydride (as proposed, "use various catalysts and combinations of catalysts to lower reaction temperatures and increase reaction rates. magnesium hydride will be used as a model system in these efforts.")
- At 7 years (began 2006, ends 2013), this is a very long-term project and it is not clear why it is so long?
- The effort on preparing and characterizing magnesium borohydride based systems should continue, preferably with theoretical guidance. However, based on the results presented here, it is recommended to turn to the results in the literature when using magnesium hydride as a model system, rather than spending time on experiments.
- It is recommended to add a task to focus on understanding reaction mechanisms of a selected promising materials system in order to learn about intermediate species forming and solid state phase transitions, as well as impurities forming, such as diborane coming off. With this knowledge, the reasons behind slow kinetics and high operation temperatures can be understood, which will guide further materials development.
- Proceed as planned.
- Overall, the project doesn't have good focus and justification for why the materials chosen are being studied. For that reason, the future work also doesn't seem to be well justified. Based on the progress to date, it does not appear likely that significant progress will be made in the future.
- The PI listed this as a 7-year project. (If this is a program that has been renewed, it would be useful to know this, and not to quote it as 50% complete over a 7-year span. [DOE note: this is a Congressionally Directed Project that was renewed in FY 09]

Strengths and weaknesses

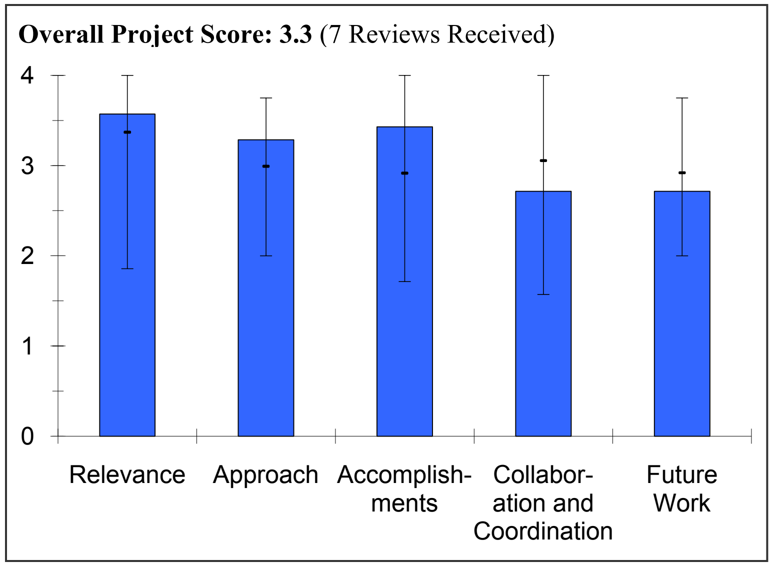
Strengths
- The project has a strong materials science focus.
- High hydrogen capacity borohydrides are being considered.
- The magnesium borohydride and lithium borohydride stabilitized systems have the potential for high hydrogen capacities.
- The project strength is the focus on developing and improving high capacity materials that are challenged by slow kinetics and high operation temperatures.
- The PI has experience with practical hydride properties and evaluations such as PC isotherms, thermodynamics, kinetics, reversibility, etc.

Weaknesses
- The materials chemistry component has quite a bit of room for improvement.
- There is too much concentration on magnesium hydride, especially in repeating what has been already done.
- Significant reversibility issues and high absorption/desorption temperatures are still problem areas.
- There needs to be more focus on understanding the results by embarking on reaction mechanism studies. The project seems disconnected with other ongoing efforts and needs to coordinate better to avoid duplication. The project team needs to publish more peer-reviewed papers.
- The base hydride materials studied are not always very novel.
- It does not seem as though DOE is getting a very good return on its investment with this project. The investment is large, but the results are very minor, with no real insight to help improve these materials. In times of rapidly decreasing budgets, DOE cannot afford to dedicate a large amount of resources to a project like this.
- In the four years that this project has been ongoing, the PI has published two papers in the area. Both have zero citations (according to the Web of Science).

Specific recommendations and additions or deletions to the work scope

- The project team should consider elimination of the magnesium hydride work from the work scope; more focus should be on destabilized borohydrides.
- The project might benefit from chemical approaches to synthesize the destabilized materials.
- It is recommended to add a task for a reaction mechanism study that utilizes multiple sets of analysis tools and teaming with other groups.
- The work scope should establish at least a limited collaboration with the Engineering CoE.

Project # ST-49: Hydrogen Storage in Metal-Organic Frameworks
Omar Yaghi; University of California, Los Angeles



Brief Summary of Project

The overall objective of this project is to increase hydrogen storage at room temperature. Objectives for fiscal year 2009-2010 are to: 1) implement "soft chemisorption" in the design and preparation of new metal organic frameworks (MOFs) with metal binding sites and with impregnation of metals and low-pressure measurements at various temperatures; and 2) prepare high surface area MOFs with preparation of expanded organic links, high throughput MOF synthesis, and activation of high surface area MOFs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The project is relevant to the Hydrogen Program's goals and objectives. However, its focus started drifting away from the original goal, the high capacity hydrogen storage in MOFs.
- The project is adressing the development of new materials for hydrogen storage, some materials exhibit promising properties for transportation applications.
- The project seeks to develop MOFs with tailored metal sites for improved hydrogen storage capacity and binding energy. This goal is within the DOE objectives for hydrogen sorbent materials.
- The development of new, high surface area materials for hydrogen storage is essential to eventual practical applications; e.g., fuel cells. This work shows 1) incremental improvement in hydrogen uptake for new materials; 2) good reproducibility and reliability of the synthesis and measurements; 3) pursuit of some new ideas; e.g., metals incorporated into MOFs; and (4) progress in the scale up of MOF production. It is good to see that this work has not fallen victim to the seemingly irreproducible and spurious results of large hydrogen uptake in carbon based materials commonly known as the "spillover effect."
- High surface area materials are likely candidates for hydrogen storage if heats of adsorption and bulk densities can be improved.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Approach has not changed from the previous year.
- Sharp focus on achieving storage density objectives. It would have been interesting to classify the materials in terms of cost in addition to effectiveness, and to discuss the possibilities of scaling production.
- A systematic approach to attach linkers which easily substitute to add platinum or paladium dichlorides is undertaken. A suite of characterization tools, including EXAFS, analyzes the platinum and paladium metal sites on the MOF-253 material.
- The approach of adding metal sites to the linkers in MOFs seems like a good approach, although it has proven to be challenging to de-solvate the final material to expose the metal site to hydrogen. It is not clear what the "high throughput MOF synthesis" approach is from the presentation.
- High surface area materials are obviously desired.
- The synthesis effort is good, but the project needs to add densification efforts.

- It is not clear that synthesizing compounds with ever higher surface areas will solve the hydrogen storage problem. This reviewer would prefer to see more of an emphasis on increasing diatomic hydrogen adsorption enthalpies. With this in mind, it is suggested that only the first of this project's two objectives is worthwhile.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- In general, the project produced quite a bit of very interesting basic results, which explains the "good" rating. However, it slowed down during the last two years. this reviewer did not find much new and exciting results or ideas in the poster presented at the AMR this year.
- The researchers realize that it will be necessary to shift from dichlorides of platinum and paladium to other ligand groups. The chloride groups inhibit hydrogen uptake.
- Regarding "high throughput MOF synthesis," it is assumed this is different from the scale-up effort, although it is not clear from the presentation. In any case, it is attractive to use some kind of high throughput or combinatorial approach to search for new materials, but an essential part of the technique is to have a rapid assessment of the result to see in a simple and easy way if there is something interesting. All too often, a "high throughput" approach means a rapid synthesis of lots of samples, or material, and it remains a laborious and tedious task to do the usual measurements and characterization of each sample. The real elegance of a combinatorial approach is really the measurement side of the process, to have a fast way to assess the products of the synthesis step for any interesting result that can then be followed up on.
- Good excess diatomic hydrogen capacity, but moderately high pressure and low temperatures are required.
- Efforts to develop MOFs with high capacity and high heat of adsorption have not yielded any materials yet.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Collaboration is limited to just a few organizations and, surprisingly, does not include DOE CoEs.
- There were few specifics on collaborations. This reviewer questions why this project was not integrated with the activities of the Center of Excellence?
- There are few existing collaborations highlighted within this work. The poster lists collaborators in introductory slides, but collaborative results are not shown.
- Some collaborators are listed, but the presentation does not specifically indicate the contributions from them (especially Jeff Long and Bill Goddard). It is unclear what the various collaborators have contributed to the work presented.
- Collaborators are listed, but little or no description of their level of involvement is given.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Not much is planned for the future. It is unclear if the PI is planning any research in the area after the project expires.
- The project is 90% complete—no comments here.
- There are plans to collaborate with a theory group for further developing the understanding of metal attachment and hydrogen uptake.
- The project ended April 30, 2009. There needs to be more elaboration on future work; only two bullet points on the summary slide were given, and they are not very complete.
- There should be a resonable extension of existing work.
- Not applicable because the project is complete.
- The two strategies for future research mentioned in the poster (slide 21 and reproduced below) are too vague to determine if anything novel is in fact in the pipeline.
    - Employ lightweight metals to create strong binding sites.
    - Material design based on theoretical prediction.

Strengths and weaknesses

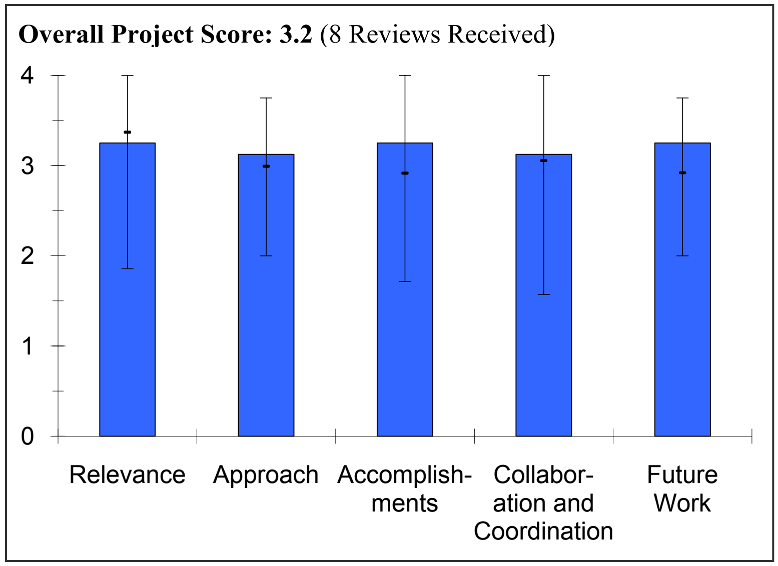
Strengths
- This is a very strong basic chemistry project.
- A new promising class of materials has been identified.
- A systematic approach to improving dihydrogen binding energy by attaching linkers which may attach metal sites is good.
- There has been good work on improving MOFs for hydrogen storage produced, with innovative ideas about metal incorporation into the linkers.
- MOF synthesis expertise in this project is outstanding.
- Very interesting materials are used that are great for low temperature, (77 K) storage.

Weaknesses
- The project has had limited collaboration and limited progress during the last two years.
- Lack of collaboration with other program participants.
- There were few collaborative results presented for this MOF project.
- It needs more explicit incorporation of collaborators' work in the project. More collaborators would strengthen the project. It is not clear what the "high throughput MOF synthesis" part of the project is, and what the results have been.
- Lack of efforts to make materials more practical (densification).
- Suitability for room temperature storage meeting DOE targets is highly questionable.
- There is an impression that this project is taking too much of a "try this and see what happens" approach and lacks a clear strategy.

Specific recommendations and additions or deletions to the work scope

- Identification of other applications beyond storage—hydrogen purification, carbon dioxide sequestration, storage of natural gas, etc.
- Develop collaborations with a theory group (as planned).
- More attention to relationship between surface area and hydrogen storage would improve the scope of the project. Something more involving combinatorial studies of MOFs, particularly with regard to how to rapidly assess the materials produced, would be a great addition to the project in the future.
- Screen methods to pelletize or use other methods to provide physically usable sorbents.
- Not applicable because the project is complete.

Project # ST-50: Hydrogen Storage through Nanostructured Porous Organic Polymers (POPs)
Di-Jia Liu; Argonne National Laboratory;



Brief Summary of Project

The objectives for this project are to: 1) design, synthesize, and evaluate nanostructured porous organic polymers (POPs) as new hydrogen storage adsorbents for transportation applications, and 2) support polymer materials development with modeling/simulation and advanced structural characterizations.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This projects aims the development of nanostructured POPs as new hydrogen storage adsorbents.
- The development of high surface area polymeric materials opens up a wide option of new materials for physisorption storage and is very relevant to DOE goals.
- POPs are good complement to other high surface area sorbent areas,including metal organic frameworks (MOFs),in the program.
- The project's objectives are relevant to the hydrogen storage goals. The project explores the potential of a new class of materials called POPs for hydrogen storage applications.
- Project objectives highly relevant to the DOE RD&D objectives in regards to the synthesis and evaluation of sorbent POPs for on-board storage.
- Project focuses on demonstrating improvements to volumetric density, a key barrier associated with this class of materials.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- The pressure to make a material that does it all has lead to a wealth of new POPs, and the PI has focused down on some of these materials that show potentially beneficial characteristics. The sheer number of materials listed makes it difficult to see if they have any type of adsorption characterization on all of them, and hence look for trends amongst the different monomer/treatments they have performed. Of those selected to be presented, the application of the right characterization tool to the issue at hand seems to be well thought out and input of theory when needed on top of this. The work is focused on both addressing pore sizes and increasing enthalpy of reaction (ΔH) through metal incorporation.
- The approach is consistent and builds on previous progress.
- The synthesis work is clearly very strong. More focus and collaborative support on hydrogen sorption characterization is needed.
- Experimental efforts are being guided by theory and insight.
- The approach has good synthesis and materials modifications strategies.
- Competency in characterization and sorption methods are an asset.
- The project's approach is well balanced between theory, experimental synthesis, and characterization of these materials. However, it is not clear how the project ultimately plans to reach the capacity, especially if metal doping is necessary to raise operating temperatures. Same challenge applies to almost all the carbon based adsorbents.

- Approach to materials discovery and development is ideal, involving novel, “rational” materials design and synthesis, property characterization and optimization; i.e., structured experiments to identify trends, and modeling/simulation for streamlined experimental efforts. The approach is structured and flexible and the goals are specific; e.g., forming POPs with high surface area and narrow/adjustable pore size. It is great to see that the project aims to understand structure-property relationships at a fundamental level.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- A dazzling array of materials has been synthesized and partly characterized (at least in terms of surface area) with some of the higher surface areas for these types of systems and spectacular control of the pore size and distributions. The uncertainty in what is required to make the best storage system makes this work a good candidate for cryogenic, or even intermediate-range temperature storage materials, but is unlikely to contribute to a room temperature tank beyond adsorbents already known. There could be some advantages in the polymer processing of these types of materials over activated carbons, however, and that aspect should be borne in mind.
- Efforts to increase ΔH have taken several routes, most with modest increases evident, but while still maintaining a remarkably high surface area and cryogenic storage capacity in several systems.
- This project led to the development of POPs that are clearly still inferior to the state-of-the-art activated carbons and MOFs. The volumetric storage capacity of these material is still an unknown quantity, and the PI is encouraged to give some results in this area, or at least the bulk density of the synthesized materials. Their compressibility without structural or capacity loss is assumed but yet to be proven. It is highly unlikely. Moreover, the PI repeatedly presents his materials as having higher binding energy. He is only presenting data at low pressure (low coverage) where the binding energy is initially high for all adsorbents. He really needs to extend his calculations to higher pressure (meaningful coverage) and compare with activated carbon and MOFs.
- Significant accomplishments have been made in synthesizing a number of new nanostructured polymeric materials with variations in pore size, metal and non-metal doping, and modified adsorption enthalpies.
- The ability to demonstrate the ability to tune the micropore is very good progress.
- The ability to dope the polyporphyrin based POP (PTTPP) with iron and show an enhancement in the heats of adsorption is also very good.
- Synthesis of boron-containing and metalloporphyrin polymers is significant achievement.
- The strategy for fine tuning POP structure and nanopore architecture has yielded significant increases in heat of adsorption. The results agree with theory.
- Capacities are still low compared to targets.
- Given that the project is in the middle of its cycle and that this is a new research area, the project has made excellent progress.
- The ability to synthesize and control the material pore size and reliable characterization is significant.
- The improved heat of adsorption with transition metal doping without sacrificing pore size is an interesting result.
- A variety of diverse POPs were prepared. It was clear this project was deliberately structuring experiments to improve key challenges with sorbents, for example utilizing porphyrin-based monomers with various transition metal cores.
- Correspondingly the impacts of metal cores on the hydrogen uptake and ΔH were analyzed. The fluid connection to theory was also demonstrated by modeling POPs with various transition metals. Finally, it was great to see an evaluation of pore size and the optimization/tuning of pore structure based on using various aromatic monomers. Overall, the project has made great progress with well thought out experiments!

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- There is certainly strong collaborations within this team of people, and their presence in the Hydrogen Sorption Center of Excellence (HSCoE) is likely a boon for them over the past few years, and focuses the results toward addressing DOE goals. While it is clear that there are different functional arms of the collaborations, the cross-fertilization between theory and experiment and synthesis seems to be present allowing for a well run team.

- There are strong collaborations with industry, the National Renewable Energy Laboratory (NREL), and universities on synthesis and measurements.
- There are good collaborations within and outside the Center.
- There is an appropriate level of collaboration.
- The project is leveraging expertise of the University of Chicago subcontractor, and is interacting as part of the HSCoE. Various other collaborations are evident, for example, for characterization and measurement validations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The addition of a go/no-go decision point will keep the program on track to address the DOE goals. The work seems now more sharply focused on addressing high ΔH materials, and, in the coming year, looks like it will capitalize on these improvements and look at the materials science problem of volumetrics.
- The project has a built in go/no go decision for next year.
- Future work is consistent with project goals.
- Special consideration should be given to the possibility of POPs pelletization.
- There is a reasonable extension of current work.
- Future plans look good.
- Project team gets high marks for highlighting measurable go/no-go decision point; although, they should also have specified the working pressure with the gravimetric and temperature targets.
- Well-planned future studies which are a continuation of current results. It is great to see a timeline of work intended for the remainder of this year, as well as going forward into next year.

Strengths and weaknesses

Strengths
- The diversity of sample synthesis, characterization, and computational skills in the team and through collaborators. Also, the enormous productivity of new materials combined with the in-depth focus when warranted.
- New type of storage materials are being used.
- Developing the ability to tune pore dimensions and maintain high surface area is a great accomplishment.
- The ability to tune the micropore is very good progress.
- The ability to dope the materials with iron is very good.
- Polymer synthesis and good adsorption work.
- The project area of research is new.
- The project team presented the positive progress made as well as the challenges in context by plotting hydrogen uptake curves at 77K and 298K in the same plot. This may sound trivial, but it is important that the PIs, students, and reviewers all keep in mind the intermediate achievements relative to the ultimate target.
- It is a very organized and focused project that is making great progress. Novel materials focused on addressing sorbent challenges, e.g. improved binding, are being studied.

Weaknesses
- Alas, some of the strengths are also weaknesses. The prolific nature of the materials synthesis likely does not allow for a full and deep understanding of what they have actually managed to achieve in producing these arrays of materials. This is somewhat of a problem with the applied nature of the program where the end goal is the only thing that matters, but it seems like a waste of a learning opportunity.
- The project has not given a clear explanation for the unusual shape of the physisorption isotherms.
- The project needs to address the ability to increase capacity.
- Pelletization and finding ways to enhance the volumetric capacity is needed.
- The research topic is of a difficult nature.

Specific recommendations and additions or deletions to the work scope

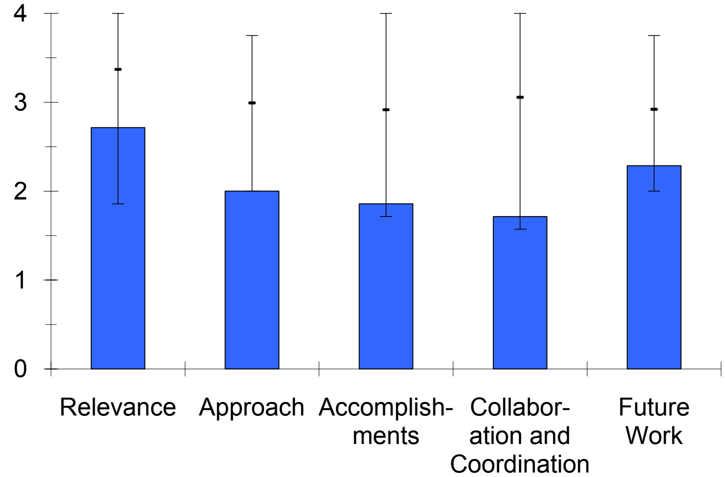
- Allow some scope to investigate more the differences and similarities between new materials and the effects on diatomic hydrogen uptake characteristics.
- Post-treatment methods (heating) seems to have made the most impact on changing adsorption enthalpies. Reviewer recommends focusing efforts on these post-treatment options in combination with the increasing surface area of the primary materials.
- Reviewer recommends collaborative testing of these materials such as dielectrics in capacitance-assisted diatomic hydrogen storage project ST-026.
- Keep the current project; however, it is recommended to start considering finding ways to effectively enhance the volumetric capacity.
- Increase polymer characterization/physics to better understand heat treatment of boron containing materials.
- The potential cost advantage of POPs (if any) should be highlighted when comparing their performance against similar materials such as MOFs.

Project # ST-51: Electron-Charged Hydrogen Storage Materials
Chinbay Fan; Gas Technology Institute

Brief Summary of Project

The overall objective for this project is the development of a hydrogen storage material and device for hydrogen quick charge and discharge, high percent weight (wt%) and percent volume (vol%) storage capacities, good durability over many cycles, and safe handling and transport. The 2009 objectives are to: 1) reselect the best hydrogen storage materials for charge modifications; and 2) develop carbon-based materials, such as AX-21 and other high surface carbon using polymer as precursor, metal-modified carbon, and ammonia borane (AB).

Overall Project Score: 2.1 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- There was no articulation in the presentation stating, except in the broadest terms, the project's goal, so it is impossible to judge on the scientific or technological merits what the Gas Technology Institute (GTI) is attempting to do.
- This project is based on the interesting idea that introducing a new thermodynamic variable to control the properties of a solid-state storage unit for hydrogen.
- The project is relevant to DOE objectives and utilizes external electron charge effect on increasing fueling rate and capacity of hydrogen charging..
- This project uses an applied electric field to improve hydrogen uptake in carbon (physisorption), metal hydrides, and AB. This is a unique approach and appears to improve hydrogen gravimetric capacity.
- The project "Electron-Charged Hydrogen Storage Materials" seems far fetched to overall DOE objectives.
- The project is generally aligned with the aims of the program; however, it takes a very high risk approach.
- This project employs a unique and entirely unconventional approach for hydrogen storage. Although, in this reviewer's view, the project is a "long shot" for meeting DOE targets; it is aligned with DOE objectives and it has sufficient novelty and potential to justify continued support.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- The approach slide shows a heating and cooling coil connected to a Sieverts. There is no explanation as to what has been done in this work and what the system that is connected to the Sieverts is meant to accomplish. As a schematic, there appears to be nothing unique in this configuration.
- The project suffers from a lack of guidance from either theory or simulations, particularly in the way the materials are selected. Specifics on the mechanisms should have been given in the presentation, and a discussion on how materials selection was performed based on those mechanisms.
- The approach of the project is to test different materials in Sievert-type thermal gravimetric analysis (TGA) stations for the effect of external electron charge on hydrogen adsorption/desorption kinetics.
- The electric field is applied within a Seivert's apparatus concurrently with increasing pressure. The research has isolated the effects of heating and cooling from increased applied voltage.
- The approach seems very sketchy relating to selection and synthesis of carbon-based materials.
- Use of electrochemistry to attack the problem is worth trying.

- Materials are good from a mass basis (carbon) to questionable (metal hydride).
- It is not clear if they have deep understanding of how this works. They certainly could not explain it well in depth.
- An approach has been adopted that uses both internal electron charge (doping) and external charge to alter the electron distribution in a hydrogen storage medium, thereby enhancing electrostatic attraction of hydrogen to the substrate. It is suggested that enhanced hydrogen adsorption occurs via induced dipole interactions between the charged substrate and the hydrogen molecule. However, the approach is highly speculative, and very little theoretical work has been performed to support the notion that improved hydrogen storage yields can be expected under practical current and voltage conditions.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.9** based on accomplishments.

- Slides 8 to 12 show pressure-composition isotherm (PCT) curves, but do not specify the metal used, what the numbers in the plot mean, or what the data show.
- It is unclear what does the weight loss in slide 13 means.
- Unfortunately, none of the slides have any meaning as there is no explanation of what is being measured and what the measurements might mean and what the motivation for the measurement approach might be.
- Although interesting effects are observed, they are somewhat marginal at ambient temperature, occurring at relatively low pressures (in the sense of the DOE storage targets for hydrogen) and disappearing at pressures of interest for storage applications.
- Very minor progress was made, few PCT curves of unknown material show minor increases in capacity;no data on the effect on kinetics were given.
- Many results were shown that show the applied electric field is successful at improving hydrogen capacity. The PI is in the process of validating these experimental results with several more trials.
- The results obtained so far do not look promising.
- The project has generated some data, though a bit low for one year.
- The graphs were mismarked, which may confuse reviewers.
- The increase in capacity is a good accomplishment, and note that the usable amount increases, too. This needs to be confirmed.
- Also claim to regenerate AB in this process.
- The basis of the extended hydride capacity is thought to be filling metastable states with energy too high to fill with moderate pressure.
- Only minor improvements (~20%) in storage capacity have been observed in a metal hydride (microporous AB on different support substrates) during electron charging. Additional work has recently focused on boron nitrides. However, results thus far are not particularly promising. No analysis has been performed to predict what actual storage capacities might be expected under the operating conditions used in these experiments. Overall, there has been only limited progress on this project since the last review.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.7** for technology transfer and collaboration.

- It is not clear what the nature of the interaction with other groups is, save for taking a few measurements.
- Not enough detail on the collaborations were provided.
- This reviewer is not clear at all on what sort of collaborations the PI had.
- There is no clear indication that this work is done collaboratively.
- Not much collaboration with Hydrogen Storage Engineering Center of Excellence partners.
- Several groups are named, but it appears that there is limited collaboration with them. Still they are on a fairly different sort of project, so that is not a terrible thing. Ought to compare data with the Los Alamos National Laboratory.
- Only limited collaborations are evident. It’s not totally clear what contributions are being made by the cooperating institutions.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Given the lack of detail in what GTI has accomplished, the goals, as set forth in the proposed work, lack context.
- The project seems to emphasize enhanced kinetics in borohydrides, and seems to conclude that little benefits are to be expected for enhanced storage capacity.
- The proposed future work is nothing more than continuation of the previous work, probably with the same limited success.
- The PI is looking to scale up to a 11-liter tank and move to the higher capacity system, AB. These are both worthy future plans.
- Changing from carbon-based materials to boron nitrides for external charge bias to increase the hydrogen storage rate is not clear.
- The key is the large scale tank and confirmation outside.
- Proposed work calls for scale up to an 11-liter tank. However, given the paucity of experimental data and the lack of theoretical results obtained thus far, it seems premature to propose a scale-up task. The continuation of the boron nitride work is probably worthwhile, but a more definitive statement about how the experimental work will actually be done is important. A forthright presentation of barriers and obstacles that must be overcome in order to achieve high capacity storage by this method is needed. Finally, plans in place to address those challenges should be presented.

Strengths and weaknesses

Strengths

- None.
- This project is based on an interesting idea, that of introducing a new thermodynamic variable to control the properties of a solid-state storage unit for hydrogen.
- The project tries a new engineering approach.
- The project ideas are unique and may effectively lead to alternative means for improving hydrogen loading.
- The PI has good background in electron shift (physisorption) and electron transfer (chemisorption).
- The project approach uses a very “out of the box” idea.
- The project will open a large new area of research if successful.
- A novel approach to enhancing hydrogen adsorption in a reasonably simple experimental system is proposed.

Weaknesses

- The nature of the work is of no value to the program as slides of data with no explanation are presented. The presentation needs to be self explanatory and not have to rely on questioning at the poster session as to the details of the work being presented.
- Although interesting effects are observed, they are somewhat marginal at ambient temperature, occurring at relatively low pressures (in the sense of the DOE storage targets for hydrogen) and disappearing at pressures of interest for storage applications. The results should also be validated by another group, and I did not see results at 77 K, which would help show whether the enhancement observed depends on a low level of filling of the pores (as suggested by the high pressure room temperature results). This reviewer also feels that there is a lack of theoretical guidance as observed by another reviewer. Referring to nanotubes as theoretical justification is a bit restrictive, taking into account the specific properties of these carbon nanostructures, which are very different from the other structures investigated in this study.
- The project shows limited improvements, and is lacking of theoretical justification and predictability.
- There is not an adequate assessment of energy balance; i.e., the amount of energy required to apply the voltage during loading relative to the amount of improved hydrogen capacity).
- The theory is not clear for the experimental design.
- The project team needs to confirm that the theory is right so that they can move foreward rapidly.
- The project team needs to figure out how to apply the process to materials with a much higher capactiy, as a small increase in a 2% material is not going to make progress toward meeting the goals.

- The PI could not explain exactly how the hydrogen is pegged to the voltage.
- There is a lack of a predictive theory or model to guide experimental effort; experimental results obtained thus far are not promising.

Specific recommendations and additions or deletions to the work scope

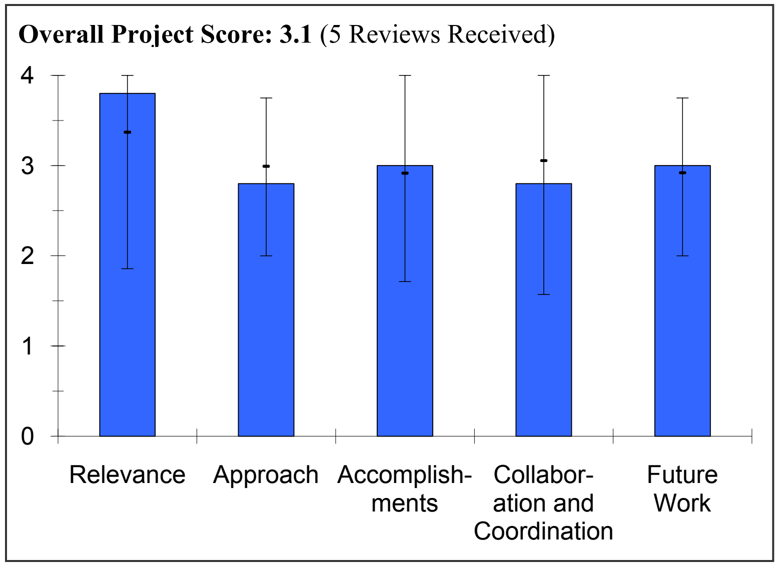
- This project appears to contribute nothing to the program, and I strongly urge that the project be suspended.
- This reviewer recommends calculations that show that the amount of energy gained from the additional hydrogen stored due to the applied voltage is greater than the amount of energy used in applying this voltage.
- The absolute first priority is to load a hydride and to have it tested outside to confirm the electrostatic influence does increase capacity over normal pressure loading rates.
- I would suggest doing a full capacity enhancement demonstration in which one polarity is applied in loading and then reversed in unloading to demonstrate the highest capacity accessible between 2 bar and 350 bar.
- It is critical to thoroughly and straightforwardly articulate the obstacles and challenges that have been identified in the experiments thus far, and to explore possible mitigation strategies. Reviewer recommends that the scale-up task be deleted or postponed until more definitive experimental results on a small-scale system are obtained.

Project # ST-53: Lifecycle Verification of Polymeric Storage Liners

Barton Smith; Oak Ridge National Laboratory

Brief Summary of Project

The project goal is to perform durability qualification measurements on polymeric tank liner specimens and assess ability of liner materials to maintain required hydrogen barrier performance. Milestones for 2010 are to: 1) complete thermal cycling and permeation measurements in Lincoln Composites liner materials; 2) complete thermal cycling and permeation measurements in Quantum Technologies liner materials; 3) complete measurements of hydrogen solubility, uptake, and the effects of hydrogen-induced swelling in tank liner materials; and 4) make a go/no-go decision on the acceptability of existing liner materials.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- The project "Life Cycle Verification of Polymeric Storage Liners" is relevant to overall DOE objectives because the durability of polymeric tank liners over the performance lifetime of high pressure storage systems must be verified and validated.
- Liner performance is critical to performance of early stage tanks.
- Storage costs are an important element with respect to enabling cost effective on-board hydrogen storage and, therefore, hydrogen vehicles.
- The project has important relevance in assessing the performance and safety of type IV pressure vessels. There has been work in the area of evaluating metals through hydrogen embrittlement work. This project focuses on testing the performance of polymers over the cycle life, which is very useful.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The approach to follow the Society of Automobile Engineers (SAE) International technical information report J2579 to develop and carry out durability test cycling measurements is good. A critical approach to address the shortcomings of SAE J2579 will be relevant.
- Good experimental apparatus and protocol for addressing problem.
- The approach is well conceived and based on industry developed standards and performance requirements.
- The general approach is fine but appears to have some areas that could be improved:
    - Consider if the bench test results could be validated against a complete cylinder; i.e., test a sample from a cylinder that completed cycling in a laboratory or in the field.
    - Consider providing further details on the test method to help the codes and standards organization develop a material permeation test.
    - Confirm theories with models or other analysis for cycling effect.
    - Include additional materials beyond standard liners to evaluate their potential.
    - Include expanded temperature range such as -40˚C and lower and potential for exceeding the traditional 85˚C maximum.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- It is good to see completed permeation measurements through 2,000 cycles that show no statistically significant departures from Arrhenius relationship and no microcracking or changes in glass transition temperature in the polymer. Analysis of activation energy indicating that the polymer is undergoing microscopic changes in polymer matrix will be helpful for future work.
- The effects of aging are clear.
- The work to date has indicated that current materials are meeting performance expectations. Information regarding performance relative to DOE targets should be provided.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Collaboration and coordination with other institutions such as Lincoln Composites, Quantum Technologies and Ticona are helpful for the project.
- Very little collaboration is evident, but it may not be needed in this project.
- This type of effort does not require a significant amount of collaboration, but it would be helpful to understand how container manufacturer participants are contributing (aside from liner samples).
- It seems like there could be some collaboration with the Hydrogen Storage Engineering Center of Excellence.
- The collaboration partners appear to be the industry leaders in type IV tanks. Since this project is focused on testing of materials, it may be useful to consider including or consulting with hydrogen test facilities to compare lessons learned. It would also be useful to provide or present the project to SAE and other standards organizations for feedback in the testing method.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Future works to verify initial lifecycle measurements including temperature cycling at 860 bar and 430 bar pressurization are going to be helpful.
- Continuation to max cycles is a reasonable extension.
- The work is well structured and scheduled.
- The future work is focused simple on testing. The work should also include an attempt to validate the results, understand the root cause effecting the polymers, transfer knowledge to codes and standards organizations, and assess potential of other materials.

Strengths and weaknesses

Strengths
- The Institution and the PI are well placed for the project.
- Good component-level assessment work.
- The project covers a needed area of research in assessing and comparing polymer materials in a hydrogen lifecycle evaluation.

Weaknesses
- Additional insight into tank liner materials for measurement of hydrogen solubility is necessary.
- It needs to be made clear that the project is addressing a problem the industry actually has.

Specific recommendations and additions or deletions to the work scope

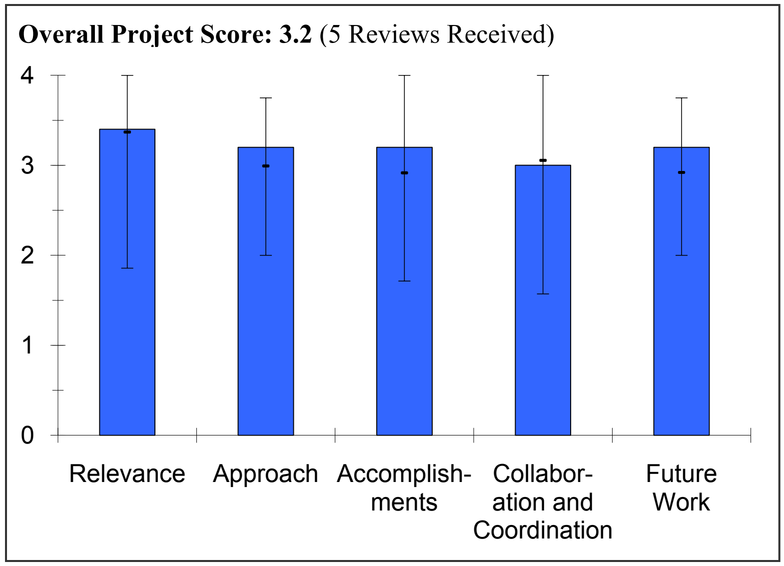
- Work with polymer physicists to ensure maximum information is gained from postmortem analysis. For high density polyethylene (HDPE), analysis should include nuclear magnetic resonance, thermomechanical analysis,

etc. Polymer producers (Chevron, ConocoPhillips, ExxonMobil, and others) should be consulted since they are the most likely to have state-of-the-art HDPE characterization.
- Overall, it's not clear if anything useful is being learned at the materials level, as it is not clear that characterization of the Lincoln Composites and Quantum liner materials compositions is being done or is allowed.
- If these materials are proprietary, it is unclear how the degradation mechanisms will be made public.
- While it's important to know if durable tanks (and their liners) can be made, This reviewer has the impression that this is an analysis that Lincoln Composites and Quantum ought to be performing at their own expense.

Project # ST-54: Standardized Testing Program for Solid-State Hydrogen Storage Technologies
Michael Miller; Southwest Research Institute.

Brief Summary of Project

The objectives for this project are to: 1) support DOE's Hydrogen Storage Program by operating an independent national laboratory aimed at assessing and validating the performance of novel and emerging solid-state hydrogen storage materials and full-scale systems; 2) conduct measurements using established protocols to derive performance metrics for capacity, kinetics, thermodynamics, and cycle life; and 3) support parallel efforts underway within the international community, in Europe and Japan, to assess and validate the performance of related solid-state materials for hydrogen storage.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Conducting accurate storage measurements is extremely difficult and subject to much error. The program needs a gold standard, independent facility to validate results from many different sources. The Southwest Research Institute (SwRI®) has established themselves as the gold standard.
- The project at SwRI® is primarily to serve as an independent measurement center for the DOE/EERE Hydrogen Storage Program, rather than to discover or develop new storage materials. They do serve an important function in the program to determine whether the claims made by other research groups are valid or not, which helps DOE to make go/no-go decisions and not spend its limited resources on unproductive candidates. SwRI® also reported at the AMR some of their own in-house and collaborative studies on potential hydrogen storage materials. This work is useful to the program, even though the recent materials do not appear to have the ability to meet targets.
- The need for standardization is great.
- This project is focused on establishing a standardized and robust testing procedure for hydrogen storage research. This is an extremely valuable element of the DOE program because reliable, reproducible results are essential for progress to be made in achieving DOE's goals and objectives.
- This effort indirectly supports DOE targets and plans, especially in the properties of weight, volume, thermodynamics, and cycle life.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Generally good— This reviewer is always somewhat confused by what proportion of effort is expended between DOE required samples and their own samples that they test and if one impedes the pace of the others. The PI should make a statement on what the throughput limitations for the laboratory are.
- Over the years, SwRI® has set up their analytical capabilities to assess the hydrogen storage capacities by both gravimetric and volumetric methods as well as to establish capabilities to prepare some types of materials and perform complementary measurements. For future work, SwRI® should make available to the general hydrogen research community via webpage links to their complete analytical capabilities and measurement procedures.

This may help to better standardize characterizations by other research groups - note the wide discrepancies for sodium aluminum hydride isotherms shown on slide 11.
- The basic philosophy of this project is to provide a national source of testing, verification, and other contributions. This is clearly important.
- The work appears to be a mixture of DOE-directed efforts, international (DOE-supported) support, private jobs, and SwRI® work. This is an acceptable approach if DOE contractors cannot provide an adequate number of samples to maintain a full-time presence at the facility.
- This project would be better if it focused more on identifying what results are accurate, what measurement techniques are needed to ensure accuracy for these kinds of measurements, and how differences in reported data can arise.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Progress can't be judged by number of samples tested at this point, as it can only progress as fast as they come in. Some samples need significant equipment modification and preparation time. Perhaps the PI could give an idea of how many samples are in the queue and what the average throughput rate is and is there a chance for improvement?
- The PI has done a great job adjusting test equipment as necessary to accurately understand and measure new materials.
- SwRI® either completed or is conducting the verification measurements requested by the DOE during fiscal year FY 10. It would have been good to see the status of measurements that were scheduled to be finished by June 2010 as shown on slide 18.
- The only DOE-directed testing at SwRI® in 2010 has been to evaluate the effect of a piezo-induced charge on hydrogen adsorption in nanoporous carbon. The results indicated that there was no measurable increase in hydrogen adsorption from the charge accumulation, at least under the SwRI® test conditions. These results tend to refute the claims by Michigan Technical University. The cause for the different results was not presented.
- There are inconsistencies in the pressure concentration temperature (PCT) data in the novel efficient solid storage of hydrogen (NESSHY) round-robin testing that indicate more work is required to develop standard guidelines for experimental procedures to ensure that results from multiple institutions can be compared.
- The results shown do not bring us closer to meeting DOE system targets.
- The internal work does not appear to have much relevance to the DOE program objectives.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The project team is working with the appropriate partners. The PI would be encouraged to maintain an open dialogue with original equipment manufacturers (OEMs) and other experienced experts in measurements.
- SwRI® has interacted mainly with members of the Hydrogen Sorption Center of Excellence (HSCoE) for DOE. They have had a least a couple of external collaborations on other possible storage materials. If SwRI® is to be the DOE source for standardization of hydrogen capacity measurement, the reviewer would like to see an indication of close cooperation with Dr. Karl Gross, who has compiled extensive documentation and reviews of the requirements and limitations of obtaining reliable results. This reviewer does not know whether this has occurred in the past or is planned for the future.
- Given that SwRI® is also having an internal materials program, i.e,. paladium/silica spill over, it is recommended that SwRI® work closely with other groups within the HSCoE to avoid any overlap.
- By nature, the HSCoE is extremely collaborative.
- This project has a very good collaborative research portfolio.
- The standardization and round-robin testing should have a positive impact in reducing error in the results obtained in different laboratories. The round-robin results indicate that independent measurements are not completely comparable; more work is needed to sort this out.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The project team should continue to test samples as they come in.
- A significant portion of the SwRI® efforts are at the specific requests from the DOE to provide independent measurements on an as needed basis. Hence, this work load can be very variable and did not seem to be very large in FY 10, but there was no indication of what will be needed in FY 11.
- SwRI® also has an internal materials program, and, therefore, it is recommended that: 1) they continue to focus on materials diatomic hydrogen sorption measurements, and 2) they focus on spillover mechanistic understanding rather than materials synthesis; i.e., collaborate with the synthetic groups within the HSCoE for materials preparation.
- Excellent concentration on technical goals.
- The project will end in 2011. Final samples are expected from the materials CoEs. It is important that the results are conveyed to the HSCoE in a timely matter.
- It is not known how many samples will be sent to SwRI® for analysis by the CoEs as they wrap up and prepare final reports. This could result in SwRI® running short on funds before all of the samples of the "best" materials can be tested.

Strengths and weaknesses

Strengths

- SwRI® has, over the past several years, established a laboratory capable of evaluating a wide variety of hydrogen storage materials. They appear to provide timely response to DOE for requested services.
- Performance of some independent research on hydrogen storage systems is useful to broaden the experience of staff and to extend the range of capabilities.
- Analytical measurement capabilities are good.
- The project is addressing a critical need for standardization.
- Standardized testing procedures are essential to good research on hydrogen storage materials, and this project is all about that.
- The concept of having a national testing laboratory is, in principle, sound and valuable to DOE and the entire storage materials community.

Weaknesses

- Other than some contact with the HSCoE groups, SwRI® has not seemed to be come involved with most other DOE hydrogen storage projects. More open communication would have been beneficial.
- It was not clear whether measurements performed at SwRI® were being subjected to oversight from other highly experienced individuals on hydrogen storage materials and the state-of-the-art measurement techniques used elsewhere.
- Collaboration with others with regards to the spill over materials research.
- None.
- It is not clear if the results so far have improved the general, national, and worldwide accuracy of testing.
- There seem to be a number of independent activities being pursued without clear justifications other than that the DOE storage projects are not providing a sufficient number of samples to fully utilize the facility.

Specific recommendations and additions or deletions to the work scope

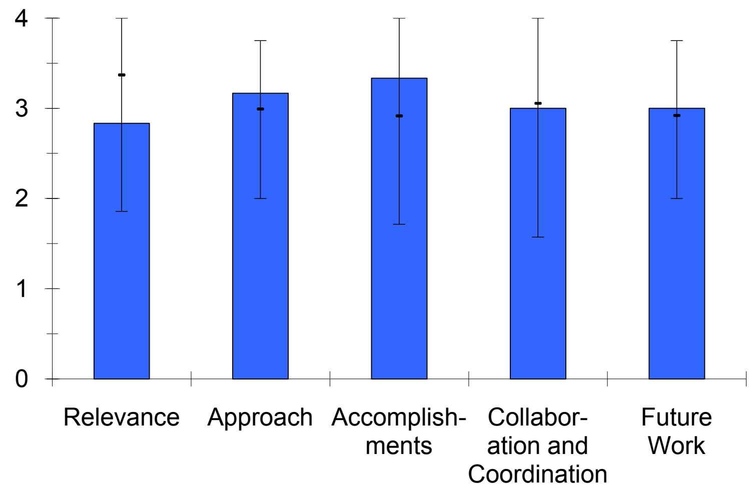
- It is recommended that a more or less formal review be held of SwRI®'s current capabilities and recent results. It is not that there are any expected deficiencies with the SwRI® work, but a peer review would serve to confirm their capabilities and potential value to DOE and outside groups involved with hydrogen storage materials. It is also suggested that SwRI® contact the DOE/EERE Engineering CoE to see if SwRI® could provide some of the additional physical and chemical properties needed for comprehensive modeling over the next few years.
- Keep the project as data validation is very important.
- The project will be ending soon. The physical state of the facility needs to be assessed to determine if it is worth maintaining beyond the end of the program.

Project # ST-55: NaSi and Na-SG Powder Hydrogen Fuel Cells
Michael Lefenfeld; SiGNa

Brief Summary of Project

The objectives for this project are to: 1) demonstrate enabling hydrogen storage technology suitable for early fuel cell (FC) market applications with high volume potential; 2) demonstrate the benefits of sodium silicide technology in a push-to-start hydrogen release system; 3) develop a demonstration system capable of ~250 W for applications such as battery rechargers, remote telecommunications, emergency responders, backup power, and personal mobility, i.e., scooter, bicycle, etc.; and 4) improve hydrogen yield and maximize water utilization for sodium silicide-based hydrogen release.

Overall Project Score: 3.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Sodium silicide technology is well matched to DOE goal of FCs for portable and back-up power applications.
- Although not directly focused on automotive transportation, the project is very relevant to the DOE objectives and will contribute to the development of niche markets for hydrogen energy technologies.
- The project uses a hydrogen source that is suitable for portable and stationary FC systems.
- This project is an example of an "off-board" regenerationable chemical hydrogen storage that releases diatomic hydrogen by hydrolysis of sodium silicide and similar compounds via an exothermic reaction. It offers the ability of using interchangeable "containers" for near-term applications of moderate power, i.e., < 1,000 W).
- Hydrogen release is irreversible, so this technology is not suitable for vehicles and will not meet vehicular targets.
- The project is not relevant to automotive FC application.
- The project is applicable to hydrogen storage for portable power and consumer electronics, but is not aligned with storage targets for automotive applications. Reviewer is not sure how FCs in portable power and consumer electronics do much toward the overall goals of reducing greenhouse gas (GHG) emissions, reducing petroleum consumption, and reducing energy use.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The project has excellent and clear objectives and methodology. The work plan is focused on the objectives.
- Early stage prototypes provide opportunity to assess behavior in various demonstrations.
- The project has a well defined approach and execution.
- The approach is systematic and well developed, from concept development to hardware development and testing.
- The project adresses the barrier of power density for consumer electronics, and has also addressed system issues and start-stop issues.
- The approach to the project is methodical and designed to address technical barriers, except for the cost issue that was absent this time (and was present in 2009).

- The presentation is more like a marketing document than technical summary of issues being addressed to improved capability and performance of this storage system.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Excellent progress has been made with an eye toward commercialization; the work has been field tested.
- The project has developed and fabricated prototype storage devices for operating FC power units.
- Investigators have developed and tested a number of small to large demonstration systems. Investigators have clearly demonstrated a suitable technology for an early FC market.
- A hydrogen release system was developed, tested, and verified. No adverse issues were identified.
- The project has developed a chemistry and designed a delivery system that can provide diatomic hydrogen cheaply to portable electronics.
- The pace of progress seems good for the length of time since the project's inception. The second generation prototype seems ready for ramping up to first scale production quantities. The remaining time to the project's conclusion in 2011 seems reasonable, as long as production commences soon. The project has focused good attention to FC performance needs.
- It is not clear what level of materials assessment has been done in this project, which has a substantial amount of DOE funding (more than $2.4 million in 2 years).
- There does not appear to be any efforts taken to address energy and cost requirements to regenerate spent fuel.
- Potential safety issues with excessive temperatures or pressure generation during the hydrolysis reaction in the storage bed seemed not to have been considered in sufficient detail.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- There is a good collaboration with an FC partner, Trulite. The "back and forth" during development seems to have led to a robust prototype.
- SiGNa Chemistry has two subcontractors who are apparently making useful contributions. There are nice collaborations with industry and universities.
- There are good collaborations with Trulite Inc. and the University of Texas at Austin.
- The project team is collaborating with TruLite.
- Collaborations with a personal computer (PC) maker or electronics maker would be good (such as Dell, Apple, Samsung, Sony, Motorola, etc.).
- The depth of collaboration with the University of Texas did not come out in the presentation nor in discussion.
- There does not appear to be any formal or informal interactions with other organizations involved in chemical hydrides or metal hydrides.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- There is a good focus on improvement, validation, and technology transfer.
- A general plan for improving design of storage system and material properties to increase storage capacity is reasonable. However, there was little insight into how SiGNa Chemistry intends to accomplish this.
- Investigators have a clear plan to improve hydrogen yield and reduce water requirement.
- Proposed future work is sound. This project will conclude in 6 months.
- Plans to look at higher diatomic hydrogen density materials are scheduled. The project team should look some at the recovery of spent material, and the comparisons of the environmental impacts of disposal of the canister versus battery disposal.
- A 9.8% pure material hydrogen content is relatively low, and even lower relative to entire system. Reducing the excess water needs are still an important objective to address.

Strengths and weaknesses

Strengths

- Although not directly focused on automotive transportation, the project is very relevant to the DOE objectives and will contribute to the development of niche markets for hydrogen energy technologies.
- This project takes a feasible chemical storage approach using hydrolysis of silicides to release hydrogen.
- Fabrication and operation of prototypes should lead to advances in system designs and optimization.
- Investigators have demonstrated a low weight, high purity, and controllable hydrogen source for portable and stationary FCs.
- The project team has excellent technical experience.
- The project pursues high energy density storage that beats batteries with a low cost.
- Their project team made a good choice for a hydrogen storage media. The hydrogen release dynamics seem well fitted to FCs and ultimate application characteristics. The disposal of spent cartridges does not appear to create undo issues. The project has applied good technical resources to the effort.

Weaknesses

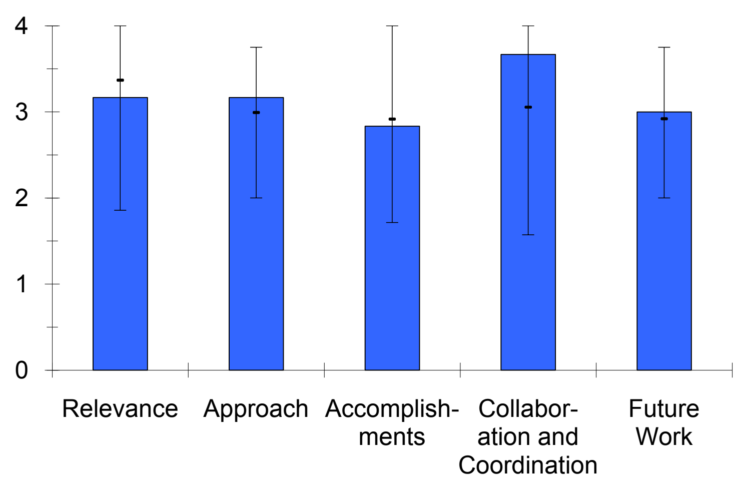
- The estimation of overall energy efficiency (although this is not critical for the likely niche applications of this technology).
- The current prototypes seem to have gravimetric capacities of no more that 2-3 percent weight (wt%), which is well below the DOE targets for vehicles, although probably sufficient for other early market applications.
- Control of reaction temperature and pressure during reaction seems difficult using the simple pumping of water into the storage container.
- The costs of the complete storage systems seem to be rather optimistic.
- Little or no attention was given to what to do with the vessels with spent fuel or how to regenerate the decomposition products.
- The primary concern is with what to do with the used canisters in the larger (stationary) systems. The final product has a very high pH and could be dangerous if exposed. More safety tests are clearly needed to ensure the canisters are safe.
- It is not really clear what will happen to the material in the larger canisters after it is used (presumably the smaller canisters will be thrown away). What type of recycling program will be used?
- Not applicable to automotive application.
- Waste product is corrosive—high pH material.
- No weaknesses evident.

Specific recommendations and additions or deletions to the work scope

- I recommend more attention be given to safety issues of the vessel along with handling and processing of the spent fuel.
- More safety tests are clearly needed to ensure the canisters are safe especially after releasing hydrogen.
- It may be worthwhile to consider a recycling program for the larger canisters.
- It would be good to have information on system pricing estimate. Ultimate commercialization depends on performance, acceptance, and pricing, among other things.
- It would be good to have information on performance data for an FC when coupled to load and to discover how an FC plus application performs under real life conditions.

Project # ST-92: SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Metal Hydride and Adsorbent Systems
Ted Motyka; Savannah River National Laboratory

Overall Project Score: 3.1 (6 Reviews Received)



Brief Summary of Project

The objectives of this project are to: 1) collect media property data for metal hydrides (MHs) and adsorbents, 2) collect operational data for storage systems, 3) develop a general format for models, and 4) develop an "acceptability envelope" of media characteristics based on 2010 and 2015 DOE technical targets.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The objective of this project is significantly important to investigate system utilized hydrogen storage materials.
- This project by the Savannah River National Laboratory (SRNL) is their technical contribution to the Hydrogen Storage Engineering Center of Excellence (Engineering CoE), which mainly addresses the behavior of metal hydrides and carbon adsorbents. All of the performance issues necessary for optimizing storage potential for these materials are being addressed for the candidate materials sodium aluminum hydride and activated carbon.
- This project is consistent with the objectives of the Hydrogen Program and fully supports DOE research, development and demonstration (RD&D) objectives.
- The overall relevance of the project aligns well with Hydrogen Program objectives, assuming that the Materials CoE can continue to support the program.
- This project addresses the prototype system requirements for materials-based hydrogen storage systems. It is crucial for the transition of such systems to actual vehicular applications.
- Since there are no metal hydrides or otherwise hydrogen storage materials that meet DOE hydrogen storage material targets, there is no real justification for conducting this work with the objectives given here. DOE funds are better spent on supporting smaller projects that address material level problems–instead of conducting a hypothetical system-level optimization effort like this one.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The presentation clearly shows the methodology to reach the goal.
- The SRNL project is closely integrated within the Engineering CoE to address the issues needed to improved storage behavior of metal hydrides and carbon sorbents. Essentially all critical barriers are being addressed, although they cannot be met with the available candidate materials. However, the concepts and methodology should be applicable to other materials with hopefully better intrinsic properties.
- Selection of near- and mid-term MH candidates for engineering development is sound.
- Compiling metal and adsorption hydride data is critical to modeling efforts.
- Developing an "acceptability envelope" of hydride properties provides a great pathway for the downselection of materials.
- The perception is that some of the tasks defined in this project may be redundant when compared with those of other Engineering CoE participants.

- The "acceptability envelope" is an excellent methodology for the optimum up-selection of MH and adsorbent materials at the end of Phase 1.
- The PI's approach lacks the core premise of having a viable hydrogen storage material at hand. Without a good MH based diatomic hydrogen storage material, the approach proposed is of little relevance.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- At present, looking from the "spider chart," there is no appropriate material to meet any DOE system targets. There are no tactics to find out candidate(s) of media for prototype testing.
- Substantial amounts of materials properties have been gathered and assessed for several candidate storage materials and these results were incorporated into detailed thermal performance models. Predictions were compared with literature bed configurations and laboratory testing data. Requirements for hydride and adsorbent beds were developed. It was shown that current candidates are unlikely to meet DOE targets.
- Resources devoted to sodium aluminum hydride may be wasted since sodium aluminum hydride has no chance of becoming a viable diatomic hydrogen carrier due to very low system capacities that will not meet DOE targets.
- Development of the kinetics, heat, and mass transfer models are on target.
- Progress on technical accomplishments appears to be on track; however, no detailed schedule was provided. The "refined" model for a physisorption storage system needs further improvement when compared with the measured thermal profiles.
- A great deal of progress has been made.
- It is unclear what is being done to address the sodium aluminum hydride system shortfall issues associated with gravimetric density, cycle life, safety, and toxicity.
- The objective of building databases for the failed diatomic hydrogen storage materials: sodium aluminum hydride, lithium amide, magnesium hydride, titanium chromium manganese, or the magnesium alloy Mg_2Ni is not clear. Building databases for compounds has definite value, but is not necessary for this work. A good deal of sophisticated computational fluid dynamics (CFD) and heat transfer modeling is in progress, but their value to the development of a system level hydrogen storage system (that meets DOE targets) from the currently less-than-adequate hydrogen storage materials is not known. For the sodium aluminum hydride that was analyzed as an example of the accomplishments in the present report, researchers point out that 17 DOE targets were achieved and 4 (excluding costs) were not! This reviewer is uncertain of the reason to spend so much money and effort if the result is already predictable and well known. Of course, sodium aluminum hydride cannot meet system-level DOE targets for the gravimetric density, cycle life, etc., because the sodium aluminum material fails those requirements! Furthermore, the arbitrary target of minimum required diatomic hydrogen percent weight (wt%) stored in the material to meet 40% of the DOE 2010 gravimetric density requirement (assuming a 50:50 material to system gravimetric ratio) does not make any sense at all.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- A wide variety of world class experts contributed to the project. In addition, they seem to collaborate closely.
- SRNL has involved many of its Engineering CoE partners in this effort, which resulted in highly coordinated assessments. SRNL also worked closely with several partners from the hydride and sorbent CoEs.
- There are excellent collaborations among the many Engineering CoE partners.
- A very capable team has been established.
- There is an excellent collection of partners. The project appears to be well coordinated.
- There are many entities involved–perhaps too many.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The role of this CoE is not to develop materials, but to conduct engineering research. This project contains only an evaluation of the media. There must be an option to continue material research even under the Engineering CoE, and this project seems to be an appropriate umbrella for material research.
- SRNL has outlined a comprehensive list of fiscal year FY 10 and FY 11 activities to support the Engineering CoE Phase 1 go/no-go review. Alternative candidates and bed configuration will be assessed using the methodology and simulation models developed during the past year. Presumably all DOE system targets will be addressed.
- Proposed future work is very good.
- There is a need to establish a clearly defined test matrix of candidate storage materials based upon the Materials CoE recommendations.
- Future work addresses the key areas.
- The project team should quit building a database of inadequate hydrogen storage materials.
- The project team should provide justification for why it is necessary to develop detailed models to compare storage system behavior for different media (metal hydrides, metal organic framework- (MOF-5) and AX-21) – none of which is acceptable hydrogen storage medium.
- The project team should add cost considerations.

Strengths and weaknesses

Strengths
- They have a clear methodology and a significant number of world-class experts in materials.
- SRNL has established an extensive materials property database to analyze and predict the behavior of storage systems based upon hydrides and adsorbents. This is a well-balanced package of numerical modeling methods with detailed integration of relevant materials properties.
- The project team has strong collaborations.
- The partners have an excellent understanding of the technical work.
- The project has a well defined architecture in the modeling hierarchy toward addressing the Engineering CoE goals.
- This is a very strong team contributing to the project.
- Successful completion of model development will establish the theoretical boundaries of expected performance from any given storage system design and storage media.
- Excellent team. Excellent approach.
- Researchers and scientists involved in this Engineering CoE are capable individuals in their respective fields.

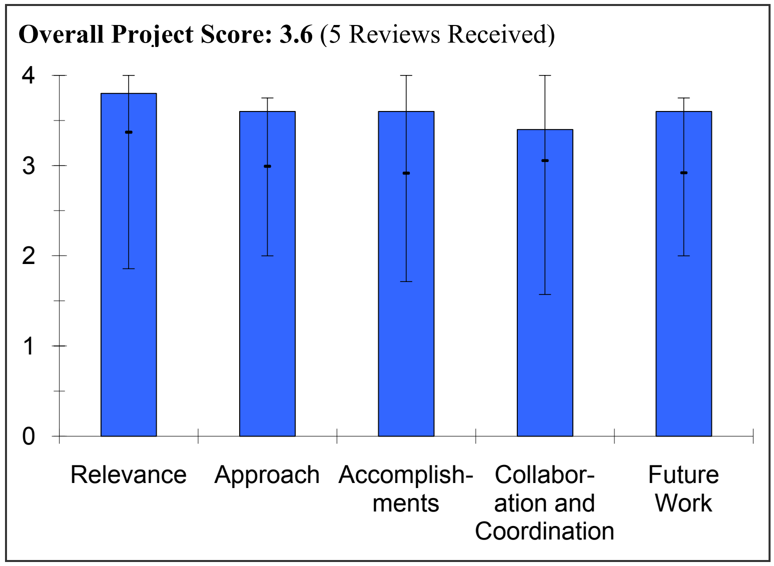
Weaknesses
- The major task is collection and evaluation of material data that have already existed. It is unclear how the risk of not finding an appropriate material will be eliminated.
- The most complete information available for evaluation is for materials that cannot meet the system targets for hydrogen storage materials. Hence, optimization procedures may not correctly identify more effective design/material combinations.
- None.
- Project activities have a high potential for replication of effort, past and present.
- While no detailed schedule of project tasks was given, the success of the project is likely to be constrained by time unless media property data become available very early in the project schedule.
- Software models and hardware designs are based on yet-to-be-discovered hydrogen storage materials with unknown characteristics and thermophysical properties. The lack of cost considerations is a real weakness.

Specific recommendations and additions or deletions to the work scope

- This project needs collaboration with materials research. The results of this study under this project should reflect to the material investigation. Feedback from this project is definitely useful for development of advanced materials.

- There are none beyond those outlined on slide 24 of the SRNL AMR poster.
- Other materials based on the Materials CoE recommendations need to be considered.
- The scope of work needs substantial modification and roll back.

Project # ST-93: High Strength Carbon Fibers
Felix Paulauskas; Oak Ridge National Laboratory



Brief Summary of Project

The overall objective of this project is to reduce the manufacturing cost of high strength carbon fibers (CF) by means of: 1) significant reduction in the production cost of the polyacrylonitrile (PAN)-precursor via hot melt methodology; and 2) later on, the application of advance carbon fiber conversion technologies development at the Oak Ridge National Laboratory (ORNL).

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Extremely high—all storage systems, with the exception of some chemical hydrogen storage materials, will need cost effective pressurized storage tanks. The CF is currently the largest contributor to cost of those systems. It is essential to find low-cost fiber options that will allow the supply to meet the demand of such high strength fibers
- This project is significantly important to reduce the cost of CF that is indispensable for high pressure hydrogen cylinder.
- This project aims to reduce the cost of CFs that are needed for super reinforced tanks for high pressure hydrogen. If successful, this project would have a significant impact on the cost of hydrogen based alternative energy systems.
- High strength fiber is critical for high pressure hydrogen storage tanks.
- This project is important for the commercial success of the hydrogen economy. It is clear the most significant cost driver of a compressed fuel system is the hydrogen tank, and the cost driver of the hydrogen tank is the CF.
- This project is essential to reduce the cost of CFs and to advance the development of CF manufacturing.
- In my opinion, this project is underfunded and needs more support based on the high relevance to the hydrogen program.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- The PI is working on the process to simplify and eliminate impurities (largest contributor to fiber strength and yield).
- The approach to reach the goal is clear, and, if successful, it will provide a novel and less expensive way to produce high strength CF.
- This project involves improving a melt spinning approach to fiber synthesis as a low-cost alternative to conventional preparation techniques. This seems like a very good direction to go in the area of structural CF production.
- Replacing the wet spinning with melt spinning may provide a cost effective way to achieve large scale production of fibers.
- The approach is excellent based on the limited level of funding. The project has a good focus from a manufacturing viewpoint. In future presentations, the results of the fiber attributes, i.e., variability and performance, should be provided in comparison to the goals.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- The PI is making fantastic progress despite the extremely limited funds.
- With only 2 years R&D, great progress has been made, but it is still not satisfactory to apply to real products.
- Progress has been made in the synthesis of long fibers using the melt spinning technique. Additional advances are needed to demonstrate viability of production, especially process efficiency and scalability.
- The program appears to be on schedule.
- The accomplishments for the past year have made a significant step toward realizing the feasibility of the melt spun process. It was nice to see the example of the filament manufactured from the PAN-melt spinning process at the AMR.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- DOE should continue and strengthen collaboration and funding of this project with the FreedomCAR Materials Technical Team (MTT) or US Automotive Materials Partnership (USAMP). Clearly the benefits of this project apply to those teams as well. The PI is leveraging the laboratory facilities with a current MTT project, but needs more people and funding to address the different program goals and intent, i.e., different fiber quality and strength.
- Such coordination could also help achieve a competitively bid status with this project.
- It seems to be difficult to find good collaborator(s) in the United States because high strength CF is produced only in Japan at present.
- This project incorporates several collaborators who bring much needed expertise to bear on the objectives of the work.
- This project has reasonable collaborations.
- The collaboration of partners seems to be well organized and appears to have the correct leaders in the field. In the future, it would be useful to have feedback from the industry producers to ensure the process steps can be scaled and transferred to a large-scale industrial setting.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- The PI is continuing efforts, but is hampered by the budget and lack of staff. This reviewer strongly urges DOE to increase funding for this project and the pace. The payback for this project for many industries and sectors will be many fold the investment.
- When IP is locked in, the PI needs to reveal the source of the "holy water."
- The plan is very clear.
- The next steps in the project are clearly indicated.
- The description of future work is vague and difficult to evaluate.
- The general future work is consistent with project goals. In the future, further detailed next steps would be appropriate to understand the items in the process that could be modified and the predicted levels of improvement in order to achieve an acceptable hot-melt PAN filament/tows.

Strengths and weaknesses

Strengths

- Reasonably-priced high strength CF is produced using unique idea from ORNL.
- Development in this area of high strength and low cost CFs is essential to commercialization of high pressure hydrogen storage tanks, plus there are many other applications. This is important work.
- There is the potential for low cost fiber production.
- There is high alignment to the cost issue associated with the hydrogen fuel system.

Weaknesses

- Collaboration with industry seems to be weak. It may be the time to start close collaboration with industry.
- This is an unusual project since the primary goal is cost reduction. Technical weaknesses, if any, are nearly impossible to estimate.
- As the project proceeds, fiber attributes should be provided.

Specific recommendations and additions or deletions to the work scope

- Collaboration with industry is strongly recommended.
- Additional funding is needed for this important project.
- Further scope should include fiber performance in a matrix structure; i.e., evaluate translation strength.

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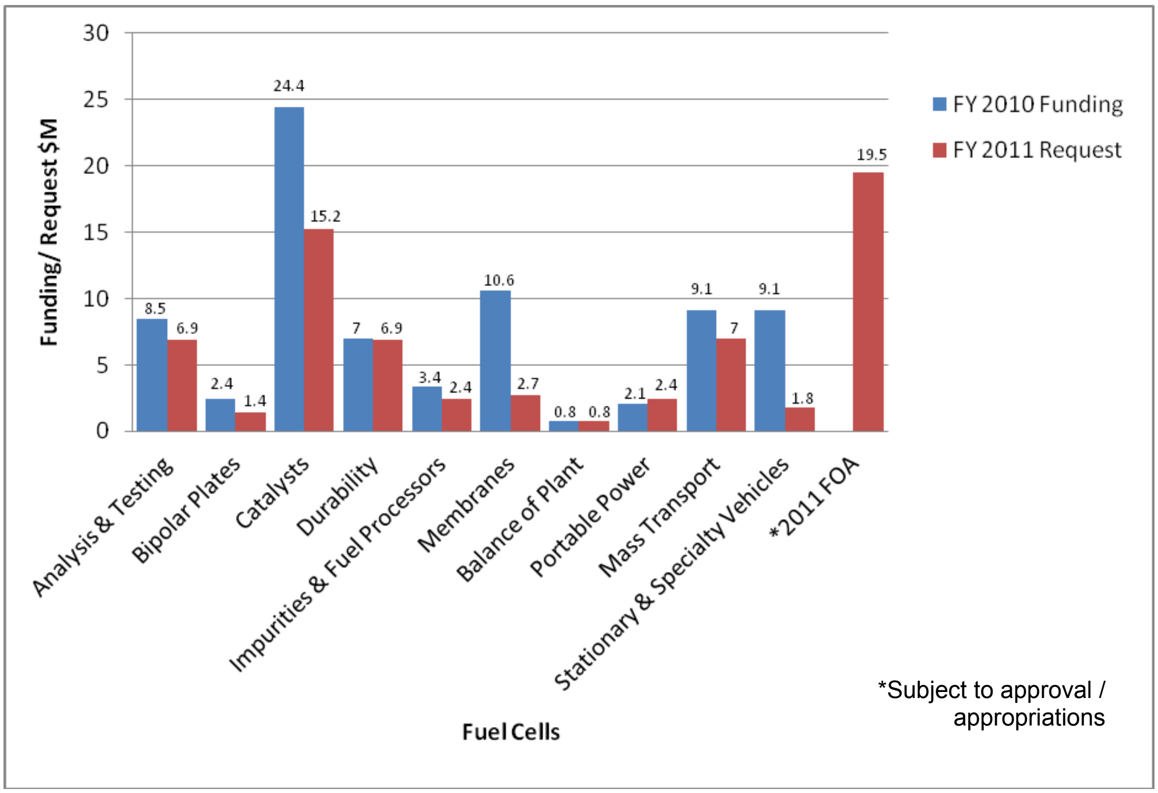
2010
Fuel Cells
Summary of Annual Merit Review of the Fuel Cells Sub-program

Summary of Reviewer Comments on the Fuel Cells Sub-program:

Reviewers consider fuel cell and fuel cell system development to be a critical enabling technology for the success of the DOE Hydrogen Program. The strength of the Fuel Cells sub-program was evidenced by reviewer comments pertaining to the world class scientists working in strong, well-managed teams across industry, laboratory, and university settings. Teams continue to enhance understanding and research, develop, and demonstrate cutting edge technologies that will further enable fuel cell commercialization by allowing competitiveness with incumbent technologies.

Fuel Cells Funding by Technology:

The Fuel Cells sub-program continues to concentrate on the critical path technology of stack components—including membranes, catalysts, bipolar plates, mass transport, and analysis/characterization—as well as system balance of plant components and fuel cell systems for transportation, portable, and stationary applications. Additional projects address cross-cutting technologies, innovative concepts, and the effects of impurities on fuel cell performance. The primary focus of the sub-program is reducing cost and improving durability.



Majority of Reviewer Comments and Recommendations:

At this year's review, 77 projects funded by the Fuel Cells sub-program were presented and 55 were reviewed. All 48 presentations given during the oral segments were reviewed as were seven of the 29 poster presentations. Reviewer scores for the fuel cell projects ranged from 3.6 to 1.9, with an average score of 3.0. This year's highest score of 3.6 was equal to last year's highest. Both the average score of 3.0 and the lowest score of 1.9 for 2010 were higher than 2009's average score of 2.9 and lowest score of 1.6.

	Highest Score	Average Score	Lowest Score
2009 Presentations	3.6	2.9	1.6
2010 Presentations	3.6	3.0	1.9

Forty eight projects were reviewed during the oral sessions by between five and eight reviewers. On average, six experts reviewed each project. Either four or five experts reviewed the seven posters, with an average of just under five reviewers per poster.

Analysis/Characterization: Of the eight projects presented in this category, three were ranked in the top six of all projects in the sub-program. Overall, the average score of 3.3 is well above the sub-program average of 3.0. The analysis projects scored highly in their relevance to DOE objectives and goals. Reviewers encouraged the PIs to collaborate with more OEMs to deepen the understanding of the components that are available and also to allow the OEMs to better understand the models being developed. The cost of an 80-kW automotive PEM fuel cell system operating on direct hydrogen and projected to high manufacturing volume (500,000 units per year) continues to decline, and the cost studies being conducted help elucidate opportunities for greater cost improvement.

Water Transport: Two water transport projects were presented, with an average score of 2.9. A considerable volume of data was produced and a wide range of experiments were performed for model validation. Reviewers were interested in understanding details of the model, such as the accuracy of the parameters.

Impurities: This year the average of the four impurities projects presented earned just over the sub-program average, with an average score of 3.05. Projects were commended for having the unique ability to synthesize and characterize different ceramic supports and for identifying, modeling, and cataloging system contaminants and then disseminating this knowledge to the fuel cell community. Reviewers consider the impurities studies on both the fuel and air sides of the fuel cell to be very important to the success of the Program.

Membranes: Ten membrane projects were presented and reviewed, with an average score of 2.8. The highest ranked membrane project received a score of 3.6 and tied for the highest scoring project in the sub-program while the lowest ranked project scored a 1.9 and tied for the lowest scoring project overall. Projects were lauded for building on proven perfluorosulfonic acid chemistry and improving conductivity and durability while maintaining manufacturability and for addressing the need to provide conductivity in the absence of water. The portfolio of ten projects is especially relevant to the automotive applications the Program pursues, as the development of robust, high-temperature, low-RH membranes is vital to the successful commercialization of fuel cells.

Catalysts: The average score for the 11 catalyst projects was equal to the sub-program average of 3.0. The highest score given to a catalyst project was 3.6, which tied for the highest scoring project in the sub-program and four of the nine highest scoring projects were catalyst projects. The lowest ranked catalyst project scored a 1.9 and tied for the lowest scoring project overall. Reviewers noted the advancements in cathode catalysts and supports and the progress that has been made in thin film electrolyte technology. The required total platinum group metal (PGM) content continues to fall as a result of sub-program research, and higher-risk non-precious metal catalyst development efforts show progress toward mass-activity targets.

Transport Studies: Four transport studies projects were evaluated and given an average score of 2.75, which is less than the sub-program average. The highest scoring transport studies project, with a 3.3, uses multiple approaches to model a single cell and stack to understand key performance cost drivers. The lowest scoring transport studies project also tied for the lowest scoring project in the sub-program's portfolio. The reviewers did not believe that the approach was satisfactory and questioned whether the project was producing transferable value to the research community. The two remaining projects scored near the average, and the reviewers offered many suggestions for how the projects could be improved in approach and future planning.

Degradation Studies: The average score for the five projects concerning degradation studies was 3.1. The projects in the degradation studies portfolio were regarded as having commendable approaches and excellent scientists with the capacity to potentially answer some key fundamental questions about fuel cell degradation mechanisms.

Bipolar Plates: This year three bipolar plates projects were evaluated, with an average score of 3.0, equal to the sub-program overall average. Reviewers were impressed with the new and innovative approach of using highly conductive and ductile aluminum to coat bipolar plates. Reviewers recommended including additional tests to demonstrate performance and corrosion resistance under real-world conditions and for greater periods of time for hardware projects.

Distributed Generation:
The two distributed generation projects received an average score of 2.75. The reviewers praised the project's development of a tubular solid oxide fuel cell and the project's success in leveraging niche markets. However, reviewers were disappointed that a high efficiency PEM fuel cell system had demonstrated lifetime, durability, and efficiency metrics well below DOE targets.

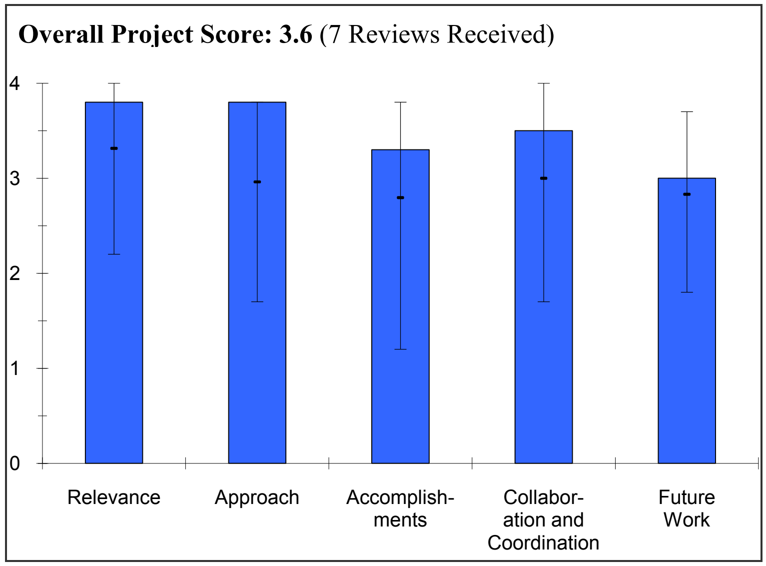
Portable Power:
One portable power project was presented this year and earned a score of 2.7. This project focused on improving the catalytic activity and durability of platinum ruthenium for direct methanol fuel cells. Reviewers noted that the most relevant finding of this project was demonstrating the first stable, high activity catalyst for methanol electro-oxidation.

Innovative Concepts:
Three projects presented this year fall under the category of innovative concepts. The average score of the projects was 2.9. The highest scoring project earned a 3.1 and studies nano-scale ceramic supports as alternatives to carbon supported catalysts, which may result in a more durable cell. A 2.9 was given to a reversible solid oxide fuel cell project that addresses energy storage while pursuing high capital utilization. The third project in this category earned a 2.6 for

an anion exchange polymer electrolyte project for alkaline fuel cell applications, which was believed to have the potential to drastically reduce precious metal usage in fuel cells. The reviewers commended this diverse portfolio of projects for their strong teams and the potential of their innovative ideas. However, they noted that more research should be conducted to better leverage lessons learned from projects conducted previously by other teams.

Project # FC-01: Advanced Cathode Catalysts and Supports for PEM Fuel Cells
Mark Debe; 3M

Brief Summary of Project



The overall project objective is to develop a durable, low cost, high performance cathode electrode (catalyst and support), that is fully integrated into a fuel cell membrane electrode assembly with gas diffusion media, fabricated by high volume capable processes, and is able to meet or exceed the 2015 DOE targets. Focus topics for past year were: 1) water management improvements for cool/wet operation through materials, electrode structure, and boundary condition optimization and understanding; 2) continued multiple strategies for increasing nanostructured thin film (NSTF) support surface area, catalyst activity and durability, with total loadings of <0.25 mg_{Pt}/cm^2 per membrane electrode assembly (MEA); 3) continued fundamental studies of the NSTF catalyst activity for oxygen reduction reaction in general and methods for achieving the entitlement activity for NSTF catalysts; 4) more severe accelerated tests to benchmark the NSTF/MEA durability; 5) development of faster, easier MEA break-in conditioning protocols; and 6) working with system integrators to validate NSTF functional properties or issues in short stacks.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- 3M's work directly addresses one of the major DOE technical areas--catalyst performance and durability.
- They always keep DOE targets in sight and work to achieve them.
- This program remains a lighthouse for direction and accomplishment toward DOE automotive goals of reducing platinum group metal (PGM) content, increasing power, and increasing durability under automotive conditions.
- This is a large, complex project that involves many activities that are focused on the DOE performance goals. The impressive record of meeting and exceeding the DOE goals demonstrates the focus and relevance of the project.
- Efficiency is the key to fuel cell commercialization, and that thrust demands improvements in electrocatalysts and MEAs, which are the main focus of this large-scale DOE project.
- This project is perhaps the most relevant of all the DOE fuel cell projects. Improving cathode catalyst activity and durability is the most crucial task toward enabling fuel cell vehicles to overcome the cost and durability barriers that prevent commercialization. This project has been addressing both activity and durability in a manner that is both consistent with atomic-scale fundamental research and with a material that is amenable to large-scale mass production. If the DOE only funded one fuel cell project, this project would be it.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- 3M uses DOE accelerated test protocols, internal accelerated stress tests (ASTs), and automotive-system-relevant cycles designed to stress 3M's NSTF materials and structures to evaluate progress against DOE technical targets.
- It is not clear whether or not the materials will transition to fuel cell systems.
- Accepted characterization techniques are a strong part of the program. This includes *ex situ* and *in situ* materials and components' performance and durability testing, as well as extensive use of characterization techniques such as transmission electron microscopy (TEM).
- The original design concept has continued to bear fruit and progress. It is good to see that the project has processed 37,000 linear feet of material and also the move to pre-production processes.
- 3M's approach to dealing with such a complex project is exemplary. Breaking the project down into manageable pieces not only facilitates performance, but also helps to explain what is going on to the reviewers. The technical approach is extremely good, although the solution to the flooding problem may have a weakness (see below).
- 3M is a good project lead because they have the ability to move quickly from the small to the pilot scale for evaluation.
- They still have a lot of uncertainty in their methods. This issue seems to be more programmatic and the project would benefit from a more detailed development method. The project is confusing as presented because the methods keep changing.
- The project combines detailed modeling, advanced manufacturing, materials science, and testing to develop new products for contemporary fuel cell systems. Dr. Debe has created a fuel cell culture that continues to evolve and adapt technical advances in real time. An example of this evolution is the inclusion of newer core-shell catalysts.
- This project is the benchmark for all other DOE fuel cell projects in terms of approach. For years, oxygen reduction activity on platinum has shown to be greater per unit area for platinum in bulk-like phases, particularly with preferred surface orientations. NSTF provides for this and, by doing so, has performed excellently with low catalyst loading in mass-produced MEAs.
- The approach of this project has been criticized in recent years for not paying enough attention to the major issues that have prevented commercialization, namely water management and conditioning. However, this year the project has addressed these issues, delivering interesting phenomenological results that may lead to practical solutions.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- 3M has met nearly all of the DOE 2010 targets for single cells and some of the targets for short stacks tested by General Motors (GM). However, not all of the targets have been met simultaneously by a single set of materials and structures.
- 3M continues to make steady progress, and are reaching the DOE goals.
- 3M has met some of the goals only through the use of non-conventional methods, such as requiring sub atmospheric pressures on the anode to assist in water management.
- There are still some concerns over water management as this still appears to be the last hurdle. 3M has suggested a clever approach to use the anode as the water removal element, along with thin membrane and differential pressure. However, the question of what the impact on the automotive system would be if the anode is run at lower pressure compared to the cathode needs to be addressed.
- There has been excellent progress in all areas. The response in regard to the flooding is innovative, but it may have a flaw. Would the flow of water over to the anode not encourage oxygen to reach the anode, thereby leading to increased chemical stress on the membrane? Perhaps the membrane stability is sufficient to stand up to this, but it would be good to demonstrate this.
- The team has made very good progress toward meeting DOE goals.

- 3M doesn't explain why its specific activities are so high (2,500 uA/cm^2, or about 8 times the standard platinum on carbon), but the mass activity has only a 2.5x increase. Where did the activity go? It is not clear whether this a measurement issue.
- The Pt_3Ni_7 seems very interesting, but it's disconcerting that 3M talked up its other manganese-based catalyst so much last year and now 3M is talking up its new catalyst. 3M was not particularly clear about how stable the catalyst was with cycling as nickel is lost or what the loss of nickel into the fuel cell might mean on a system level.
- There is much to admire in the results of this activity. 3M has an excellent, integrated team that has the ability to do many things well and thus retain a leading position. 3M has a long history of making large volumes of coated polymers, and it is obvious that the fuel cell products are part of that technology flow.
- In regard to the major issue--water management--the project has not yet delivered a practical solution. It is very difficult to see how sub atmospheric pressures can be targeted for particular operating modes without adding parasitic loads to the system, which would create more costs for needed components. However, it can be said that the sub atmospheric pressures used were good for demonstrating the phenomenon needed to enhance water management: removal of water from the cathode catalyst layer, particularly towards the anode. From a phenomenological standpoint, 3M reported some good work.
- The development of pure platinum NSTF is very welcome from a processing standpoint. It would be interesting to further develop apples-to-apples comparisons with PtCoMn mass activity and cycled durability.
- Major progress has been reported with respect to dryer conditioning. More progress is still needed to reduce dryer conditioning time from six hours.
- Pt_3Ni_7 is interesting, but it would appear that just a small change in nickel weight percentage would result in a drop in activity.
- Cycling tests (0.6-1.2 V) show major loss in performance over 4,000 cycles, but these cycles are not necessarily equivalent to realistic cycling. This underlines the need for more realistic cycling to be done.
- Open circuit voltages (OCVs) appear consistently low (near 0.9 V or below). It would be interesting to understand more about this, or whether OCV is actually being measured.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- 3M's program involves collaboration with several universities as well as the U.S. auto original equipment manufacturers and some stack integrators.
- 3M is willing to provide NSTF samples to any credible fuel cell R&D entity.
- It is not clear how strong a link there is with the fuel cell manufacturers.
- There is a wide array of partnerships, especially with GM and Nuvera Fuel Cells.
- The presentation showed some of the collaborations and how they were integrated, but did not show all of them, which was probably due to a lack of time.
- It would have been helpful if they could have talked more about their relationship with fuel cell manufacturers, and try to present more non-proprietary results.
- It is obvious that 3M has effective collaborations and is teaming with quality institutions. It is also obvious that the principal investigator has highly effective procedures that have resulted in effective group productivity.
- The Dalhousie collaboration has been particularly valuable toward developing more active alloys, and perhaps even more valuable toward screening out less active alloys.
- Assistance from Argonne National Laboratory (ANL) with activity measurements has also been worthwhile in providing insight into expected activity measurements.
- The National Aeronautics and Space Administration's (NASA) role was not explicitly stated in this year's slides.
- Most other collaborations' results are either mostly unreported or will be expected to deliver more progress in the future. This is especially true in the case of the interactions with the water transport projects (e.g., the Lawrence Berkeley National Laboratory), and in the case of stack testing at Nuvera.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The focus of future work will benefit from further evaluation of catalyst layer structures and compositions.
- Plans include optimizing catalyst compositions, structures, and fabrication processes to meet and exceed DOE catalyst targets on a single materials set.
- There is not much time left. Considering the extensive accomplishments in the past, a focus on stack testing and water management may be more than enough for future work.
- The plan for proposed future work is good in most respects, except for water management. Future work in this area should focus on whether the method of dealing with flooding affects membrane life.
- 3M keeps moving along making incremental, but steady, progress.
- The next act is obviously optimization. There appear to be concerns about water management ("flooding"), break-in details, and perhaps durability. Concerns will happen; the important attribute is to address them, and this is happening.
- It would be good to see some of the details within the categories of future work described. It is unclear whatthe strategy on water management is. They could assume atmospheric anode pressures are acceptable or to attempt to find other routes toward shifting water away from the cathode catalyst layer.
- There was nothing described in terms of the direction for durability cycling. There is still not a realistic drive cycle in the project, and it would be interesting to see if certain collaborations (e.g., GM) would be able to provide results. The results from 0.6 to 1.2 V cycling do not look good, but the cycle is more aggressive than needed.

Strengths and weaknesses

Strengths

- The project is simultaneously addressing all aspects of catalyst development.
- 3M is making outstanding progress on an innovative concept that is fraught with difficulty and requires new understanding of what happens with ultra-low PGM MEAs.
- 3M has a very strong team.
- Their mindfulness of the transition to manufacturing processes is also a strength.
- Even though this is a large complex program, it is being managed well. Breaking the component activities down into manageable parts brings order to what could easily be an out-of-control complexity.
- 3M has the capability to evaluate catalysts in pilot-scale MEAs.
- The project team is making slow but steady progress toward DOE goals
- The introduction of a new Pt_3Ni_7 catalyst with very high activity as demonstrated in MEAs is a strength.
- As with all successful technology, the 3M team is the key to this project's success, and is the most significant strength.
- This project holds the greatest promise toward developing a catalyst that will enable fuel cell vehicle commercialization. The project's approach is entirely consistent with the scientific fundamentals of improving oxygen reduction platinum-based catalyst activity and durability. At the same time, industrial processes for mass production exist.
- The project is well-organized and disciplined. It is able to deliver results each year toward improving mass activity and surface area, and quantifying durability. We can now say the project is on its way to understanding the reasons for water management deficiencies.

Weaknesses

- In one reviewer's opinion, the project has no weaknesses.
- The project has a clever approach to water, but there still seems to be a compromise to force an operation on the automotive system.
- As discovered in the program, water management on the cathode becomes the largest issue and ultimately may deserve the greatest effort to overcome.

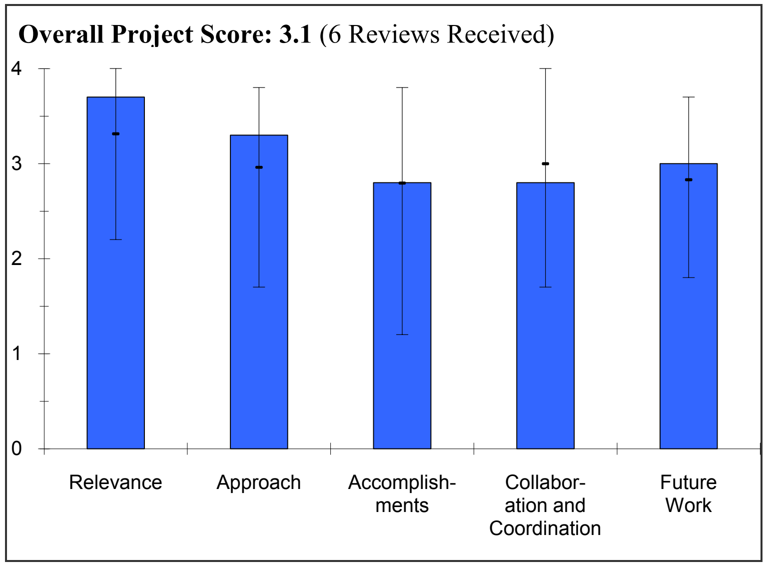
- The methods being used are inconsistent.

- 3M is still battling water management.
- It is not clear if the use of 3M membranes in automotive fuel cells requires extensive reengineering to help with water management.
- The viability of Pt_3Ni_7 is not clear due to the impact of nickel loss.
- The project seems to be dominated by the fuel cell technical team, and its stringent focus on just one technology—light duty vehicles. Today, high purity hydrogen is the fuel, and thus lightly loaded anode catalysts perform well. One suspects that fuel specifications are unreasonable, and "technical grade" hydrogen will change things. The issues of water flooding, durability, shutdown/startup, and freeze protection involve stack design issues. 3M probably needs to be involved with stack technology, because fuel cell commercialization always requires optimization of the catalyst with the reactor.
- Practical solutions are still needed for water management.
- The conditioning time still needs to decrease.
- While NSTF is more durable than platinum on carbon, a proper cycle is still missing that would help toward understanding how performance would respond to vehicle operation.

Specific recommendations and additions or deletions to the work scope

- There needs to be an agreement on the last period of stack testing at potential customer sites and on the hardware.
- There should be more focus on water management.
- Membrane stability measurements under flooding conditions need to be tested, particularly under shut-down conditions where the voltage goes to over one V.
- In order to support 3M commercialization plans, the project should involve a large-scale product-testing activity, shipping products to both customers and developers, as part of a significant market-testing endeavor. This could be accomplished with a near-term commercial project, using, for example, a small-scale combined heat and power (CHP) system. However, the next phase needs to involve thousands of systems under test, and that is probably best done in early market activities.
- There is some "rumor" that the 3M products do not always perform as advertised. This needs to be thoroughly investigated, probably by a "round-robin" or some other focused testing activity.
- The project should remain focused on delivering water management solutions in at least atmospheric pressures.
- The addition of a realistic drive cycle (most likely through collaboration) would be useful to help understand the true durability of NSTF.
- The project should provide some reporting on OCVs. Some polarization curves pique curiosity by at least appearing as if OCV is a bit lower than 0.9 V.
- It would be interesting to know more about how grain sizes of PtCoMn and Pt_3Ni_7 compare (in a fashion similar to what was reported for pure platinum versus PtCoMn).

Project # FC-02: Highly Dispersed Alloy Catalyst for Durability
Vivek Murthi; UTC Power



Brief Summary of Project

The overall project objective is to develop a compositionally advanced cathode catalyst on a support that will meet DOE activity, durability and platinum group metal loading targets in a structurally optimized membrane electrode assembly (MEA) capable of performing at high current density. Tasks include: 1) dispersed alloy catalyst development, 2) core-shell catalyst development, and 3) carbon support investigation.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This project addresses key issues of stability and activity.
- Durable alloy catalysts are essential to meeting 2015 cost and durability targets.
- The project is aligned with DOE goals and targets. It addresses critical issues of cost and durability.
- This is an excellent project that advances state-of-the-art catalyst technology.
- The DOE targets A, B, and C are addressed.
- The project aligns well with the hydrogen program objectives. It is focused on improving catalyst durability and also support durability, with more emphasis on catalyst durability. Catalysts investigated are ternary and core-shell-type alloys.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- It is not clear whether this approach will get to the low platinum group metal (PGM) loadings required by DOE.
- Ther project has a good blend of theory and practice for a difficult core-shell target. As discovered by the team, more time and/or tasks to ink development should have been considered earlier.
- With limited time remaining, they need to combine the highest activity catalyst with the best support and try to optimize MEA activity. It's not clear the value added by modeling work.
- UTC is addressing technical barriers of cost and durability.
- The approach is sound and is yielding good results.
- With the three tasks being dispersed alloy catalyst development, core shell catalyst development, and carbon support investigation, the most prominent approaches are covered. The first two tasks are supported by modeling in addition to experimental investigations.
- The approach is broad, as far as catalysts are concerned, and there is still insecurity in manufacturing of the catalysts.
- The overall approach is good. The project spreads itself thin by focusing on two families of catalysts - ternary alloys and skin/core-shell type. The core-shell approach still has unresolved issues in regard to cobalt or chromium leaching, due to the difficulty of coating the shell completely with platinum. It is not clear why the Pt_8IrCo_2 alloy was selected for carbon support durability tests, even though Pt_2IrCr was found to be more durable. The overall score approach on slide eight (fiscal year 2009) is an excellent way to grade the various catalysts. The improved MEA results for the Pt_2IrCr catalyst are encouraging.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- UTC was able to identify a possible failure mode for core and shell, which is the loss of structural integrity. Is this the link to loss of performance with MEA making?
- Modified carbon (C_4) has shown to experience the lowest loss, but there may not be enough surface area to support the activity goal. One wonders whether the DOE targets can be met. Ketjen black has far higher surface area and thus more corrosion compared to the other standard Vulcan XC72. It's surprising this group did not use the carbon employed for its 40,000-hour life phosphoric acid fuel cell--certainly that has to be a benchmark grounded in actual field testing and results.
- Some activity gains were made on the rotating disk electrode (RDE), but it is unlikely that they will meet MEA activity targets before project completion. MEA durability was not tested.
- UTC has decreased platinum group metal (PGM) loading from 0.8 g/kW to 0.5 g/kW, which is not at the 2010 target of 0.3 g/kW, but is still a significant improvement.
- UTC has downselected to one carbon support, and has demonstrated improved durability on this support.
- It would be nice to know why the MEA activities are so much lower than RDE activities (for the core shells especially, but also for the PtIr alloys).
- Tests with the fuel cell technical team recommended cycles would be useful for comparison to other projects (0.6V-1.0 V triangle wave at 50 mV/s).
- Excellent progress has been made and should be encouraged to continue.
- The 2010 DOE targets are not fully met for topic 1. In particular, cycling durability investigations are still to be performed. A corrosion-stable carbon material has been downselected; yet corrosion tests are to be extended to 5,000 hours.
- The project has made significant progress toward DOE goals. However, for the MEA tests, there are significant gaps for RDE results. While the team is focusing on MEA formulation optimizations to improve MEA results, it is not clear if this can be achieved in the time left for the project. The core-shell approach leads to leaching of palladium and cobalt; hence this is a high risk approach.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- It appears the collaborations do not extend beyond the immediate team. Since the core-shell approach is novel, new methods for the verification of structure may be needed and obtained through additional collaborations.
- It's not clear how Brookhaven National Laboratory (BNL) and Texas A&M University (TAMU) partnerships have provided value.
- The good collaborations within the team.
- UTC has very good collaborations in place, including top people at Johnson Matthey Fuel Cells, Inc. (JMFC), BNL, and TAMU.
- The collaboration structure was not shown clearly in terms of concrete results. The synergy of the collaboration is not clear, although the basic work share is outlined in chart 5. It is not clear to the reviewer whether modeling leads to a better understanding of the catalyst systems, and whether that information was taken up by the catalyst manufacturer.
- The team is very strong, with JMFC, TAMU and BNL. The project has very good collaboration, with each team role well-defined, as seen in slide five. JMFC provides the catalyst, while UTC Power tests MEAs (not clear who fabricates the MEAs). BNL looks at core-shell fundamentals. UTC Power also does testing to determine the corrosion resistance of alternate supports. TAMU does computational calculations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The core-shell approach may not have the potential to meet the program's targets. Author cites some stability issues in acid and leaching of transition metals. It seems this would make MEA production inherently difficult. It is unclear how the team would address a final catalyst that either loses activity with leaching or obtains a certain activity with a certain degree of (uncontrolled?) activity.
- This reviewer agrees with the author that, in the final phase, efforts should be dedicated to ink-making/electrode structure to show the potential to meet DOE goals.
- There is no MEA durability planned. With limited time remaining, they need to combine the highest activity catalyst with the best support and try to optimize MEA activity. The performance model is not providing value at this point.
- The project ends October 2010; the proposed work is logical to wrap up the project and accomplish deliverables.
- The project is going well and should continue as proposed by the PIs.
- The proposed work is described at a general level, which does not refer to the status achieved and does not show that the issues discovered will be tackled in a focused way.
- Since the project ends October 2010, the amount of future work proposed appears excessive compared to the time available for completion. The team did not state if the future work will be done by the end of this program, or if some of the proposed future work would be done as part of additional funding.

Strengths and weaknesses

Strengths
- This is a good team with depth--should the core-shell succeed, a commercialization partner is ready.
- They have screened many concepts for alloys, supports, and core shell concepts.
- Others can learn from their alloy work.
- Two strong and renowned industry partners are working together in this program.
- There is good collaboration among team members.
- UTC showed promising results for PtIrM alloy with chromium.
- UTC's ability to scale-up catalysts and generate scale-up data is a strength.

Weaknesses
- There was insufficient time reserved to develop the electrode structures necessary to support these new catalysts and/or carbons.
- UTC has not been able to make an MEA with good activity. There is no MEA durability. Others cannot learn from support work because materials are not disclosed.
- The project leaves the impression that the activities are still somewhat tentative.
- This project is overly focused on its core-shell approach.
- UTC has displayed an inability to get MEA activity comparable to rotating disk electrode (RDE) activity.
- There is a lack of criteria for catalyst selection for support.

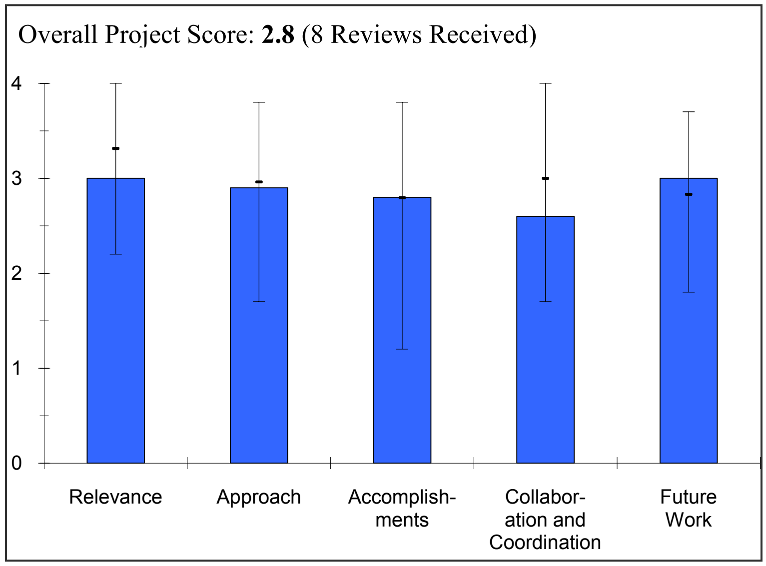
Specific recommendations and additions or deletions to the work scope

- Focus on the ink/electrode structure.
- The project ends soon. With time left, try to combine the highest activity catalyst with the best support and try to optimize MEA activity, test MEA durability, and forget everything else.
- Demonstrate durability of the MEAs.
- There is a need for an academic partner to provide deeper understanding of the catalysts, or a more focused approach of the existing project partners.
- The team should focus on catalysts that work (especially for support durability tests), which is PtIrCr based. It should focus less on the core-shell approach, since leaching issues remain unresolved. The team should test the most stable catalyst with the most stable support before the project is over.

Project # FC-03: Development of Alternative and Durable High Performance Cathode Supports for PEM Fuel Cells

Yong Wang; Pacific Northwest National Laboratory

Brief Summary of Project



The overall project objective is to develop and evaluate new classes of alternative and durable high-performance cathode supports. The objective for 2010 is to demonstrate durability under accelerated test protocols that meets DOE lifetime criteria.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Some of the supports investigated consist of two or more components that may be difficult to use in real fuel cells.
- The relevance is obvious. A major decay mechanism is the oxidation of the carbon support of the cathode catalyst.
- This project's relevance is somewhat limited in that it only addresses alternative catalyst supports, and therefore one major DOE barrier, i.e., fuel cell durability. Also, the overall objective of just two times higher stability than conventional carbon supports is a very modest goal and is subject to the test used to demonstrate it. Also, it depends on the type of carbon chosen as "conventional." Suppliers already sell much more stable carbon supports than Vulcan XC72, for example.
- They have a good focus area which is much needed in terms of catalyst development. Factors in development include understanding and development of the support as it relates to durability.
- The goal is to develop durable supports, which is critical to the DOE goals. It is difficult to determine whether their chosen supports are providing any real value, because of their inadequate electrochemical and physical characterization of the samples before and after durability testing.
- This project has been well focused on the critical issue of catalyst support durability which is necessary to meet DOE objectives and for commercial success of fuel cells.
- The DOE Hydrogen Program seeks to develop fuel cells that can last over 5,000 hours in real world automotive cycling. Historically, one of the barriers to durability has been the corrosion of the catalyst support, which is typically a carbon black (Vulcan, Ketjen black, or a more state-of-the-art graphitized carbon). This project is relevant because it is studying how to replace these carbons with a more stable oxide, or how to protect the high surface area carbon with a stable oxide or carbide.
- The use of carbon in a lot of the project threatens to reduce relevance, but, at the moment, the community lacks definitive public data that would dismiss all carbons as candidate catalyst supports.
- Elimination of carbon substrate corrosion is an important objective for the polymer electrolyte membrane (PEM) fuel cell system to decrease the degradation of the fuel cell performance.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- Although the role of support in a catalyst is secondary, selecting a good support can improve the catalyst's stability and activity. The selection of the systems seems primarily based on considering stability, rather than optimizing catalyst-support interaction.
- The approach is very good. Care should be taken on material selection. A prohibitively expensive catalyst support, while interesting, may be of little value.

- The choice of new classes of alternative supports is very much in line with the literature and there are no surprises. A lot has been studied on these systems already. The innovative concept appears to be to use these inorganic oxides, nitrides, and carbides to protect a graphitized carbon surface.
- The project has a good approach covering major areas of supports--carbon and non-carbon. However, I wonder how relevant the benchmark Vulcan XC72 is as I thought the automotive companies have moved to more graphitic acetylene black standards.
- The impact of support on the structure of the electrode layer in the membrane electrode assembly (MEA), such as porosity and hydrophobicity, is not clear from the approach as described.
- The approach is systematic and the choice of supports is adequate; however, the lack of characterization is a serious deficiency. The synthetic and heating procedures should be adequately described, as they significantly impact catalyst stability. The use of theory is good, but is perhaps premature.
- The PIs overlook the effect of metal-support interactions and carbon wettability in their results.
- There is a good partnership with the right balance of theory and analysis.
- Given the instability of carbon, it can be said this project is looking for incremental gains instead of breakthroughs by focusing so much of the work on carbons. Thankfully, there is some work on oxides.
- While activity and electrochemical surface area (ECSA) results are reported, there has not been enough supporting data from microscopy to explain the results. Reviewers are left to wonder what the particle-size influence on reported activities might be.
- The stability calculations are interesting, but it remains to be seen whether pH and voltage were accounted for, similar to what would be expected in developing a Pourbaix diagram.
- The reporting of mass activity and ECSA is done for Pt/GMC (graphitic mesoporous carbon), but this needs to continue for other catalysts.
- The durability cycle seeks to eliminate convolution with the Pt dissolution/oxidation mechanism, which is fair. However, the stability of the catalysts at lower potentials, which do arise in realistic automotive operation, is still necessary to observe. The FreedomCAR (Cooperative Automotive Research) and Fuel Partnership's Fuel Cell Technical Team (FCTT) has generally avoided specifying accelerated stress tests beyond those that are obviously imperative for Pt and C. However, with alternative chemistries that involve other metals and poor metals (e.g., Ti, In, and Sn), that might have corrosive potential regions at low pH in between immunity and passivation regions, the investigators should take it upon themselves to learn the limits of stability.
- The approach is reasonable, but the comparison to Vulcan XC72 is questionable for a stable substrate. The comparison should be to graphitized XC72 (Vulcite), which is more stable than Vulcan XC72, and Vulcite has been used commercially in fuel cells.
- Testing at 1.3 V or 1.4 V is a severe test. I did not see a correlation of this "accelerated test" with actual test results.
- I did not see mass activity or specific activity in plots for Pt/WC. Is the platinum coverage is the same for Pt/C and Pt/WC-C?

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The stability of Pt-based catalysts is apparently improved with several substrates investigated, but, occasionally, only marginal improvements were found.
- The activity of new catalysts is quite modest and Pt mass activity for the best samples is lower than that of Pt/C by E-TEK.
- The accomplishments to date are very promising.
- This project has achieved progress toward its milestones, although they were not too difficult to start with.
- The mass activities at 900 mV on slide 15 seem to be quite small compared to state-of-the-art materials (45 A/g_{Pt} or 0.045 A/mg versus the target of 0.44 A/mg).
- The percentage of retained ECSA should be compared to state-of-the-art Pt/C catalysts sold by Tanaka Kikinzoku Kogyo (TKK) for example, not just the old Pt/VC.
- The concept of stabilizing graphene with indium tin oxide (ITO) modification is very interesting and potentially useful. There were good results and work done to optimize it. Fundamental studies of why it works were also very good.

- ITO-mediated graphene is a good facilitator of catalytic site dispersion on the support, and there is good analysis to show why.
- The project has shown an increase in performance (activity and durability), but how does this improvement hold up to DOE final goals?
- The project is difficult to evaluate given its poor characterization.
- The density functional theory (DFT) model has predicted stable Pt/ITO/graphene with low activity. It is unclear whether the team has independently determined the conductivity obtained in their method of ITO deposition and optimization. There may be room for much improvement here.
- It is not clear why the team reported rotating disk electrode (RDE) with sulfuric acid. Perchloric acid would be better used alone (slide 12 and supplemental slides).
- The team could attempt going below 0.85 V to see how the various classes of supports fare.
- For the cycle used, a 2x durability improvement over Vulcan was demonstrated for GMC and ITO/Vulcan. However, it is unlikely that a 2x durability improvement will be sufficient for commercialized fuel cell vehicles.
- The investigators make a good point during their discussion of the Pt/carbon nanotube (CNT) work when they mention that MEA durability is different than RDE durability. Because RDE durability can be convoluted with adhesion issues, MEA durability should be used for reported measurements.
- No reporting was given for catalyst layer thicknesses.
- Microscopy data are needed.
- Good progress has been made providing 3x stability with ITO at an initial ECSA of 40 m^2/g.
- All RDE measurements reported were in sulfuric acid. Supplemental slides show that perchloric acid RDE was done. Because of sulfuric acid poisoning of Pt(111), sulfuric acid data should be thrown out, and the perchloric acid data should be those that are reported.
- Oxygen reduction reaction (ORR) mass activities are all below those of conventional Pt/C. Of course, the objective of this project is durability, not activity, but it is fair to comment that the supports studied have not contributed a catalyst-support interaction effect on activity.
- Good progress was made, but some questions remain. The PIs claim no degradation of TiO_2, but the V-I curve shows a loss in performance due to the addition of TiO_2 and it is not clear if there is an advantage. The PIs claim the CNT is "very stable," and this does not appear to be correct. CNT is more stable than XC72, which is a proper claim.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- There are a number of experts in the group of collaborators.
- The collaboration with industry partners is lacking. Collaboration in this area might yield additional materials to consider, political and technical support, and a path to commercialization.
- There were no specific slides on the coordination or functionality of the collaborators beyond the overview slide. Based on that, it appears most of the work was done by the Pacific Northwest National Laboratory.
- More industrial collaborators are needed. The Automotive Fuel Cell Cooperation (AFCC) is only a consultant, while a catalyst company or experienced partner such as General Motors may make a difference in the program.
- The PIs could do a better job getting input from industry on relevant supports by reaching out to the DOE community for help with characterization.
- Good guidance of experimental work by theory.
- DFT predicts good ORR and stability for Pt-VC(111), yet this is not borne out in testing. No reason was given for why.
- The project has been light on reporting fuel cell testing this year (with the exception of some ECSA for Pt/CNT and Pt/ITO/graphene catalysts), so the guidance from AFCC has probably not been extensively used.
- In general, the PI needs to take better advantage of collaboration or it needs to provide more explicit reporting of collaborations. Perhaps ORNL could be leveraged for more microscopy results.
- The partner list looks good; however, it is unclear which contributions came from each of the partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The focus of future work should be selecting the most active, simple, and stable systems.
- The future work plans are very good.
- Focusing on the ITO-treated graphene system would seem to be warranted given its novelty and use as a model for a new type of alternative support.
- More work on how the ITO contributes to stability is needed. It is unclear whether ITO is a facilitator or a direct contributor to durability.
- The PI should plan for ink development, with respect to compatibility, to make key porosity and hydrophobicity targets for the electrode layer in the MEA.
- I think more work with conductive metal oxide-modified CNTs would be important.
- Given the greater durability of CNT and GMC, it is surprising that ITO and WC have not yet been supported on these carbons. Thankfully, this is identified in the future work.
- MEA tests on Pt/GMC and Pt/ITO/graphene are worthwhile to see if the durability is actually greater than two times. MEA tests, of course, are better than RDE for this measurement.
- Further work to improve non-carbon materials (such as Pt/ITO) is good.
- More emphasis needed on microscopy and catalyst-layer thickness. Future work should also include cycling at lower potentials. So long as MEA mass activities are reported, there will be expectations that hydrogen crossover will be verified to be low.
- "Study carbon degradation under more severe and representative conditions" is stated in future work plans, but it is not clear why these conditions are not representative. The exact solubility of metal oxides in water was never touched upon, and neither was the effect it would have on the stability of the membrane. It is hard to understand why 1.4 V was so representative. There don't seem to be many instances of a fuel cell reaching 1.4 V. It is also unclear whether the mechanism for carbon corrosion is the same at 1.4 V as it is at 0.7 V.

Strengths and weaknesses

Strengths

- The project has a good team and good facilities.
- The project has well-defined concept and material selections.
- The project seems to have a good rate of productivity.
- There is a good connection between theory and practice.
- The project team has shown great productivity and advances.
- High durability catalysts used in this project are of importance to DOE and the fuel cell community.
- There is good synergism between theory and experiment. CNTs have shown good stability compared to the control.
- The project has made progress with a more stable, higher surface area ITO.
- The PI has been one of the more responsive PIs in regard to technical team and reviewer feedback, as evidenced by doing RDE in perchloric acid, adding non-carbon materials to their plan, and removing some nitrides suspected to be unstable.
- The project includes many needed experiments in its future work plan.
- The program is evaluating important substrate materials such as CNT and graphene.

Weaknesses

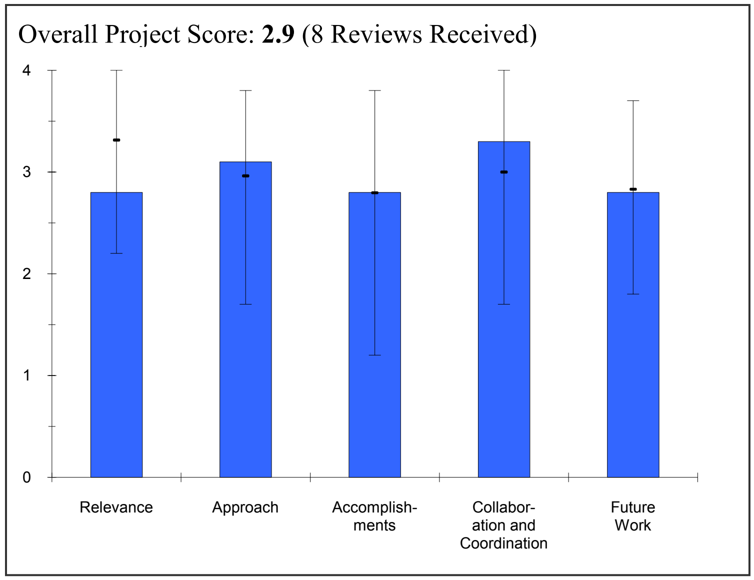
- The activity measurements are not easily compared with the data in the literature. In similar research, 0.85 V is generally not used for comparison; it indicates a low activity of the catalysts.
- There is a lack of a path to commercialization.
- The homemade fuel cell testing, questionable ORR activity values, and questionable (low) MEA performances shown may be compromising the ability to accurately compare the effectiveness of the different supports.
- The PIs are underplanning for ink/electrode structure development using these novel materials.
- Electrochemistry is the only characterization tool, and it is used in a non-standardized format, using H_2SO_4 as the electrolyte. Thus, all of the results are unstandardized and difficult to understand.
- No physical characterization tools are used.

- There is no insight to determine whether there are any metal-support interactions which might affect the catalysis, or how the wetting of the different catalysts might affect their performance.
- I would like to see more work done with CNT, which by far showed the best stability results.
- The project approach was conservative at the outset. Instead of looking at non-carbon supports, the project has sought to stabilize carbon-based supports.
- More microscopy is needed to understand whether trends in activity and stability are affected by particle size, surface orientations, and other nanoscale phenomena.
- Beyond Pt/ITO, the project may not have delivered a catalyst material that would be of interest to the end customers of the research. This is partially dependent on how MEA testing proceeds with some of the materials that have been identified through RDE as two times more stable.
- The results were not reported as mass activity or specific activity. This does not permit adequate comparison from sample to sample or with the work of other researchers. There did not appear to be any collaboration with industry.

Specific recommendations and additions or deletions to the work scope

- The PIs should select one to two systems and optimize them.
- Find an industry partner.
- Evaluate the materials for other technologies, like phosphoric acid. PEM fuel cells use a sulfur-based transfer ion at temperatures up to 120°C. Phosphoric acid fuel cells use a phosphorus-based transfer ion at temperatures up to 240°C. Look at the other technologies opens up the potential commercial possibilities.
- Have Los Alamos National Laboratory cross-test the activity and durability of their best candidate materials.
- Obtain state-of-the-art dispersed catalysts from TKK and compare them to that rather than Pt on Vulcan.
- Use remaining time on ink/electrode structure development.
- The project should not continue without redirection to include extensive physical characterization of the catalysts with techniques such as transmission electron microscopy, scanning transmission electron microscopy, Brunauer-Emmett-Teller surface area analysis, chemical analysis (ICP), and/or point of zero charge.
- Researchers should do their electrocatalyst characterization using standardized methods, e.g., 0.1 M $HClO_4$ References exist in literature and there is significant help within the DOE laboratories to support.
- Test optimized ITO on CNT both in DOE RDE and MEA stability tests.
- Microscopy needs to be added to understand the relationship of nanoscale properties to activity and stability.
- Durability measurements should rely on MEA testing. Reporting of catalyst layer thickness is needed. There should be some reporting that hydrogen crossover was verified to be low.
- Modeling calculations for stability should provide some assurance that results are valid for low pH, high voltage operation.
- Durability cycling should be performed at lower potentials, in addition to cycles being used.
- Reporting procedures should be updated to include mass activity.

Project # FC-04: Non-Platinum Bimetallic Cathode Electrocatalysts
Debbie Myers; Argonne National Laboratory



Brief Summary of Project

The overall project objective is to develop non-platinum cathode electrocatalysts for polymer electrolyte fuel cells to meet DOE targets that: 1) promote the direct four-electron oxygen reduction reaction with high electrocatalytic activity, 2) are chemically compatible with the acidic electrolyte and are resistant to dissolution, and 3) are low cost ($5/kW), 0.3 mg_{PGM}/cm^2)). This year's specific objective is to prepare and characterize model systems (bulk alloys) and nanoparticles of Pd-Mo, Pd-Re, Pd-Ta, and Pd-W binaries and Pd-Cu-Mo, Pd-Cu-W, Pd-Cu-Re, Pd-Cu-Ta, and Pd-Cu-Ni ternaries.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- While I appreciate the need for alternatives to Pt as a catalyst, I am concerned about the perception that Pd is a low cost material. The precious metals market has shown in the past that the Pd price is volatile at best. This means that, given the poor stability and performance of Pd as a catalyst, this approach may not meet the objectives of the RD&D.
- Searching for more cost-effective Pt alternatives has merit; success could be a game changer in light of performance and durability issues with Pt at low loadings.
- My only concern is that the entire presentation was deep into the science without any obvious connection to practical targets being made. The project addresses cost barriers, so a simple parametric study of material/cost/performance tradeoffs would work wonders to frame the work and the talk, and place the candidates discussed in proper context.
- This project directly supports, in theory the major barriers of electrode performance, durability and cost. It focuses on reducing cost by replacing Pt while achieving the same Pt mass activity targets. The widely known negative impact of excess transition metals dissolved into membranes that reduce fuel cell limiting currents is a counterweight to this objective that does not appear to be factored in. Lower high current density performance can have a greater effect on increasing stack costs (by requiring more cells) than would be lowered by replacing Pt with lower cost noble metals.
- The development of low cost catalysts does address the objectives of the Hydrogen Program. A more detailed analysis of the likelihood of this project meeting the DOE cost targets, if it meets technical targets, should be provided.
- Cost and durability are the most critical gaps for fuel cell commercialization and the use of a platinum group metal (PGM) catalyst is a large fraction of the cost. A non-PGM catalyst approach is expected to lower the catalyst cost. However, current performance of non-PGM catalysts is far from a performance target that could replace the PGM catalyst.
- DOE shows a catalyst target and requires measurement of its performance in the membrane electrode assembly (MEA), regardless of whether it is PGM or non-PGM. This project should follow this target and metric.
- The team needs to enact relevant, basic research to determine whether d-band effects with alloying of Pt are admissible for other metals, such as Pd.
- This project is focused on the DOE R&D plan leading to fuel cell commercialization, focusing on these twin thrusts: reduced dependence on foreign oil (due to reducing overall energy demand by creating more efficient

technology), and environmental concerns. It is safe to say that newer DOE plans will dictate fuel cell technology competitiveness in the global marketplace, and the activity also addresses that!

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- The combined theory and analysis is an excellent approach.
- I appreciate the approach to use bulk alloys, as this has been missing for a long time in working with alloys. The progression from this to dispersed catalysts with complimentary calculations is well thought out.
- Strong team competencies were revealed in a sophisticated approach from molecular theoretical calculations to innovative and informed material production methods.
- This project's fundamentals approach is very good for generating basic information on the likelihood of ever engineering multi-material compositions that can effectively simulate a Pt surface.
- The PIs have a very strong team with all the basic elements represented: theory, synthesis, characterization, model system study, and functional testing.
- New Pd alloy materials are being developed. Computational studies, nanoparticle synthesis, and characterization are well focused. Model system work is also a plus. More characterization focused on stability toward oxide formation and dissolution in acid is needed. MEA testing should also be done at this point, as this can help understanding of stability, as well as activity and performance.
- The use of quantum mechanical molecular dynamics is a good idea to help understand oxygen reduction reaction (ORR) catalyst mechanism and materials screening.
- The project team uses a very solid approach to make alloys and characterizes them well.
- The degree of alloying might be better assessed with Reitveld analysis (neutron or high energy X-ray diffraction), although this might be irrelevant as the surface structure is what is most important.
- There does not seem to be any evidence that the bulk alloys will stay alloyed at the nanoscale, and the formation of oxides (for example of molybdenum or tantalum) seems inevitable. If the PIs want to retail alloys at the nanoscale, they will have to introduce a rigorous synthetic approach, and perhaps explore capping the nanoparticles.
- The ANL project appears conventional and is designed to explore the "latest" catalyst, the core-shell, and concept. This direction is necessary and potentially useful.
- The project should continue to focus on bi- and tri-metallic systems that have both the highest ORR activity and show the lowest tendency for surface oxide formation.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- I think that the project as stated is on track. The only concern is that the MEA fabrication and fuel cell testing task is not on track for 2010, yet it does appear to be a deliverable. One wonders if this has been abandoned. It would be the ultimate demonstration and culmination of the project. Perhaps this task should be a focus of the last six months, instead of further fundamental materials research. It would be preferable to be able to do both.
- Extensive calculations, material preparations, and activity tests are good. With that being said, it was not clear why Pd was the pre-eminent specie of choice around which the majority of the work was conducted. Even if this case has been made before, it is critical to educate the audience each time the work is presented, otherwise it begs many questions. This choice would not impress most as a cost-reducing alternative to Pt.
- There has been an excellent amount of fundamental work completed. However, because of its fundamental nature, it is difficult to conclude whether the main DOE barriers will be overcome by this approach. The progress on the Pd_3Mo bulk alloy is promising, but when compared to PtCo or PtNi state-of-the-art catalysts, it shows there is a long way to go yet to equal Pt. The nanoparticle systems based on Pd also indicate the very large parameter space associated with synthesis and the low ORR activity of these best-to-date prepared systems.

- It would be useful to know if any of the Pd-TM alloys studied were perhaps explored decades ago for ORR by more empirical approaches, and if there was any evidence of any activity approaching Pt. These do not seem to be unusual materials to combine.
- The ultimate stability of a Pd skin on a non-noble metal core is also a critical assumption in this approach and could perhaps be validated by alternative types of tests.
- New alloys show that progress can be made. Computational models are providing insight into the structure and composition activity characteristics.
- It is interesting to see an optimized d-band center for ORR activity. It seems to be similar to Pt.
- Progress toward DOE targets for fuel cell catalysts is only fair because this is really a basic research project and the system cannot meet DOE targets. If this project had been an Office of Basic Energy Sciences (BES) project, their progress would have been outstanding.
- Progress is evident and the project (80% completed) is moving forward. The PI should explore techniques for reducing surface oxides on bi- and tri-metallic nanoparticles. This approach will improve ORR results, which should be reported at 0.9 V.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- There are no industrial partners and this means that the project is research-based, rather than delivering to the program. Los Alamos National Laboratory (LANL) has had no input at all, as no MEA fabrication has taken place, nor is it planned before the end of the project.
- The impression is that a lot of work was done with the California Institute of Technology and ANL. A limited contribution from the University of Nevada, Las Vegas and the University of California, Irvine is also evidenced. The role of the Oak Ridge National Laboratory and LANL may be reflected in the content, but it is not apparent, given their central role in studies of this kind. Their contributions and perspectives should have been better highlighted.
- These are very strong team members and their organizational contributions are well utilized.
- The project has an excellent team for computational, electrochemical and *in situ* (MEA) evaluation.
- The collaboration is working. It is too early to include industry partners.
- There is good work being done with theorists.
- It is unclear why the project has not been collaborating with another ANL catalyst group (Markovic, et al.).
- ANL has teamed with competent and supportive organizations. The tasks seem well partitioned.
- They have excellent collaboration with their partners, who appear fully engaged.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The proposed future work seems to be to do more and more research without ever demonstrating that it is worth doing. The demonstration in an MEA is not proposed before the termination of the project. A key deliverable is being dropped. The assumption seems to be that this research may continue indefinitely without a hard deliverable, and perhaps that is the case, but this project must deliver.
- The project should focus on model systems. The evolution of certain analytical and preparation techniques is enduring; however, it is not clear the specific extent to which the team anticipates progress toward technical targets -- this should be quantified.
- In general, the proposed future work extends the most promising routes identified to date. However, it focuses on the surface compositions and less on the surface structure. They may not have identified the key factor for effective ORR by assuming it is related to just d-band center or valence band width or segregation energies. Consideration of surface defects or ideal surface structure seems to be less appreciated.
- There doesn’t appear to be much reason to expect that ternary systems will be better than the binary systems previously studied.
- Testing in MEAs should begin.
- Materials stability is not well defined.

- There is no clear path to forming alloyed nanoparticles. It will be a significant accomplishment if they can make alloyed nanoparticles with Ta, Mo, etc., with no oxides.
- The project is about completed and has produced new literature on structured alloy catalysts. These results support the thesis that this direction is worthwhile.
- Since only Pd_3Ir and Pd_3Os are segregating in the presence of absorbed oxygen, possibly add iridium and/or oxygen to your tri-metallic future work with your alloys showing the best ORR.
- A large increase in ORR is seen in PdRe with only a small amount (95:5) of rhenium added. I wonder if more could be done with this system.

Strengths and weaknesses

Strengths
- Very good fundamental science is used. The approach is sound and the project is well organized.
- The PI and certain team members have experience with the spectrum of work, bridging theory and experimentation.
- The expertise of the respective team members and access to facilities is good.
- The new alloys show that progress can be made. Computational models are providing insight into structure and composition activity characteristics.
- The project is well coordinated between theoretical modeling approaches and empirical validations.
- The project builds upon basic research and understanding of how d-band effects affect ORR.
- The PI is competent and an excellent manager. The integration of theory and fabrication is well done.
- Great computational work has been done. The concept of the project is sound and true advances in ORR over palladium have been reported.

Weaknesses
- The project does not seem to be reaching any conclusion in its closing stages.
- There is little connection to practical technical targets. The project seems to be on the verge of becoming a BES project.
- One weakness is associated with the basic premise for this project, and a lack of a larger appreciation for the economics of the proposed approach. It should be possible to state for given Pt/Pd cost ratios, for example, how much performance decrease with a Pt-free system can be tolerated to compensate for the gain in cost reduction brought by the Pd. That is, from the Directed Technologies, Inc. or TIAX system cost breakdowns and estimates of stack cost per cell, how many additional cells would it take to eat up the cost savings by replacing the Pt with Pd? This would give a minimum performance target on the one hand, that may be less aggressive than with Pt, at least to indicate how far the materials are from a practical standpoint.
- A more detailed analysis of the likelihood of this program meeting the DOE cost targets, if it meets technical targets, should be provided.
- This should not really be an EERE project.
- The transition from bulk alloy studies to nanoparticles was not made clear.
- The core-shell concept has been around for decades: alloy surface modifications have been observed and described. The real issue is durability addressing the utility of these formulations. The project needs to be focused on significant testing of corrosion on these catalysts, which necessarily involves a highly focused testing activity. ANL has shown good ability to do this and should do this, because the tests need to be inclusive. The source of all materials must be known, and the fabrication steps must be done carefully and reproducibly. Then tests need to be completed with sufficient replication to get reliable results. Critical post-mortem analyses, including "tear-down" investigations on samples that are still performing well, need to be done, with continuous sampling throughout the testing period. The effects of the testing regimen, say the voltage scan, on the rate and mechanisms of degradation must be documented.
- I see no obvious weaknesses other than possibly becoming spread too thin and evaluating too many tri-metallics in the time remaining.

Specific recommendations and additions or deletions to the work scope

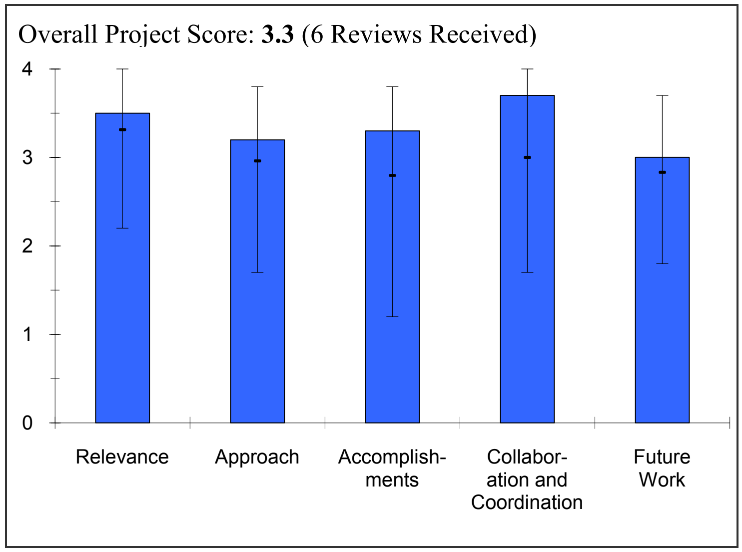
- I recommend that the project take the best that it has in regard to a dispersed catalyst and demonstrate the performance and durability in an MEA before the project ends.

- They need simple analyses framing the targets and progress toward them. These analyses should include materials, costs, performance, and tradeoff calculations.
- The project should justify why they expect ternary materials to perform better than binary materials.
- The project should address the stability of Pd-transition metal alloys in acid media.
- The project should add considerations for how surface structure might affect ORR activity of the materials, not just composition.
- As mentioned, the end target of this project should be consistent with DOE metrics.
- This project should now focus on durability testing.
- I would suggest focusing on techniques that could reduce formation of the surface oxides from the base metals **and then adding a few that show promise for ORR.**

Project # FC-05: Advanced Cathode Catalysts

Piotr Zelenay; Los Alamos National Laboratory

Brief Summary of Project



The overall project objective is to develop oxygen reduction reaction (ORR) catalysts as alternatives to pure platinum and electrode structures suitable for new catalysts that together are capable of fulfilling cost, performance, and durability requirements established by DOE for the polymer electrolyte fuel cell cathode and to assure a path to large-scale fabrication of successful compositions. Project impact in past year has been to: 1) significantly advance the knowledge of factors affecting ORR activity of the platinum monolayer (facet, shape, particle size), and to achieve specific activity and mass activity targets for catalysts with ultra-low platinum content, 2) develop a non-platinum group metal (non-PGM) catalyst with combined activity and durability better than shown in any prior reports, and, after mass-transport correction, meet the DOE 2010 target, 3) demonstrate an active non-PGM catalyst with high durability to voltage cycling, and 4) scale-up the synthesis of selected core-shell catalyst to 20 g per batch.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The objectives of this project align well with the objectives of the DOE research, development and demonstration (RD&D).
- This program is very relevant to DOE R&D objectives.
- This project is of high relevance for the development of PGM and non-PGM catalysts for polymer electrolyte membrane (PEM) fuel cell systems and fully supports the goals and objectives of the DOE Hydrogen Program. The PIs developed a number of core-shell PGM catalysts that significantly reduced Pt content, while not sacrificing the required catalytic activity and stability. The PIs also accomplished industrial scale-up of the first generation of core-shell catalysts. Even more importantly, the PIs demonstrated that activity and stability of non-PGM catalysts are approaching the DOE targets.
- This is a highly relevant project. The electrodes studied were all very interesting and showed promise.
- This project fully supports DOE RD&D objectives.
- The project is very relevant to the Hydrogen Program objectives, targeting catalyst durability, cost, and performance.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- This project is massive ($10 million) and is made up of two seemingly unrelated parts--one with Pt core-shell catalysts and the other with non-PGM catalysts. It is unclear to me why these two catalysts are included together in one project, especially when the research takes place in National Laboratories that are not close.
- This project has an excellent approach, targeting both non-PGM and low PGM catalysts. This approach is very ambitious and increases the probability of success.
- The project's approach is unique, combining many important synthetic and characterization methods for unraveling the most active and stable sites for the ORR. By utilizing this approach, many milestones are fully or

partially completed. Go/no-go decisions are carefully chosen, in particular for the non-PGM catalysts and the methods for peroxide detection in real systems.
- The approach was very good and well thought-out. The PIs were effective at meeting their milestones and goals, as well as leveraging the technical strengths of the various group members.
- This team has been sharply focused on the technical barriers and has done an excellent job in overcoming them to meet the DOE milestones.
- The project has obtained very good results for low Pt loading and non-PGM based catalysts. The agreement between rotating disk electrode (RDE) and membrane electrode assembly (MEA) results is quite good. The hollow nanoparticles approach appears to be quite novel. The increase in high current density results for the PANI-Fe-C catalyst, while quite encouraging, needs further analysis to demonstrate what is going on.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Good progress has been made and some elegant science has taken place on this project. I am concerned that the use of Ir and Pd in the core-shell catalysts is not low-cost as the precious metals market is so volatile. The current price of these metals is not a good guide as to the ultimate cost. Therefore, claims to have met the DOE cost targets, while accurate to the letter, are perhaps not on a path to enable the commercialization of fuel cells in the 2015 timeframe.
- The project has good, interesting experimental results.
- The project has good durability results.
- An understanding of how these alloys work is being developed.
- Technical accomplishments for both PGM and non-PGM catalyst developments are relatively high. For the former systems, the highest activity is observed for the Pt shell-Pd/Ir core, with an improvement that exceeds almost three times the DOE target of 0.44 A/mg_{Pt}. Although the mass activity of such catalysts is significant, the stability issues are not addressed adequately. The PIs should focus more on how to stabilize the Pt shell. The same issue applies for the non-PGM catalysts. It is very important that industrial scale-up of the core-shell catalysts is accomplished.
- The group as a whole showed excellent progress toward their goals, specifically by improving the mass activity of the core-shell catalysts and the stability of the PGM nanoparticles.
- I am a little surprised at the stability of the gold in the gold displacement method, as I wonder whether some of the gold would ultimately end up on the surface.
- The exact surface area of the hollow platinum spheres and whether or not one can access the center of the sphere were questions that did not get answered.
- We have also seen no performance loss in non-PGM iron-based catalysts, even with the loss of significant amounts of the iron. Ithas been speculated that the iron is necessary to form the site but is not needed after the site is formed. It is conventional wisdom that the lost iron would be detrimental to the integrity of the membrane, but I have not seen it.
- It has not been shown that the hydrophobicity changes as a function of voltage, and whether or not that is the reason for the variation of flooding with voltage for the PANI-Fe/EDA-Co-C. To me, it seems that the flooding should have been more severe at 0.4 V.
- It is not clear why sulfuric acid was used as an electrolyte in the PANI-Fe/EDA-Co-C RDE, instead of perchloric acid.
- The project has made good progress toward achieving high ORR activity for core-shell, hollow nanoparticles and non-PGM catalysts. Scale-up has been achieved for core-shell based catalysts. Non-PGM catalyst results look very good. The Br-Br adsorption/desorption approach to improve performance appears to work well. The significant improvement in mass activity for $Pt_{80}Ir_{30}/Pd/C$ is clearly evident from the zoomed plot in slide 7. The progress shown in slide 23 for ORR activity of non-PGM catalysts is very impressive.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- This project seeks to provide funding for a wide swath of national laboratories and achieves this end. The involvement of industrial partners is minimal, and this may affect the scope of the project.
- There is excellent collaboration with universities and other national laboratories. Collaboration with Cabot should also be very important.
- This project is multifaceted, and as such requires a close collaboration between the PIs. Eight partner organizations with highly complementary skills are put together in catalyst development, electrode-structure design, materials characterization, and catalyst fabrication. The degree of collaboration is indeed outstanding and certainly a big part of the success of the entire project.
- The group did an excellent job leveraging the individual strengths of the various group members.
- There has been excellent collaboration with the partners over the duration of the project.
- The project is well coordinated, with the team members' roles clearly indicated. The assembled team is very strong, with each team member a leader in PEM fuel cell development.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- No details are given as to how the proposed work is to be carried out. For example, there is a proposal to "reduce mass-transport resistance in non-PGM cathode" with no indication of how this is to be carried out. This task is not trivial and I would like to know the approach.
- There is a good plan for future research. Understanding stability and/or durability aspects of non-PGM catalysts is important.
- Future research focusing on scale-up of low PGM is important.
- The project clearly builds on past progress and is sharply focused on barriers.
- The project seems to have met most of its goals and is starting to wind down. It is difficult to address future work.
- Future work could include modeling using density functional theory to help demonstrate Pt-O interactions on Pt(ML)/PdIr/C and give direction to other possible metals to evaluate.
- There is a very good outline for future work. Durability issues of non-PGM catalysts have been addressed. Scale-up of these catalysts has also been proposed. The durability of non-PGM catalysts would be a point of high interest.

Strengths and weaknesses

Strengths
- The science is very good at a fundamental level and great progress has been made. This is a very large project and appears to have been managed well.
- An understanding of how these alloys work is being developed.
- The major success of this project is based on the fact that the PIs are developing real systems based on fundamental knowledge of the ORR and enacting novel synthesizing approaches for creating the most active sites.
- Overall, the group had a very good plan and did an excellent job executing it. The PIs worked on very interesting and relevant materials and made significant progress with the materials.
- Excellent results were obtained with the non-PGM catalysts, especially considering the fact that the actual reaction site is still not known.
- The PIs had very good collaboration with their partners.
- There were some very good performance results, especially for non-PGM catalysts.
- The durability results look promising.

Weaknesses
- There needs to be more focus on the *in situ* durability of these catalyst approaches. It is not helpful to make a high performing catalyst that cannot deliver that high performance for the lifetime of the product.

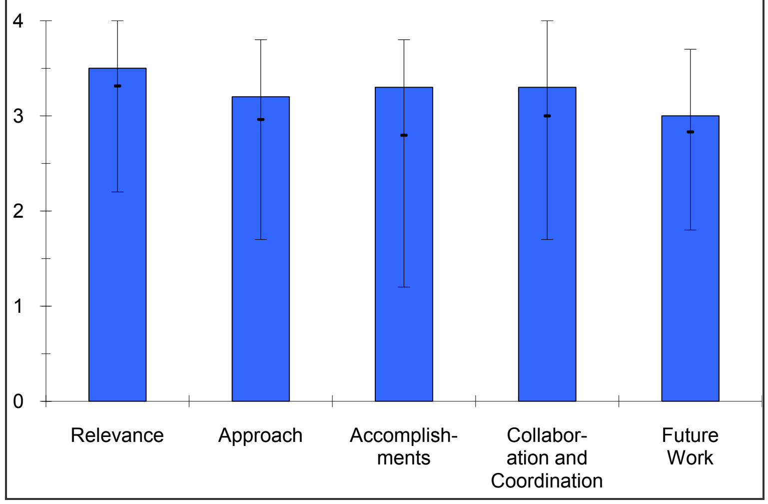
- The weak point of the project is that stability of the catalysts is not fully addressed, as in the case of activity. The PIs should develop a new strategy for handling stability of active sites. This is the key requirement for the development of non-PGM catalysts as well as for preserving high activity of a monolayer Pt shell.
- I see no weaknesses other than possibly optimizing the non-PGM catalyst to an optimized support.
- The project lacks durability information for non-PGM catalysts. It is not clear how PANI-Fe-C catalysts would hold up at 1.2 V.

Specific recommendations and additions or deletions to the work scope

- I recommend that this project be split into two separate projects: one PGM-based and the other non-PGM based. The smaller projects would be easier to manage.
- A cycle between 0.6-1.2 V vs. NHE to determine the durability of catalysts using a MWNT substrate (which is known to be stable) should be used (3M-FC-006 adopted this approach). For ultra-low Pt content catalysts, slide 12 shows a 0.05 mg/cm^2 anode. It would be preferable to use higher anode loading so that the anode is not limiting performance.

Project # FC-06: Durable Catalysts for Fuel Cell Protection During Transient Conditions
Radoslav Atanasoski; 3M

Overall Project Score: 3.3 (6 Reviews Received)



Brief Summary of Project

The overall project objective is to develop catalysts that will enable proton exchange membrane fuel cell systems to weather the damaging conditions in the fuel cell at voltages beyond the thermodynamic stability of water (>1.2 V) during the transient periods of startup/shutdown and fuel starvation, by favoring the oxidation of water over the dissolution of platinum and carbon. Such catalysts are required to make it possible for the fuel stacks to satisfy the current 2010 and 2015 DOE targets for performance and durability.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Fuel starvation is a real concern in automotive fuel cell systems. However, there are other known ways to mitigate fuel starvation via system controls and corrosion resistant supports.
- The project is aligned with DOE Hydrogen Program goals and objectives. It addresses key issues of fuel cell durability.
- This is a very good project that addresses a key area in terms of durability.
- This project has well -performing fuel cells that address a technology relevant to DOE objectives. This program is well aligned with those goals. Obviously, catalyst durability is a key issue, and this work addresses those concerns.
- This project aligns with DOE R&D objectives. It focuses on durable catalysts for fuel cells during transient conditions.
- This project is well aligned with DOE objectives, addressing major cathode durability issue. While the odds for success are not high, a possible payout is significant.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The project has a fundamental approach of modifying both the anode and the cathode catalysts to favor the oxidation of water over carbon corrosion without compromising performance that is solid. I'd like to see the scope expanded to materials that would most benefit from this type of mitigation, such as dispersed carbon catalysts.
- The project is sharply focused on improving durability during start/stop cycles, which is one of the key issues concerning fuel cells for automotive use.
- The project takes a robust approach by looking at two independent methods to alleviate degradation from fuel-starved regions during startup and shutdown (enhance oxygen evolution reaction (OER) at the cathode; decrease/inhibit oxygen reduction reaction (ORR) activity at the anode). Both methods will reduce corrosion.
- The project's approach is excellent and the hypothesis is sound.
- The philosophy here is obtuse. The question now should be "what is necessary to assure contemporary fuel cells survive with acceptable durability," rather than "what is necessary to make contemporary fuel cells 'bullet proof', no matter how they are abused." It is probably worth thinking along these lines. However, the early

- markets are being established now, and other developers have solid, durable products. Indeed, there may be a variety of modern membrane electrode assembly (MEA) designs that prove useful.
- The idea of using an OER catalyst at the cathode to compete with and inhibit carbon corrosion is novel. The idea to incorporate an ORR inhibitor at the anode is also novel. This approach addressed the technical barrier of catalyst durability under startup and shutdown conditions. The scale-up is being done by 3M.
- The success of the proposed cathode approach appears contingent upon solving the conundrum of catalysts for "reversible" fuel cell/electrolyzer systems. It is not obvious what makes this effort more likely to succeed than previous attempts aimed at developing dual-function ORR/OER catalysts for such systems.
- While a "discrete-nanoparticle" approach is, in principle, attractive, ruthenium migration from the OER component to the ORR one, with ensuing formation of a platinum-ruthenium alloy with poor ORR activity, is very likely. It is not clear how such a migration can be prevented, especially over long operating times of the fuel cell.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- In a short time they have already identified several candidates for both anode and cathode catalysts.
- The project has been active for less than a year.
- The project has identified trends in ruthenium-iridium catalysts for OER, and has achieved an initial milestone of 1mA/cm_2 at 1.45 V and 10 mA/cm_2 at 1.5V.
- Results were found that platinum tantalum decreases ORR on the anode by an order of magnitude with no losses in anode performance.
- There has been a limited project performance period thus far, but the work that was reported is very good.
- The approach is thoughtful and novel. The challenge is to reformulate the anode catalysts without impacting hydrogen oxidation. Certainly the amended anodes will react differently to anode impurities, and will complicate fuel quality issues. This all seems to be leading away from commercialization, no matter how interesting it will be.
- Significant progress has been made toward identification and testing of an ORR-suppressing catalyst anode (tantalum based). They have also identified a ruthenium-based catalyst for OER increase at the cathode. Cycling to 1.25V showed little changes; however, at 1.45V, ruthenium starts to disappear, giving rise to platinum behavior. Since the cathode may be exposed to voltages as high as 1.45V during startup and shutdown, this may be a concern. The MEA results for OER catalysts were given, but no MEA results for ORR inhibition catalysts were made available.
- Good progress has been demonstrated to date in simple voltammetric experiments.
- Ruthenium is a questionable choice due to its many stability issues. A promising replacement for ruthenium at the cathode needs to be identified, and both of these points should be raised by the PI at the next AMR.
- Most performance targets appear to have been reached (but only in cells operating for a limited time).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- There is already evidence of valuable contributions from both the Oak Ridge National Laboratory and Dalhousie.
- There is close collaboration between partners. These collaborations worked well in the previous program and appear to be working well here.
- Collaboration with electrolyzer developers may be useful in screening and/or development of OER catalysts.
- The project has a very good team in place.
- 3M has people and facilities as good as any.
- The tasks among the team members are well defined.
- The project seems well integrated among the participants. No collaboration with other institutions has been demonstrated to date.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Future work should include proof that mitigation strategy works under real start/stop conditions and with dispersed carbon catalysts.
- Using 3M nanostructured thin film (NSTF) will allow the researchers to isolate the reactions of interest, while minimizing carbon corrosion. This approach should be even more applicable to improving durability of dispersed catalysts on carbon. Some tests with platinum or carbon would be useful to show how much this approach could help there. Measurements of carbon corrosion in both instances (by carbon dioxide evolution measurements) would be beneficial (microporous layer (MPL) or gas diffusion layer corrosion in NSTF case).
- Future work should include investigating whether there is iridium, ruthenium, or titanium plating on the anode.
- Future collaboration with electrolyzer developers may be useful in screening OER catalysts.
- The work proposed is excellent; however, it would be great if focused efforts were set up to ensure that the added catalyst materials were stable under all operating potentials.
- There needs to be a clear back-up plan, because the described attack seems to involve high risk. Because others seem to be achieving useful durability with their MEAs, 3M needs to sort through its procedures. It is possible that the durability testing protocols are too severe, and that maintaining the MEAs within same potential limits might greatly increase durability. Achieving durability now is very important.
- The future work is well planned and laid out. More details are needed for MEA testing of ORR reduction catalysts at the anode.
- More advanced kinetic studies beyond cyclic voltammetry are needed.
- Catalyst stability is missing in future research.

Strengths and weaknesses

Strengths
- They have exhibited a sound fundamental approach. The targets being set are based on electrochemical fundamentals. There is a nice combination of *in situ* and *ex situ* experimental techniques.
- 3M's experience with durable MEAs and novel approach to limiting start/stop degradation, are good contributions to the project
- The project has a sound hypothesis, an excellent team, and very good preliminary results.
- The people on the project team are impressive.
- The project uses novel ideas for OER enhancement at the cathode and for ORR inhibition at the anode.
- The MEA results have been good for OER catalysts at the cathode.
- Solving a system challenge through electrocatalysis is very attractive and represents a major strength of this project.

Weaknesses
- A weak point involves the fact that this team's strategy is only being applied to electrode design that is already robust to potential excursions. Not exactly a weakness; however, it did not come across that stability of added materials will be examined under all potential ranges (if the PIs intend to do this, then this comment can be ignored).
- The problem is to kill the anode without killing the anode, so to speak. Certainly the explanation was acceptable, and may be correct. However, there is danger along this path: if you make the anode even more susceptible to low level contaminants, the progress may turn to be negative.
- No MEA performance for ORR inhibitor at anode has been provided. (the project is young).
- RuO_2 almost disappears at 1.45V.
- This project is not fundamental enough in its approach.

Specific recommendations and additions or deletions to the work scope

- There needs to be validation on dispersed carbon electrodes.
- Hydrogen peroxide formation results should be reported for these catalysts.

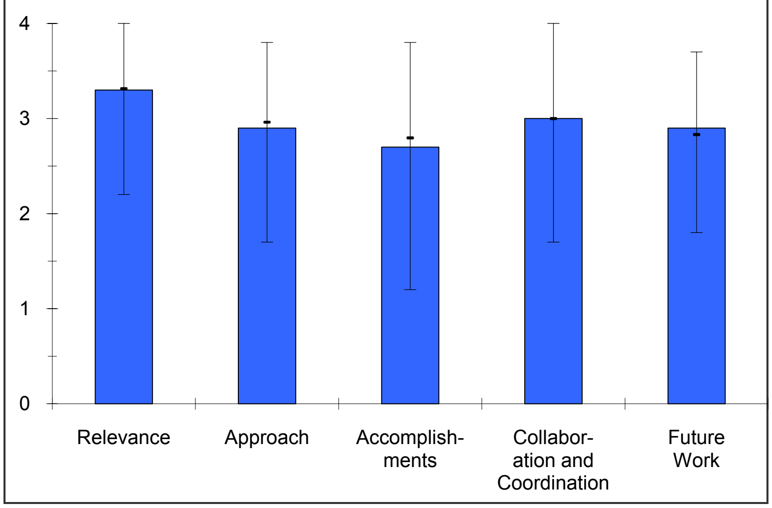
- The DOE accelerated test protocol (full voltage cycle test) needs to be performed on materials that have been shown to be useful, but also include lower voltages (e.g., 0.1-0.65 V) to test stability of the added metals.
- Consider measuring carbon dioxide on at least some of the experiments; e.g., during high potential and simulated start/stop testing (there is carbon in the MPL).
- Look at applying concepts to dispersed catalysts on carbon supports.
- There seems to be various reactor engineering solutions that would solve this concern in rather simple ways. Certainly, 3M can figure ways to keep oxygen out of the fuel cell anodes. It is possible that durability will turn out to be a fuel cell stack issue, not really addressed by the MEA alone. 3M is talented, and needs to use its chemical engineering resources to address "durability" broadly, including reactor design; i.e., stack design. Like any chemical fixed bed reactor, the catalyst and the reactor engineering are necessarily merged.
- The team should continue this direction.
- In principle, this project represents an effort in fundamental electrocatalysis and, as such, it would benefit from more thorough characterization using both electrochemical (other than voltammetry) and non-electrochemical tools.
- Poor durability of several proposed anode and cathode electrocatalysts needs to be addressed as soon as possible and alternative solutions should be proposed.
- More challenging second-year milestones should be proposed. They ought to combine desired OER (cathode) and ORR (anode) activity with stability.

Project # FC-07: Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes
Bryan Pivovar; National Renewable Energy Laboratory

Brief Summary of Project

The overall project objective is to produce novel catalysts based on extended platinum surfaces with increased activity and durability. 3M's (and others) demonstrated improvements in specific activity and durability using similar materials have shown significant promise. This project focuses on limitations in terms of mass activity and water management.

Overall Project Score: 2.9 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The National Renewable Energy Laboratory (NREL) is directly addressing a key DOE target--catalyst/electrode structure and performance.
- The project is well aligned with the DOE research, development and demonstration objectives.
- Alternative pathways for cost reduction are imperative.
- The project proposes to address two critical barriers--activity and cost of electrocatalysts. It will probably be forced to address durability as well.
- It is not clear how the alternative approaches considered will reduce cost as there is no consideration at this early stage for manufacturing capabilities for many of the alternative, extended-surface catalyst supports.
- Cost and durability are the most critical gaps for fuel cell commercialization and the catalyst is important for both attributes. It is good to look at bulk characteristics, which would be a breakthrough from the current state-of-the-art dispersed platinum on high surface area carbon technology.
- It is worthwhile to look at the impact on morphology and "novel" structures on catalyst activity within the platinum/carbon system.
- The project includes using different methods for making the catalysts.
- The cost of implementing the new catalysts is not clear, as the synthetic methods could be expensive, but perhaps this problem could be addressed later if they find a particular structure with exceptional attributes.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- The project's several different approaches seem to be modifications and/or enhancements of approaches taken previously by other researchers. The emphasis seems to be on alternative ways to prepare catalyst layers or electrodes.
- There is a strong modeling component to the project.
- The project has a good approach. I am a little concerned that there are too many avenues for exploration, but it is early in the project and I expect that some downselection will take place during the course of the project.
- Working with known material, and interrogating roles of substructures and morphology, is an exciting innovation. The team has a bold approach with regard to fabrication methods that is applauded.
- Some of the assumptions inherent in the approaches cited may need to be revised based on acquiring a deeper understanding of the 3M nano-structured thin film (NSTF) catalysts used as a model and baseline.
- It is not clear exactly what the electrode studies will entail, that is different from much of what is already in the literature, in order to pursue extended-surface catalysts; e.g. nanowires, carbon nanotubes (CNTs), and vertically aligned carbon nanotubes (VACNTs).

- A clear strategy is not seen and activities are kind of exploratory if CNTs and the like will work.
- It is necessary to dig in bulk characteristics. This project is looking at atomic layer deposition. Additionally, crystallite orientation and interaction with substrate materials should be considered.
- Materials stability is important for durability and it is necessary to develop its metric for this oxygen reduction reaction (ORR) catalyst concept.
- As for the modeling approach, it would be good to add quantum mechanism molecular dynamics to understand the ORR activity enhancement mechanism for this catalyst concept.
- This project includes using a variety of synthetic tools to make new platinum structures.
- The overall characterization of the project is good.
- There are questions about whether the catalysts can be heated to the high temperatures needed to order platinum and impart stability.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The project is less than a year old, so progress is limited but meaningful.
- Some approaches have already been downplayed.
- The initial results are encouraging.
- These are early days for the project, but the team is off to an excellent start. Clearly, much work has been done to climb the learning curve of substrate materials and novel material production methods.
- This project is very early in its beginning. It is too early to judge, but there has definitely been some progress to date.
- The relevance of their achievements is hard to judge.
- The project has produced a good atomic layer deposition result.
- Their accomplishments are adequate, considering that it is still early in the program.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Collaborations are plentiful and varied and include an automobile original equipment manufacturer (the Nissan Technology Center of North America).
- . I would like to see more industrial involvement from the end users of the technology to be developed.
- This is a "dream team", and Pivovar is an apt maestro. Coordinating this number of contributors is a tall order; however, the level of interaction and focus at the start seems excellent.
- The PI has enlisted a wide variety of collaborators with clearly defined functions. This is good for a start.
- The processes for applying catalysts may be limited by just relying on academia and NREL. It is critical that, whatever approaches are considered, that consideration be given to ultimate scalability to the high rates and low costs needed for automotive production.
- More cell manufacturers on the team is desirable.
- The project incorporates an effort to bring university accomplishments (e.g., at Riverside) to a DOE laboratory.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- Much of the future work is aimed at fabrication of structures and not on new catalyst materials.
- Milestones and/or go/no-go points in the next year are not clearly defined.
- The plan is clear and well thought out. I hope that some downselection takes place as time passes to reach a preferred approach to deliver or demonstrate a significant breakthrough.
- The presentation offered a fantastic vision of what could be possible. It will be exciting to see the results of successful fabrications and the performances of the synthesized structures.

- The currently proposed work is focused on the material structures that appear to look like the 3M NSTF as a model of the extended surface catalysts; however, many other properties must be considered besides one structural aspect in order to make the whole electrode and its catalytic properties and manufacturability feasible. These aspects still do not seem to be appreciated or understood.
- It looks more like “trying out”, rather than pursuing a clear strategy.
- As mentioned above, the crystal orientation of the atomic layer deposition should be investigated.

Strengths and weaknesses

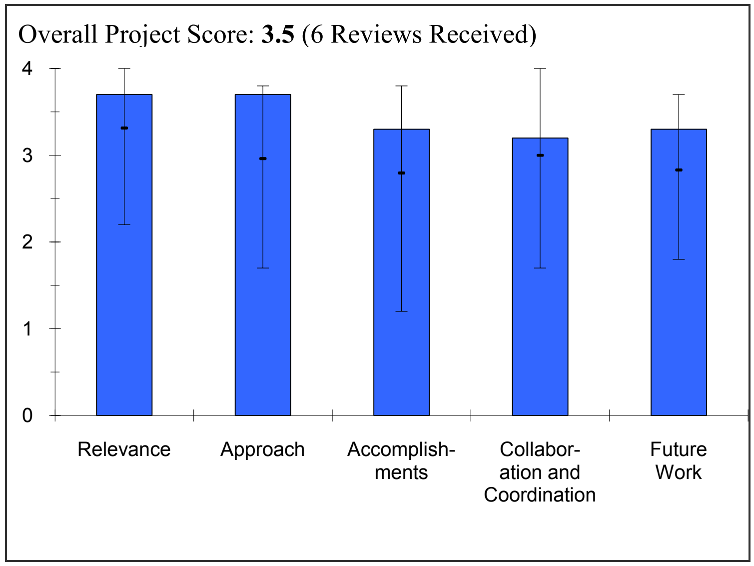
Strengths
- NREL employed a clear plan, and has displayed good organization starting out with the project.
- The team has unprecedented bandwidth and capability, from theory to testing. The research builds on platinum attributes which lend high confidence for a positive outcome.
- The project has many collaborators with good capabilities and a strong PI.
- It is important to consider how novel structures can be used to affect catalyst activity and durability for polymer electrolyte membrane fuel cells.
- There is good collaboration with the University of California, Riverside.

Weaknesses
- It is too early to see where the cracks are.
- Team size and coordination challenges may interrupt quick progress at times.
- This project is trying to duplicate the 3M catalyst project without really narrowing in on the one aspect that makes it so unique, the crystalline organic whisker support. This technology area could benefit from high level fundamental work befitting a national laboratory consortium. The organic pigments represent an extremely broad and novel approach for high volume manufacturable, high durability catalyst support concepts. Materials and structural options are far broader than for any type of CNT or VACNT type system. These options should be looked at far more broadly. This suggestion has been communicated to the PI in private, with the encouragement to look at this class of materials in more depth. 3M cannot do the fundamentals alone or at the level potentially possible at national laboratories. They should not assume they cannot bring a great amount of further fundamental understanding to the 3M catalyst system, and therefore have to spend resources studying less promising, and, in many cases, already evaluated catalyst systems.
- The team needs more fuel cell manufacturers.
- The odeling approach used is relatively weak. It would be good to consider inclusion of Professor Goddard, at the California Institute of Technology, as well as an Argonne National Laboratory non-platinum bimetallic cathode electrocatalyst specialist.
- Some of the results for the catalysts look poor. The project team should enlist more help from Shyam Kocha to aid in the analysis.
- It is not clear if the catalysts can be annealed to higher temperatures needed to impart stability on the platinum.
- It is not clear how NREL is working with 3M. Because the 3M catalyst system doesn't seem to be workable in practical fuel cell systems, it seems like a waste of effort to try to improve this catalyst system and/or format.
- It's not clear how the alternative deposition methods would be used at a large scale and what the cost would be. If using the atomic layer deposition method and no nucleation takes place until after 100 cycles, is platinum lost? If so, this factor should be taken into account for the overall cost of the catalyst.

Specific recommendations and additions or deletions to the work scope

- Eliminate the focus on CNTs and VACNTs, which will always have the issues of cost, speed of manufacture, limited currents from thick dispersed electrodes, and carbon corrosion.
- Add a focus on the entire new class of organic heterocyclic pigments of which the 3M perylene red is the classic and first example.
- The project should include two or more fuel cell companies.

Project # FC-08: Nanosegregated Cathode Catalysts with Ultra-Low Platinum Loading
Nenad Markovic; Argonne National Laboratory



Brief Summary of Project

This project focuses on developing a fundamental understanding of the oxygen reduction reaction (ORR) on PtM bimetallic and PtM_1M_2 ternary systems that would lead to the development of highly efficient and durable real-world nanosegregated Pt-skin catalysts with low Pt content. ANL's materials-by-design approach will be utilized to design, characterize, understand, synthesize or fabricate, and test nanosegregated multi-metallic nanoparticles and nanostructured thin metal films.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This project team is probably the only group to set targets beyond DOE-specified goals, although the outcome is more theoretical. This setting of a target beyond current goals is probably necessary as one moves to the membrane electrode assembly (MEA) and stack stage where the inevitable loss due to scale-up is found.
- The relevance is clear and well defined. DOE targets are somewhat outdated and ANL has taken a forward-looking step of promising to achieve even higher catalyst activity targets.
- We agree that a new DOE target for catalyst total content target should be 0.1 g/kW or 10 g_{PGM} per 100 kW stack to keep up with the improvements in catalyst activity as well as to reflect the increased cost of Pt ($50/g). It is not necessary to define a specific activity. A mass activity of ten times today's Pt/C is also appropriate (rather than four times).
- This is a really relevant project.
- The project addresses the top stack component R&D need: decreasing the loading and increasing the performance of oxygen catalysts.
- This is an excellent project that addresses an area of need (high activity electrocatalysts).
- The project, with its focus on ultra-low PGM loading, is well aligned with DOE R&D objectives.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The experiments correlate well with the theory. The theory has guided these experiments well.
- The approach has a sound scientific basis that demonstrates improved activity in half-cells followed by implementation in fuel cells.
- This was a really interesting project, very well thought-out and planned. The team made significant progress in a relatively short period of time. It was an outstanding approach, starting with fundamental mechanistic studies to optimize the electrodes, and then moving to both small scale and short stack fuel cell testing. It will be interesting to see the results following the fuel cell experiments.
- The project seeks to exploit lessons learned in the study of extended surfaces to the realm of nanoparticles, contributing directly to addressing technical barriers.
- This was a great approach from a strong team. The PIs have an outstanding prior publication record in this field.
- The technical targets are three times the DOE activity targets and focus on alloys and skin/core-shell type catalysts. Activity has been as high as 2,100 mA/mg.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The project is very good, but it is still unknown as to why or how the effect could be translated into final systems.
- Task 1.2 is related to the reaction mechanism of ORR and is supposed to be 90% accomplished. If this is simply based on the data showing all Tafel plots are similar, then the task has not been accomplished. The ORR mechanism is complex and has not been resolved for the last 40 years. There is no evidence of any results or data showing any further clarity on the ORR mechanism in the presentation. Deeper analysis of the rotating disk electrode (RDE) data, reaction orders, and Arrhenius effects have not been shown. An explanation of why nanosegregated particles have higher specific activity compared to extended films or bulk Pt and Pt alloys has not been touched upon.
- It is not clear from the presentations as to which materials are supported on carbon and which are not. It is unclear whether or not unsupported materials are considered films or nanoparticles. All the other accomplishments are outstanding!
- The group really made significant technical progress in a relatively short period of time. They did an excellent job optimizing the activity of the electrodes through modeling and experimental activities.
- The project has shown high performance and durability of novel catalysts in many *ex situ* tests.
- By using well-ordered and defined systems, the project has demonstrated improvements in electrochemical activity and durability.
- The only weakness is that these results have not yet been verified in fuel cell tests leaving some concern that the promising results shown will not translate into in-system gains.
- It is not clear when carbon supports have been used in testing and if the results presented are ever for unsupported catalysts. The role of the catalyst-support interaction is critical for some of the conclusions presented and it is unclear to what level this has been considered.
- Great progress has been reported in terms of enhancement in catalytic activity.
- The project determined the effects of particle size, preparation temperature, and annealing temperature on specific and mass activity. The platinum skin protects the Pt_3Ni core. 30,000 cycles at 0.5-1 V. Mass activity for Pt_3Co of 1,500 mA/mg was obtained, while ternary PtNiCo has an activity of 2,100 mA/mg and 5 mA/cm^2. The MEA results are needed.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The addition of GM as the MEA fabricator is critical to take this discovery to the next step.
- Determining the lifetime under real conditions may give additional insight.
- Collaboration is sufficient at this stage of the project.
- The group contains highly technical and accomplished individuals who work together seamlessly.
- The project consists of several collaborators who are appropriate for the project.
- It is not clear from the presentation what role, if any, the collaborators played in the data presented. While the high-resolution transmission electron microscopy (HRTEM) is likely from ORNL, the roles (if any) of JPL, Brown and Indiana University-Purdue University Indianapolis for the data presented are unclear.
- The team in place is very strong and has very good interaction.
- The tasks for team members are well defined and well laid out.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- This reviewer agrees that more work on stability will be done.
- There is concern that there is not enough time left in the project to address MEA making and testing.

- The PIs need to explore the mechanism of ORR for the new materials in depth and explain the reasons for the high activity of their materials in half-cell studies. The first steps in incorporating these materials into MEAs need to be attempted sooner.
- The group has a clear plan going forward. It will be very interesting to see the performance of the MEAs and short stacks they plan to make.
- Future work, particularly for fiscal year (FY) 2010, focuses on a continuation of prior work. It is not until FY 2011 that MEA testing is incorporated and the exceptional results shown in *ex situ* testing should be validated sooner in fuel cells.
- The project started in Fall 2009. The future work is well laid out, with focus on MEA testing in 2011.

Strengths and weaknesses

Strengths
- There is an outstanding combination of theory and practice to identify a "super" class of catalysts on this project.
- New materials that exhibit high catalytic activity have been demonstrated in half-cell experiments. DOE targets are somewhat outdated and ANL has taken a forward-looking step of promising to achieve even higher catalyst activity targets. The project has implications both on the fundamental level as well as potential for application at a practical level.
- This is overall a very solid project that has already produced interesting results in a relatively short period of time. The work achieved has been very nice.
- The project has demonstrated durability and performance in *ex situ* testing.
- There is a strong fundamental basis for developing next generation materials.
- The project has a strong team, excellent prior publication record, and a very good and rational hypothesis.
- There is a very high specific activity for all types of catalysts.

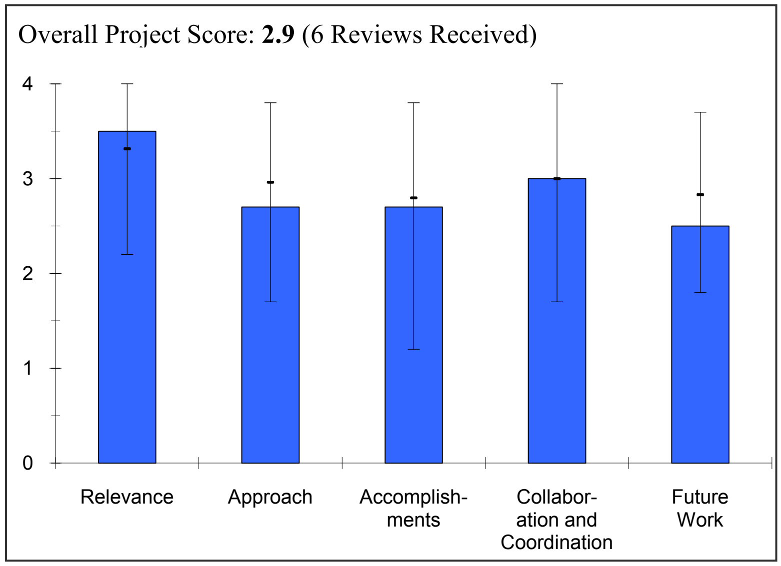
Weaknesses
- There is a delay in bringing this advance to the MEA and short stack testing stage. This reviewer is not sure if enough time is left for this critical final task.
-
- There is a lack of demonstration of materials in application: for example, testing in fuel cells or studies investigating the carbon support and/or catalyst support interactions.
- The MEA tests are not done yet; however, this is not a weakness because the project is young.

Specific recommendations and additions or deletions to the work scope

- The PIs need to explore the mechanism of ORR for the new materials in depth and explain the reasons for high activity of their materials in half-cell studies. The PIs need to describe the materials more clearly.
- The PIs should focus on MEA fabrication with partner GM.
- The PIs should accelerate tests involving fuel cells.
- The PIs should perform studies investigating the influence of carbon support, and the processing and characterization of electrode structure using novel catalysts.

Project # FC-09: Contiguous Platinum Monolayer Oxygen Reduction Electrocatalysts on High-Stability-Low-Cost Supports

Radoslav Adzic; Brookhaven National Laboratory



Brief Summary of Project

The overall project objective is to develop high performance fuel cell electrocatalysts for the oxygen reduction reaction (ORR) comprising a contiguous Pt monolayer on a stable, inexpensive metal or alloy, including nanoparticles, nanorods, nanowires, carbon nanotubes, scale-up syntheses of selected catalysts, and membrane electrode assembly (MEA) and stack testing.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project addresses three barriers associated with electrocatalysts, but only for the electrocatalysts. The scope is not broad enough to address catalyst/membrane or catalyst/ionomer interface effects or MEA operating condition effects, which would be outstanding.
- BNL uses one basic catalyst approach that is quite innovative in trying to overcome the barriers; i.e., developing contiguous Pt monolayers on various supports.
- The project is aligned with the DOE Hydrogen Program targets and goals. The project addresses the critical issue of fuel cell cost.
- The project addresses the top stack component R&D need: decreasing the loading and increasing the performance of oxygen catalysts.
- The team is trying to develop new, active catalysts and apply new synthetic methods to do so.
- Justification of catalyst costs with Pd and Ru seems unrealistic in that there is no information given on how these costs were derived and whether it was for massive production quantities.
- Clearly, improved fuel cell catalysts address DOE goals pertaining to cost and performance.
- The development of new ORR catalysts is perhaps the most important activity to address the commercialization barriers of cost and durability. This project is working on this topic exclusively.
- Two arguments can be made against the relevance of this project: 1) that Pd is used to replace Pt and therefore cost will remain high, and 2) that the catalysts will be unstable. In regard to the first argument, there are catalysts being made that have shown 0.44 A/mg_{PGM} (mg of platinum group metal) for both Pt and Pd) from the rotating disk electrode (RDE), so relevance is not lost. In regard to the second argument, there is a contention that Pt will be stable on Pd, which remains to be seen.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The basis of the research plan appears to be one factor affecting ORR activity that is directly related to surface fundamentals of the nanoparticles, e.g., how to obtain properly compressed Pt(111) facets. This is good as well as specific to the basic approach of trying to develop stable contiguous Pt monolayers. There may be other fundamental characteristics of the supported monolayer that will be important for finding the best ORR to use. It would be useful to see a consideration of these.
- The approaches with BNL focusing on more standard Pd nanoparticles and with MIT and Johnson Matthey on high aspect ratio support particles is a good start to cover a wide variety of support types.

- The project is directed at increasing the activity of ORR catalysts through manipulation of structure and use of extended surfaces.
- The durability of Pd cores (and Pd nanorods) and the redeposition of Pd on Pt is a concern. There is also a concern about stability in the cycling of Pd materials to low potentials (~ 0.1 V), where hydrides could potentially form.
- For Pt monolayer on Pd nanowires, the PIs should be able to extrapolate activity to see if it is feasible to make Pd nanowires thin enough to reach the target mass activity (per gram PGM).
- The project presents a range of materials and approaches to addressing the issue of decreasing Pt use in fuel cells. While each of the individual approaches has merit, the project lacks clear organization and direction for advancing and evaluating specific materials.
- The project seems like several independent projects that do not benefit specifically from synergies between the different approaches.
- This project should be synthesis driven. For Cu underpotential deposition (UPD), pulsed wave, etc., it is not clear how compatible the synthetic approaches are with MEA manufacturing.
- A better approach would be to understand what the catalyst manufacturers need, and then work backwards toward practical approaches.
- It was not clear which catalysts were carbon-supported.
- The PI assumes that spreading the platinum to a maximum extent will improve performance. However, fuel cells are influenced by a wide variety of factors which result in non-homogeneous three-dimensional space. So, this approach might be useful, but not necessarily so. This process focuses on carbon supports, which is a very well-studied area. Carbon-containing supports might not be the best choice.
- For the most part, the PI seeks to layer Pt on Pd, Ru, or Pd alloys (with the exception of some samples such as Pt hollow spheres). Pd and Ru are both precious metals and the price of both will rise with high volume automotive commercialization. While the PI has been able to show that high PGM-based mass activity can be achieved on an *ex situ* basis while using Pd or Ru, the approach of doing so implies that the PI is making it tough on himself. Layering Pt on a cheaper base metal would be more robust to precious metal prices.
- The durability of the samples generated is still in question. With the exception of work at 3M (for samples where mass activity was not reported in these slides), cycling reported here has been in RDE with no potential below 0.6 V as the cathodic limit. Durability needs to be shown *in situ* with lower potentials.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Each of the approaches suggests pathways to overcome the barriers of nanoparticle ORR activity, PGM cost, and stability. This project is very good in considering a wide variety of support particle approaches.
- The amount of data shown is very limited; however, it suggests an opportunity for much more testing and evaluation to statistically support the conclusions. Also, the testing conditions were very sparse or not reported at all, which limits conclusions as to the effectiveness of the tests.
- Testing stability under voltage cycling should be done under more severe conditions (higher temperature, wider voltage range, step function cycling) to determine the limits of stability.
- It is not clear how the Pd nanorods were made. The STEM (scanning transmission electron microscopy) photo is very unclear, with no size information given.
- The fuel cell testing demonstrates one potential caveat on the ultimate utility of any of these approaches, and that is the intrinsic limitation of dispersed electrodes to enable high current densities, ~2 A/cm^2, which is the fastest route to reducing stack costs besides reducing PGM loadings. The DOE targets for mass activity do not dir^e^ctly correlate with peak power and therefore are not sufficient metrics for predicting catalyst utility.
- The project is slightly less than one year from its start. The team has made a good start.
- The project team should make sure all mass activity measurements are reported in terms of total PGM. Adjusting activities for today's PGM price is not useful because the price of Pd (or other PGMs) will undoubtedly increase if demand increases with use in fuel cell applications.
- Hollow Pt spheres show good mass activity, exceeding DOE target.
- For a Pt monolayer on Pd nanowires, the PIs should be able to extrapolate activity to see if it is feasible to make Pd nanowires thin enough to reach target mass activity (per gram PGM). Pt/Pd on CNTs may be more likely to get to the target, and use of Pd seems to provide more stability than Pt on CNTs from cycling test at BNL.

- There needs to be an explanation for the large electrochemical area loss for Pt/Pd/C MEA testing at 3M.
- Significant progress has been made in the area of bromine treatment to remove low coordinated sites and hollow nanospheres. These advances move toward overcoming technical barriers associated with electrocatalysis.
- Studies involving multiple Cu UPD steps and displacement with Pt and Pd may be difficult to scale to meaningful quantities. The potential cycling to only 0.9 V may limit some of the potential aging effects.
- The work to date on CNTs has not shown increases in performance that are particularly compelling.
- Pd nanorods and nanowires are interesting from an academic point of view, but may not offer meaningful cost benefits when used as supports for Pt monolayers.
- 3M's fuel cell results showed meaningful, although modest, losses from potential cycling after just 1,000 cycles on durability.
- The team has made some progress, although it seems to be a repeat of progress made in prior years, and the PIs do not appear to have any big breakthroughs this year.
- The results, to date, appear modest.
- The project has done a nice job of quantifying activity for a number of samples. However, the value that counts is mass activity on a PGM basis. This value is highest for Pt hollow spheres (1.1 A/mg_{PGM}), Pt/Pd/Ru/Pd particles (0.44 A/mg_{PGM}) and Pt/Pd nanorods (0.42 A/mg_{PGM}). PGM-based mass activity was not reported for Pt/Pd nanoparticles and Pt nanowires, but should be.
- Durability needs to be shown using *in situ* data.
- Microscopy on some samples (e.g., Pt nanowires) would help toward explaining whether Pt coordination or crystal orientations contribute toward higher activity. Microscopy would also help toward predicting whether coordination or orientation would remain stable after cycling. Because fuel cell cathode half-cell potentials will inevitably rise high enough to create surface oxides, it would be interesting to see if such oxides would cause restructuring or piercing of Pt monolayers.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- BNL has strong team members with MIT and JMFC, but it was not brought out how they are mutually collaborating to make any one approach succeed.
- There appears to be good collaboration between the partners. Collaborations outside of project partners with key MEA manufacturers and stack developers are present.
- BNL has a history of collaboration in the area of catalyst development.
- The project team has limited collaborations, but project members each bring specific strengths.
- Johnson Matthey and 3M provide significant support with catalyst scale-up and fuel cell characterization.
- MIT's role seems largely disconnected from the primary focus and most promising results to date of the project.
- The project has a good team of DOE laboratory, university and commercial partners; however, the activity level of the commercial partner was not very clear.
- BNL teams with excellent partners.
- Most collaborators listed (MIT, 3M, Johnson Matthey) have a clear role in assisting the PI (BNL), and their efforts are well represented in the project. The exceptions are United Technologies Corporation (UTC) and the University of Wisconsin. It is clear in the future work slide that UTC will be expected to fabricate MEAs and test them. University of Wisconsin appears to do calculations on onion catalysts, but it is unclear from the slides if the calculations pointed in the direction of Pt/Pd/Ru/Pd formulations, or if the modeling was done following the synthesis.
- Johnson Matthey has already been incorporated into catalyst scale-up work, and 3M has been used in cell testing. The specific activity data are interesting, but hopefully mass activity can be shared from *in situ* tests in the future as well.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- The breadth of work in any one of the three approaches is enormous, so it would be expected that the rate of progress might be small.
- For Pd-Nb, future work needs to reduce the total noble metal content in the core.
- Future plans are primarily a continuation of the different thrusts presented to date. The future work is quite disconnected with little synergy or overlap between UPD synthesis of Pt/Pd, the synthesis of Pd particles and metallization of CNTs, oxides, and nitrides.
- Scale-up of catalysts seems premature without some sort of performance metric associated with the decision.
- Future work shows no clear path to scale-up to 20 g of catalyst.
- Certainly, detailed testing and stack testing will take more resources, and would depend on the preliminary results to be achieved successfully here.
- The future work should focus more on those samples that have shown the highest A/mg_{PGM} values. It would be interesting to see Johnson Matthey scale-up these catalysts and have 3M or UTC test them *in situ* for activity and durability.
- Improved or initiated synthesis of Pd-W and Pd-Nb core materials is worthwhile, although low limiting current for Pd-W may point to two-electron reduction. Stability of Pd-V seems dubious. Calculations from University of Wisconsin may be helpful in this regard.
- At some point, a go/no-go decision may need to be made as to whether CNTs are too rough for layering of extended catalyst structures. If roughness causes more PGM to be laid down, perhaps a base metal could be used to "coat" the CNT. If not, then perhaps this aspect of the project needs to be cut.
- Islands of Pt on Ni, Fe or Co would not be expected to be stable.

Strengths and weaknesses

Strengths
- The project has good fundamental concepts and strong team members.
- There is a good understanding of ORR activity dependence on Pt structure.
- Collaborations in place with major companies in the area of MEA and stack development are worthwhile and productive.
- BNL uses an excellent materials set and has demonstrated activity improvements using specific systems, namely bromine-treated nanoparticles and hollow nanoparticles.
- There should be a focus on scale-up of some of BNL's outstanding catalyst systems.
- Presentation slides gave a lot of information on synthesis methods.
- The team is admirable, and appears to have adequate resources.
- The project demonstrates consistent reporting of RDE measurements, as evidenced by limiting currents shown. RDE is performed cleanly and accurately.
- The PI has sought to incorporate novel catalyst synthesis methods and morphologies.
- Collaborators provide useful content, whether through novel fabrication (MIT), catalyst scale-up (JMFC), or testing (3M).
- Some materials, such as Pt hollow spheres, have demonstrated very high PGM-based mass activities. In future years, this could prove to be an interesting discovery.

Weaknesses
- The team uses too many approaches to pursue the basic concept of developing contiguous Pt monolayers on a stable support. There are many questions to be answered that are common to all the various supports being considered. Faster progress on the fundamentals, as well as practical utility, might be made if the team chose one approach and focused on it.
- There are too many disconnected materials and approaches without a clear sense of direction or information regarding the decision-making process for each of the materials presented.
- The role of carbon and support interactions is largely unclear for the systems presented.
- There is a lack of hard focus on synthesis, and it is not clear why BNL cannot get a serious synthetic partner. JMFC seems to be playing a minor role, and no progress was reported from them beyond mentioning that JMFC is going to scale-up.
- The basis of the cost analysis was not explained.

- In general, the testing environment appears weak. There needs to be a real focus on experiment replication, and on highly specified testing conditions. Tests need to follow device changes during the test period, for instance, migration of catalyst materials. Fuel cell testing involves complicated fabrication, and there needs to be considerable interaction between the catalyst people (generally this team) and the MEA people (probably JMFC).
- More emphasis needs to be on PGM-based mass activities, not Pt-based ones. Decisions about which catalyst to scale-up need to be based upon PGM-based mass activities.
- More emphasis is needed on microscopy, and how atomic-level features influence observed activity.
- Better reporting of the role of modeling is needed.
- Some of the future work points toward catalysts whose instability may be obvious before testing. Perhaps calculations could help screen unstable catalysts.

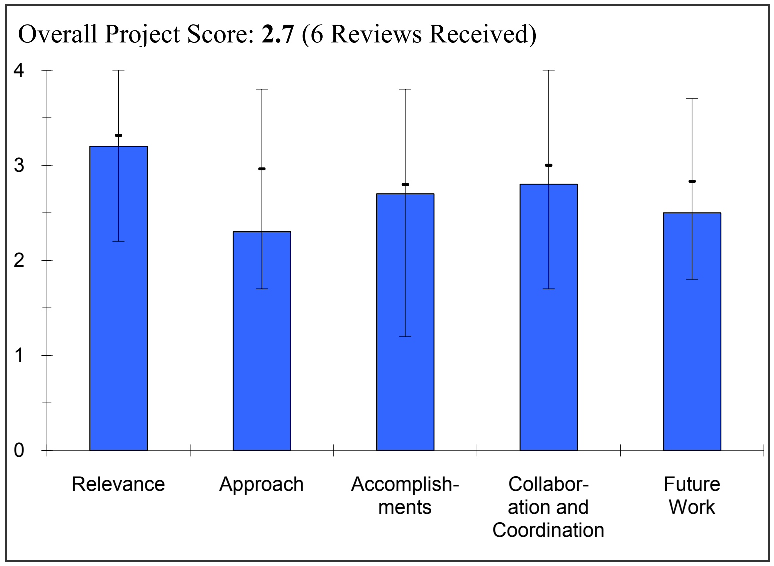
Specific recommendations and additions or deletions to the work scope

- The work scope should include trying to downselect one of the approaches for determining the entitlement potential of the "continuous Pt monolayer" concept, and focus the whole team on it.
- The high activity of hollow Pt spheres suggests that work on these should be emphasized in the near future - durability studies on MEAs, etc.
- There should be a more narrow focus on the material set to include only a few of the most promising materials. Downselect MWCNTs or quickly establish ability to form two-dimensional islands as a go-no go decision for this material.
- Reconsider quantity of catalysts synthesized, based on some combination of performance metrics.
- The team should have stronger synthetic chemistry. The team at MIT is not known for synthesis, and might be replaced with a better team.
- The testing plans need to be crafted and published. Testing protocols need to avoid, at least initially, moving the fuel cell specimens into destructive conditions, such as fire and high voltage. After adequate performance has been thoroughly and successfully demonstrated, more vigorous testing may make sense. Those types of tests might be called "crash testing."
- The work scope should include probing whether PGM loading of CNT-based catalysts can be reduced using base metals. Rough surfaces may necessitate high loading of PGMs if only PGMs are used.
- The project must stay focused on PGM-based mass activity.
- A scale-up of Pt hollow spheres is worth trying.
- BNL should not include unstable catalysts in future work by using calculations to discern likely instabilities.
- BNL should make greater use of microscopy for understanding the fundamental reasons for activity increases.

Project # FC-10: The Science and Engineering of Durable Ultralow PGM Catalysts
Fernando Garzon; Los Alamos National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) develop durable, high mass activity platinum group metal (PGM) cathode catalysts to enable lower cost fuel cells; 2) elucidate the fundamental relationships between PGM catalyst shape, particle size and activity to help design better catalysts; 3) optimize the cathode electrode layer to maximize the performance of PGM catalysts to improve fuel cell performance and lower cost; 4) understand the performance degradation mechanisms of high mass activity cathode catalysts to provide insights to better catalyst design; and 5) develop and test fuel cells using ultra-low loading high activity PGM catalysts to validate advanced concepts.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The project focus is on catalyst materials and structure, which directly address critical DOE technical objectives. Performance, durability, and cost are considered.
- Potential cost reduction deriving from novel supports exploiting known catalytic materials have compelling prospects.
- This project had particularly good relevance for understanding. Final goal should be realized at larger scale membrane electrode assemblies (MEAs) or short-stacks.
- Durable ultra-low PGM catalysts are needed to meet DOE cost and performance targets.
- Although the relevance appears reasonable, work in this area of catalyst nanoparticles, low loadings, etc., has been carried out for 40 years. Fundamentally, smaller particles with high surface area to volume will always be less stable than larger particles or extended films. There is no scope or basis for improvement in this direction. Spheres have the highest area-to-volume ratio.
- Automotive companies have already succeeded in preparing low-loaded electrodes; as low as 0.05 mg_{PGM}/cm^2, and employ these in their stacks. Other pathways to low-loaded thin catalyst layers have been heavily funded through the work of 3M in nanostructured thin film (NSTF) for the last decade.
- The project's focus on ultra-low loading is well aligned with DOE R&D objectives.

Question 2: Approach to performing the research and development

This project was rated **2.3** on its approach.

- The approach includes depositing low PGM loadings on novel support structures and materials. The novel support structures may make uniform PGM deposition and ionomer infusion into the catalyst layer difficult.
- I am not sure why the project flow slide in the supplemental slides was not shown–this would have added needed clarity to the vision of the project. Also, theoretical approaches to catalyst design are mentioned, but there is no explanation of how this will be done–the framework needs to be explained.
- The project seems well rounded with multiple approaches to achieve the objectives.
- It would be interesting to know whether sputter deposition on nanotubes will be implemented in a manufacturing process, and if so, how it would be implemented. Including a schematic could be helpful.

- This project clearly builds on the premise of the successful NSTF project, which has brought positive results. It is not clear why we would expect anything superior would come from this project. Electrodes tested to date are too thick. Thickness optimization will be needed.
- The approach lacks any clear pathway in that it basically claims to employ modeling to come up with an improved catalyst layer. It is unlikely that a significant improvement will result from work in this direction, considering that the lab does not have an in-house catalyst layer that meets currently attained performance and durability standards reported in the literature by automotive fuel cell companies.
- Different shaped catalysts were synthesized. Novel supported catalysts such as conductive polymers and Pyrograf nanowires were used. Various methods such as PVD and RF sputtering were used to deposit low PGM on supports.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The project is less than a year old, but meaningful progress has been reported. Test articles have been fabricated and tested.
- All of the major areas of research have been initiated, including rotating disk electrode (RDE), modeling, and characterization.
- LANL has very impressive micrographs that show interesting structures and morphologies. It seems that some of the enabling methods for materials preparation have been developed successfully.
- LANL has good results demonstrating low loadings of Pt distributed over the various supports (sputter deposition), but operation in an actual MEA appears to be inhibited, with the exception of the use of ionomer. The fact that ionomer is necessary to extend the three-phase interface implies that these are not really thin layers of catalyst, or that the innate hydrophobicity is higher than typical materials and the ionomer assists in making them more hydrophilic.
- Electropolymerization, with the use of a heparin template to keep discrete tube architecture instead of aggregates, provided very controllable results. This is an exciting development that may be important later on for developing a controllable manufacturing process.
- Limited fuel cell (fuel cell) data is very poor. No RDE or fuel cell activity measurements were available. The use of ionomer in the cathode, not surprisingly, slightly improved performance.
- The preparation of Pt on polypyrrole nanowires is interesting work.
- A baseline characterization of standard materials demonstrating the benchmarks established by DOE for Pt/C has not been done. Without the basic electrochemical characterization of currently available electrocatalysts and meeting benchmarks already achieved by the fuel cell community, it does not make sense to proceed forward with new materials.
- The fuel cell H_2|Air curves show, along with the conclusions, very poor performance with no analysis of electrochemical surface area and oxygen reduction reaction (ORR) activity. There is no scientific basis for claiming poor utilization without all the requisite and obvious electrochemical diagnostics.
- Current density of 0.65-0.9 A/cm^2 was achieved at 0.05 mg_{Pt}/cm^2. The project is only one year old, but the team has done a lot of synthesis on various types of carbon supports and different catalyst types. The team has shown rapid progress in testing MEAs from these synthesized catalysts.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- The team includes a fuel cell stack/system integrator, universities, and National Labs with relevant expertise. Input from an automobile original equipment manufacturer (OEM) and a catalyst supplier would be beneficial.
- The collaboration slide explains the roles of the partners clearly, but the extent of actual coordination and contributions to the results is not as clear.
- The project has a well-rounded team comprised of National Laboratories, universities, and industry partners.
- So far the subcontracts have not contributed much to the project, but this is due to delays in getting contracts in place. Also, LANL should be careful of overlap with the Pivovar's NREL project.

- LANL should partner with fuel cell companies who can show how to prepare a good state-of-the-art electrode that is currently used in automotive fuel cells and has high performance and durability. This would be a starting point for further improvement (rather than show improvements over poorly functioning in-house MEAs).
- The tasks among the collaborators are well defined.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- Most of the elements to understand and advance the concept are included.
- Catalyst/support interactions should be understood for the new support materials/structures.
- The work so far has produced some interesting materials, and has developed in the audience an appetite for seeing additional experimental results.
- The project has operated dry to minimize flooding. It will be interesting to see how this limitation (dry operation) is addressed in future plans. Similarly, it is unclear what will be done to reduce the use of high cathode flow to get around the flooding problem.
- As has been pointed out with 3M's "whiskers," the use of ultra-low precious metal electrodes brings with it a new challenge in developing alternative ways to remove water on the cathode. This topic should be addressed in future plans.
- More effort on developing techniques for introducing Pt is likely required.
- While carbons may still be of interest to automobile OEMs for supporting high activity nanoparticles, it is difficult to see where a study that focuses on carbon sites for nanoparticle nucleation is relevant. Even with conventional Pt/C fabrication, nanoparticles are not necessarily nucleated as the LANL project described.
- A modeling effort on large particles (which are more likely to be stable) requires an extraordinary amount of computational throughput. It is doubtful that much will be gained from the computational efforts described.
- Significant changes in future work based on baselining and benchmarking, as well as achieving the performance and durability of currently available electrodes and catalysts, needs to be undertaken. Any improvements should be on top of currently achieved performance and durability by the fuel cell industry.
- The future plan includes ORR testing, optimization of catalyst synthesis, and MEA durability performance. This plan is well laid out.

Strengths and weaknesses

Strengths
- The project team has an understanding of fundamental issues.
- The team has selected a great portfolio of approaches, which increases the chance of success in achieving DOE goals.
- There is a very good balance of theory and practice.
- The project team is well selected, whereby the strengths of the various team members complement each other.
- A wide range of conductive supports will be evaluated for continuous thin platinum surfaces.
- Some of the new substrate materials are interesting.
- The project has produced good results at low Pt loading.

Weaknesses
- There is a lack of articulation of the project structure and a clearly explained connection between development efforts and ultimate technical targets.
- Interfacial contact, as mentioned by the author, is a current weakness that should be addressed in the future plan.
- There are no plans that anticipate issues with water balance in the cathode ultra-low PGM structures.
- There is no clear fundamental basis why these concepts will be an improvement over existing materials. The project plan is too quick to jump to fuel cell testing while significant materials development is still required.
- A baseline characterization of standard materials demonstrating the benchmarks established by DOE for Pt/C has not been done. Without the basic electrochemical characterization of currently available electrocatalysts and

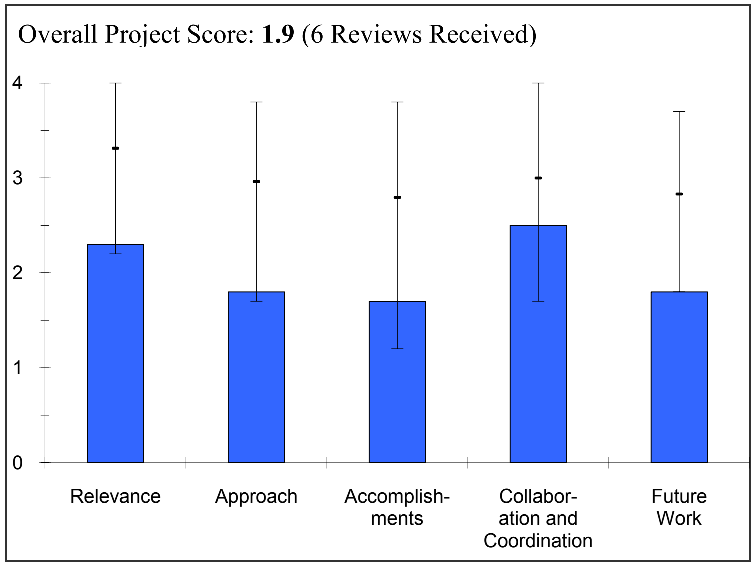
meeting benchmarks already achieved by the fuel cell community, it does not make sense to proceed forward with new materials.
- The approach lacks any clear pathway in that it claims to employ modeling to come up with an improved catalyst layer.

Specific recommendations and additions or deletions to the work scope

- The project needs to include durability tests.
- Some of the supports are hydrophilic. It will be worth seeing how this will impact mass transport at high current densities.
- Remove work on studying nucleation sites on carbons, as this is not seen as value added. Focus on RDE work before scaling up to MEAs. Be sure to include Pt alloys.
- It is questionable to continue funding this project based on the work and approach so far.
- The team should proceed in the same direction. The team may want to consult with LANL regarding their microporous layer development to mitigate flooding issues for hydrophilic supports.

Project # FC-11: Molecular-scale, Three-dimensional Non-Platinum Group Metal Electrodes for Catalysis of Fuel Cell Reactions
John Kerr; Lawrence Berkeley National Laboratory

Brief Summary of Project



Project objectives are to: 1) demonstrate that non-platinum group metal (PGM) catalysts can be used for oxygen reduction in polymer-coated electrode structures based on polyelectrolyte membranes; 2) incorporate catalysts into polymer binders of composite electrodes for the construction of membrane electrode assemblies to demonstrate that this is an effective matrix for the testing of new catalysts; 3) demonstrate that the three-dimensional structure of polymer-coated electrocatalyst layers can offset slower kinetics of the catalyst centers when compared with two-dimensional platinum or non-platinum catalysts; 4) demonstrate that significant stability of the matrix is possible; and 5) demonstrate the design, synthesis and scale-up of new catalysts capable of performance that is superior to platinum group metals.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.3** for its relevance to DOE objectives.

- The project does address and support DOE objectives by considering non-PGM catalysts.
- Cost and durability are the most critical gaps for fuel cell commercialization and the electrocatalyst is the most important factor for these attributes. A non-PGM catalyst has the potential to make a breakthrough.
- It is unlikely that this project will contribute to furthering fuel cell objectives of 2015. This is a fundamental research project that will take 20 years to demonstrate useful results and should correctly be funded through other sources.
- The development of improved non-precious metal catalysts offers the potential to overcome the largest stack component cost in fuel cell systems. The decrease in turnover frequency and packing density of such materials limits their potential impact.
- This is a project on non-Pt catalysts in polyelectrolyte membranes. It has no apparent benefit to DOE. While non-Pt catalysts are of interest, this project is just a jumble of ideas that seek to replicate successful projects on non-Pt catalysts by making them in a different format. A significant effort was made in the presentation to justify the concept, but there was no quantitative path. This project might be a good Office of Basic Energy Sciences project for $300,000 a year, but it is not ready to be an EERE project.
- Achieving cost, performance, and durability is essential for fuel cell commercialization. This project is projected to make progress in all three areas. Clearly, the focus is correct.

Question 2: Approach to performing the research and development

This project was rated **1.8** on its approach.

- The approach is novel, but it is very high risk with insufficient rationale for expectations of success.
- The expected conductivity, activity (turnover frequency), and stability is in the polymer electrolyte membrane (PEM) environment should be included with reasons why the teams expect those values.
- It was not clear what the technical basis was for considering this approach.

- It will be interesting to see how materials will be characterized for structure and activity.
- Homogeneous catalysis in a fuel cell would likely add ohmic losses.
- The go/no-go criteria at the 24-month milestone are not well defined.
- Basically, it is too early to discuss *in situ* level testing. Prior to discussions on how to make the catalyst layer with ionomer, it would be good to focus more on catalyst mechanism understanding to see whether or not the non-PGM catalyst can meet the end-game target.
- The approach is not well described and does not take into account the work done in the literature.
- The implementation of non-precious metal catalysts in fuel cells has significant challenges associated with the performance and durability of the catalytic centers.
- The project focuses on biomimetic complexes and oxygen separation systems that are unlikely to be suitable for commercial applications. Biomimetic systems operate near neutral pH and have significant lifetime limitations, perhaps due to the fact that biological systems do not prefer having long-lived species that can accumulate inside living systems.
- The approach to using a three-dimensional catalyst layer (<500 nm) does not make sense. The turnover frequency of these systems will be slower, the packing density of active sites much lower, and the traditional catalyst layers are much thicker (~10 microns). Add to this the short lifetimes of materials and the ion and electron transport issues, and it seems almost certain this project will not contribute to overcoming DOE barriers involving performance and durability.
- The speaker gave no path toward meeting the DOE targets. The PI does not quantify in any way how they can meet the DOE targets. The team is blending a lot of old ideas, and it is not clear how they will now be reformulated or leveraged into a catalyst breakthrough.
- Much of the "new" stuff is old and well tried. "Enzymes" clearly work as they are abundant in nature. However, such catalysts are short-lived and readily rebuilt, as needed. Fuel cells are invariably not homogeneous with flow starvation, varying reactant conditions, and a large number of factors that result in variable current densities across the current collecting surface. A "differential reactor" approach (essentially high stoichiometry) could sort some of these factors. Of course, one key issue is that the water concentrations are not uniform. Indeed, the same cell may be experiencing flooding and dehydration at the same time.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.7** based on accomplishments.

- The project is less than a year old. There is little real progress that is evident.
- It is difficult to distinguish between actual new results and summaries of previous work by the PI and others.
- It is difficult to evaluate this project with limited results shown.
- Results to date are essentially nonexistent. A handful of voltammograms based on either commercial catalysts, "Rh" polymer, or di-nuclear copper complexes show no promise for the catalysts to enhance the oxygen reduction reaction (ORR) in a meaningful way.
- Data on the effects of polymer chain mobility using traditional acidic polymers with traditional catalysts are not insightful for the systems proposed.
- The rest of the "technical accomplishments" section is essentially future work.
- The team is nine months into a four-year program (~20%) and no progress has been made. Slow progress was blamed on graduate students. The PI did not explain where approximately $2 million allocated to date has gone at the National Labs and other university laboratories.
- Not much new was described, although the classical electrochemistry was well presented and fun to listen to.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- National Laboratories and industry partners are appropriate and will benefit the program.
- A little collaboration exists, but it is primarily a two-National Laboratory project where the synergies between the two National Laboratories is not well defined. The role of 3M as an in-kind contributor is potentially useful, but also poorly defined.

- The PI has made a good effort to bring together an expansive and knowledgeable team, however they have not produced any new ideas or results, so the effort does not appear to be well managed or coordinated.
- It appears as if the teaming is well considered, and tasks are well assigned. One wonders about the capacity of the National Laboratories to do successful polymer synthesis, especially these polymers. They probably need to find additional talent to pull off the polymer engineering tasks, with the understanding that synthesis of polymers is just the beginning of the process required to make useful materials.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- Next steps are logical and include modeling and synthesis followed by characterization and testing.
- If Milestone 1, ORR catalyst demonstrated with polymer coated electrodes, is not accomplished, then Milestone 2 cannot be met, and serious consideration to ending this project must be given.
- The future work has little chance of eliminating barriers based on the significant challenges of the system presented.
- No specific plans to take on limitations with packing density, turnover frequency, performance, or durability are presented. Similar systems have been studied in detail without addressing these limitations.
- There is no quantifiable approach going forward. Most of the talk was spent as a Gordon Conference format giving the general physical and chemical background to their ideas.
- There is much to do, and the targets are set high.

Strengths and weaknesses

Strengths
- It is a fundamental research project of scientific curiosity and interest.
- The role of polymer catalyst interaction and factors controlling their interaction are worth further study.
- The project has a large, talented team that is schooled in the classic ideas of polymers, and have been brought together to make a breakthrough in catalysis.
- The people are excellent, and they have access to quality tools and appear to have adequate financing.

Weaknesses
- Project management seems to be relatively weak.
- Minimal results have been produced and the approach is unlikely to succeed and be useful to the fuel cell industry for the next 20 years.
- The systems presented operate biologically under very different conditions than those for fuel cell systems. It is unlikely that much beyond increased fundamental understanding can be gained.
- The project is very expensive, unproductive, and poorly organized.
- The technical progress is non-existent.
- The testing regimes are not appropriate. So, a key weakness is that, as crafted, this project will not gain much new knowledge.

Specific recommendations and additions or deletions to the work scope

- Testing has to come first to achieve performance; probably most specimens are not worth testing (discard junk quickly). Then there needs to be experiments conducted that carefully expose the test specimens to highly controlled testing conditions, protecting the specimens from harmful (say, high voltage or toxic) events. A significant number of parallel tests should commence to assure reproducibility. After specific testing times, some fractions of the specimens should be extracted and thoroughly examined, so that an understanding of time-dependent changes is garnered. That is, the experiments need to determine normal aging processes, including those that might, for example, result in time-dependent improved performance. A thorough investigation of totally dead test articles might be useful, but usually the "bullet hole" is pretty apparent, and not much is learned by doing that (The totally dead cathode shown by ORNL should invoke only the question, "who killed this fuel cell?").

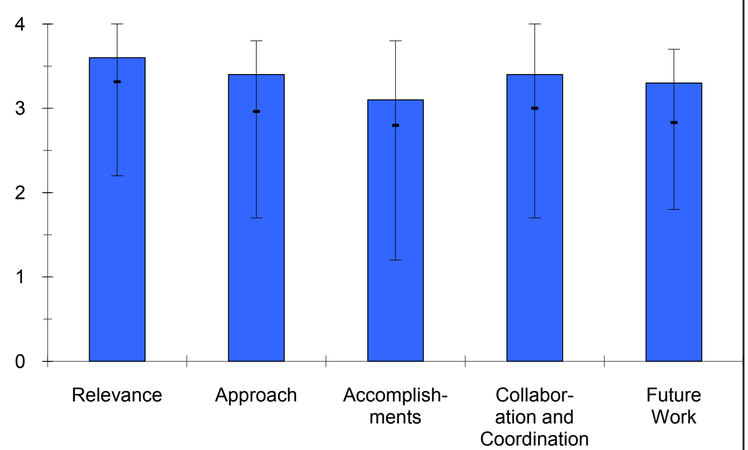
- Very early go/no-go decisions should be made on materials for those that demonstrate theoretical feasibility, based on packing density and turnover rates and some sort of durability under relevant conditions.
- The project should be cancelled or scaled back to ~$300,000/year either immediately or after their review next year.
- It is not too late to refocus the testing plans that should be done. There could be some interesting homogeneous (flow battery) type systems that could work in alkaline conditions, and those might be considered in the longer term.

Project # FC-12: Polymer Electrolyte Fuel Cell Lifetime Limitations: The Role of Electrocatalyst Degradation
Deborah Myers; Argonne National Laboratory

Brief Summary of Project

Project objectives are to: 1) understand the role of cathode electrocatalyst degradation in the long-term loss of polymer electrolyte membrane fuel cell (PEMFC) performance; 2) establish dominant catalyst and electrode degradation mechanisms; 3) identify key properties of catalysts and catalyst supports that influence and determine their degradation rates; 4) quantify the effect of cell operating conditions, load profiles, and type of electrocatalyst on the performance degradation; and 5) determine operating conditions and catalyst types or structures that will mitigate performance loss and allow PEMFC systems to achieve the DOE lifetime targets.

Overall Project Score: **3.3** (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- This project is critical to the DOE R, D&D objectives. There needs to be a deeper understanding of the failure mode associated with catalyst degradation in PEMFCs.
- The project objectives of understanding and mitigating performance losses defined by catalysts properties are directly relevant to the DOE catalyst durability targets.
- This is a very relevant project, specifically because the group is looking at the durability of electrodes that have not really been durability tested before.
- The barriers “Durability, Cost, and Electrode performance” are well addressed.
- The project is highly relevant to further increase the lifetime of PEMFCs. Catalyst and catalyst support degradation are the major life-time limiting factors for fuel cells. Enhanced lifetime contributes to cost reduction as well.
- The project is we focused, has good partnerships, and theory plays an important part.
- The project focus on catalyst degradation, cost and performance is well aligned with DOE objectives.
- It is an important effort to develop an understanding of Pt/C degradation and performance loss. The efforts should address the Pt alloys used for fuel cell systems.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- The approach is clear, logical, and well thought out. I am concerned about the scale of the work fitting into the timeline. It may be necessary to focus on some key tasks.
- There is a strong combination of experiment and modeling work in this effort. The systematic approach is well thought out and looks very good.
- This is a nice, simple, straight-forward approach with a number of critical variables planned; however, in studying the various electrodes, I think it will quickly become very complex. I would advise the group to look at both overall effects on the electrodes, as well as the morphological changes to the actual material. In a true fuel cell environment, the changes in activity associated with morphological changes may be overshadowed by membrane-electrode interfaces or water management, e.g., “whisker” penetrating the membrane over time in nanostructured thin film (NSTF). Additionally, I would advise the group to monitor the state of the ionomer in

the electrode ink, as this may also affect electrode durability. Again, these comments may fall under the other durability projects, but hopefully the groups will work together.
- The approach encompasses a broad range of well-selected methods. Cell degradation tests are well combined with *in situ* and *ex situ* analytics, as well as fundamental materials properties studies. Modeling exceptionally complements the approach. For the *ab initio* modeling, it may take longer than the project duration to devise optimization strategies for the catalysts. Yet, from this method, the most fundamental impact on catalyst optimization can be expected if pursued persistently.
- The milestones and decision points are appropriate.
- It might be rather ambitious to develop a cyclic voltammetry model and oxygen reduction reaction (ORR) catalyst layer performance model parameters by September, 2010, but with the basic code developed and studies under way to determine the relevant input parameters, this may be accomplished.
- ANL describes novel catalyst synthesis methods. It identifies degradation modes. *In situ* and *ex situ* studies have been carried out. Modeling work supplements the testing very nicely.
- The approach appears to be systematic.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- The initial work presented is excellent and is a very good start.
- Good technical progress appears to have been made already. ANL has produced a lot of experimental data early on, as well as a sophisticated kinetic Monte Carlo (KMC) model to predict behavior in a fuel cell environment.
- The project is just getting started, so there is not too much to discuss at this point. The experiments performed to date have generated interesting initial data, but much more work needs to be done.
- The project results are impressive for the short time the project has been in place, underlining the pre-existing skills of the project partners.
- It was not clear what the pressure was on slide 12.
- On slide 11, it appears there is something different going on with the 7.1 nm particle size sample. There is no initial decay and no long-term decay approaching the mass activity of the 12.7 nm sample such as the smaller initial particle sizes exhibit. This behavior was never explained. It could be that there is a critical size where the initial decay occurs, and if that were the case, I would also like to hear why that is, as well.
- It will be interesting to see what you find out for Pt_3Sc and Pt_3Y, as we have tried Pt_3Y without any real improvement.
- The KMC model will be interesting if you can obtain fuel cell parameters to incorporate into it that represent all the degradation modes.
- ANL determined the effect of Pt particle size on degradation rate. Smaller particles led to a faster degradation rate.
- Anomalous Small-angle X-ray scattering (ASAXS) and transmission electron microscopy have been carried out before and after degradation. The effect of relative humidity on degradation was determined. Low relative humidity led to low degradation. Rapid progress had been made in less than a year.
- The explanation of data on page 11 misses an important point. While the surface area of the small initial particles degrades to that of the particle with larger (7.1 nm) surface area, the mass activity of the degraded smaller catalyst is much less than the mass activity of the 7.1 nm catalyst. An explanation would be welcome. It appears there must be a specific crystallite orientation for the 7.1 nm material that is not reached by the degrading 1.9 nm or 3.2 nm catalyst, suggesting crystallite size effect. An explanation for that would also be welcome.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- This project is relatively small and does not seek to involve as many National Laboratories as possible. There are two key industrial stakeholders involved and that is a preferable position.
- There is an extremely talented group of collaborators.
- The activities of all partners look well integrated at this point in time; e.g., the *ab initio* results shown are definitely relevant for the industry.
- There is a well-integrated team.
- The tasks for various team members are well laid out. The team assembled is very strong.
- The program has brought in a good mix of industry, academic, and National Laboratory contributors.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The proposed plan of work is very good. I am concerned that it may be too much to complete in the allotted time. This issue may become clear in the future, in which case I would recommend a focus on task three.
- Future work builds on the current experimental and modeling progress and extends the effort toward platinum alloy catalysts.
- The proposed plan for future work was in general good; however, it did lack specifics. This lack of specifics was most likely because the project was just starting and the durability groups were still distinguishing themselves.
- The tasks are structured along the skills of the partners. The longer this complex project will be going on, the greater the risk will be that the tasks get or stay less integrated. A strict project management is advised to keep the project on track.
- I agree with the first critical assumption. There will be process variables in the membrane electrode assembly (MEA) studies that will not be applicable in the aqueous studies such as diffusion/current collector issues, compression, crossover and shorts, etc., and verifying the relation between the two by modeling is prudent.
- Proposed future work is well laid out, with detailed description of synthesis, modeling, and MEA test plans.
- It does not appear that this team is going to answer the question about decreased surface area and mass activity for *in situ* decrease in surface area versus as-prepared larger surface area.

Strengths and weaknesses

Strengths
- A clear plan to deliver has been established that incorporates a manageable project scale and a good approach.
- There is a talented group of collaborators.
- The project is focused on the understanding of fundamental fuel cell durability barriers.
- The approach is planned and very systematic.
- The team working on the project has highly skilled individuals and they know, in general, what they want to accomplish.
- The project incorporates a broad approach and skilled project partners.
- This is an important project that could answer some very key fundamental questions about fuel cell degradation mechanisms. It appears to me to be a very sound approach to solving this long-term decay problem.
- The effect of particle size on degradation rate (RDE and MEA) is backed up by ASAXS results.
- The team has access to electrochemical and analytical methods, which are necessary to resolve the research problems.

Weaknesses
- The scope is perhaps a little too ambitious to be able to complete all planned tasks within the timeframe.
- The plan is missing some details.
- There is a high level of complexity.

- Viable modeling will be very important, which means valid parameters for the major processes are needed soon.

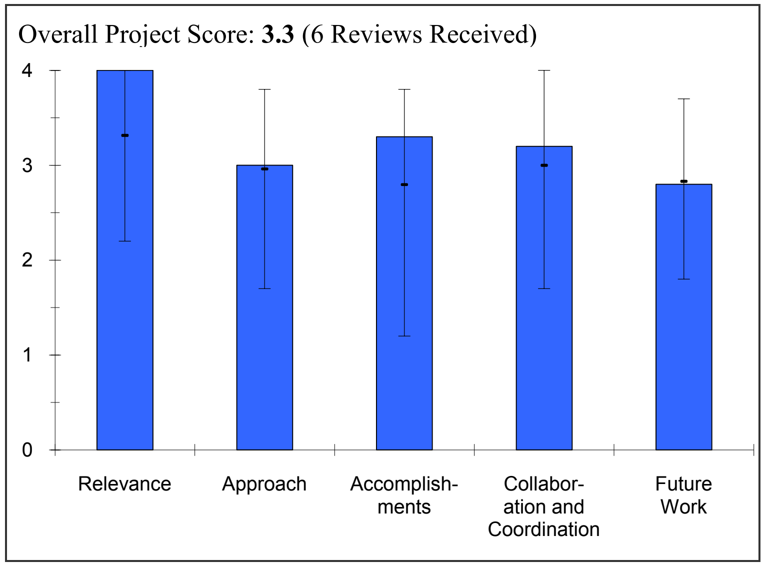
Specific recommendations and additions or deletions to the work scope

- As far as determining the effects of contamination, using $HClO_4$ is a good starting point; however, I would also include sulfates. There have been a couple of papers looking at the effects of sulfates on Pt/Vulcan electrodes with high loading. It would be interesting to study the effects on low loaded novel electrode materials.
- It was not clear how the KMC code would predict Pt nanoparticle evolution at fuel cell conditions. It is not clear if the model is independent of experimental data and is just predicated on first principles. It would be worthwhile to figure out how this model would predict catalytic activity as a function of surface area and/or particle size. If the model is not able to predict catalytic activity as a function of surface area or particle size, then it should not be a part of the program, unless it serves some other purpose.

Project # FC-13: Durability Improvements through Degradation Mechanism Studies
Rod Borup; Los Alamos National Laboratory

Brief Summary of Project

The objectives of this project are: 1) identification and delineation of individual component degradation mechanisms; 2) development of advanced *in situ* and *ex situ* characterization techniques for analysis of fuel cell component degradation; 3) quantifying the influence of the operating environment on different fuel cell components; 4) degradation measurements of components and component interfaces; 5) elucidation of component interactions, interfaces, and operating conditions leading to cell degradation; 6) individual degradation models of all fuel cell components; 7) development and public dissemination of an integrated comprehensive model of cell degradation; and 8) identifying methods to mitigate degradation of components.



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Understanding the fundamental degradation mechanism is vital for the successful development of highly robust and price competitive materials, which in turn are vital for the successful commercialization of the fuel cells.
- Degradation due to interactions among components is an important area of study, ultimately leading to advancing DOE's goals. This topic is missing from many other studies. The final product is a model, not physical deliverables *per se*.
- The program is relevant and addresses the barriers: "Durability, Cost, and Electrode performance". It includes durability with cycling.
- This project is clearly relevant.
- Cost and durability are the two vehicle-related barriers to polymer electrolyte membrane fuel cell (PEMFC). commercialization. This project is scoped to investigate nearly every mode of PEMFC performance degradation. For this reason, the project is entirely relevant to DOE's objectives.
- LANL seeks to study durability on a fundamental component basis. In other words, LANL is not seeking to derive empirical relationships for durability (or "acceleration factors") based on one particular technology that may be irrelevant to future commercialization.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Obviously, with the technical competence of the PI and all of the various subcontractors, the approach is top notch. Doing the fundamental characterization from scanning transmission electron microscopy (STEM) and transmission electron microscopy (TEM) all the way to *in situ* fuel cell testing is vital for success. My only major concern is the shotgun approach of trying to do everything, which could lead to a dilution of heads, dollars, and effort. Perhaps a more focused approach may be necessary. Perhaps to help clarify this, a Global Gantt Chart should be created, clearly stating who is doing what and when.
- The project has a good general approach to performing the R&D, but, with a team this large, a flow chart showing how information and materials is supposed to flow would be beneficial.

- It appears electrodes and membranes are the prime focus.
- There is a clear and systematic approach that is well oriented at real-world conditions.
- The work is very comprehensive, if not too comprehensive, for one single project. The different degradation mechanisms addressed are not necessarily interrelated and could be addressed in single projects. The objective to "quantify the influence of inter-relational operating environment between different fuel cell components" needed more specific substantiation. The overall objective of this project is hard to identify, even more so as it includes electrode production methods in the applied science tasks.
- My key question is the integration with the other durability projects. I was very glad to hear that a working group has been formed and met during the review meeting. I wouldn't mind some overlap between the various durability groups--multiple teams performing similar experiments and reaching the same conclusions will give us a lot of confidence in the outcome. At the same time, complete duplication of effort is not good, and if each of the durability efforts can have their own area of focus we will get the most out of the DOE investment. It is too early to tell--I hope next year we will clearly see that the different groups are effectively coordinating their activities.
- At this stage, the approach still suffers by the size of the scope. Some prioritization should be given to the different activities involved. For example, after studying catalysts, membranes, gas diffusion layers (GDLs), and plates, it remains to be seen how relevant a study of seal and seal degradation will be.
- The project will benefit from including a modeling effort, which will help to limit the experimental scope.
- The project has a considerable amount of characterization techniques at its disposal. It would be good to see a preliminary plan as to what the work flow will be.
- The use of low catalyst loaded membrane electrode assemblies (MEAs) is excellent.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The amount of results and progress (some from before the project began) in all of the various subprojects is clearly stated and at a very high level. Especially of interest is the work being done to understand the inconsistency between electrochemical area (ECA) and fuel cell performance and how using different solvents in the catalyst-coated membrane (CCM) fabrication leads to very different fuel cell fuel cell polarization curves.
- LANL has produced significant findings for solvent-dependence on catalyst ink and MEA durability.
- It is hard to really judge the project because it is only six months old; however, results so far promise that the project is headed in the right direction.
- The progress is hard to assess since the project start date is not provided in the charts. The results in catalyst degradation owing to freeze cycling and the carbon corrosion results, for instance, do not seem to be interrelated, though each single result may be good. This leads to too superficial a description of the single effects throughout the presentation.
- I particularly liked the “applied science” task on solvents used to fabricate electrode structures. I think it's relevant, and should certainly be continued with other high boiling point solvents and with input from groups that directly apply catalyst structures to membranes, as opposed to the LANL decal method. I would be particularly interested in seeing what fraction of these high boiling solvents like glycerol are left behind in the electrode structure (not necessarily intact as glycerol--there are a number of acid-catalyzed decomposition/polymerization possibilities). The freeze start and carbon corrosion results also look promising, although it is far too early to know that these investigations will lead to effective mitigation of these problems.
- The project reporting would benefit from further notations of experimental details. For example, when comparing two different GDLs identified as "cloth" and "paper," it would be good to know if they had the same microporous layers (MPLs).
- The systematic evaluation of how half-cell potentials affect carbon corrosion is very well designed, but more useful knowledge would be obtained if the corrosion rates were also linked to properties of the carbon; e.g., graphitization level, agglomerate size, etc.
- The project should aim to extract further meaning from the data. Performance losses that trail electrochemical area (ECSA) losses are known. Investigating whether lower ECSA loss would affect kinetics due to underutilization of the catalyst, yet still affect mass transport due to change in MPL surface energy, would be beneficial. Phenomena like these need to be explored.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Collaboration between the partners exists and is ongoing; however, it is not readily transparent on who is doing what and where. It is my recommendation that the partners' responsibilities are clearly labeled in the presentation to make it more evident.
- In this particular presentation, it is hard to gauge what was done before the project and what will be done during the project and by whom.
- This is a broad group covering National Laboratories, one university, and two industrial concerns, with advanced modeling and characterization.
- Collaboration and dissemination look excellent.
- Engaging relevant industry is good.
- Though each of the activities looks worthwhile, the whole project leaves the impression of too little overall coordination.
- It looks good on paper—next year's review will tell the real story.
- The collaboration planned is extensive, but it primarily involves the National Laboratory community. Industry is represented by a stationary / material handling vehicle application original equipment manufacturer (OEM), Ballard Power Systems, and by the MEA supplier, Ion Power. Collaboration with automotive stack OEMs may be helpful.
- It may be useful to compare supplier or in-house catalyst layer properties versus those from other suppliers to check how representative catalyst layer properties are to other commercial catalyst layers.
- Collaboration has created an almost unmatched set of characterization techniques at the project's disposal.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Future work focus is largely on individual components. I am hoping the future will show more on interaction effects. I applaud the future topic of studying the impact of mechanical compression on the MEA and durability.
- The future work is no logical continuation of the results presented, yet it introduces a very important but also very different topic, the impact of mechanical loads to degradation.
- The parametric aging studies are needed, but I am very concerned about whether they will ever lead to a model that can predict durability for a new material or operating condition. It will be easy to look at the data and say what does not work, but it will be harder to predict what may work. Without a predictive model, I do not see this effort shortening the development cycle significantly. The presentation mentions "science-based degradation models," but we heard no details about this effort.
- The scope needs to be further refined and prioritized. At this moment, the project seeks to investigate every type of performance degradation.
- For prioritized studies, it would be good to see a deeper plan. At the very least, the initial stages of some parts of the research (applied science for electrodes excluded, since this has begun in earnest) should be defined so as to provide a starting point. Experimental designs could be presented showing expected control factors (operating conditions), response variables (voltage, material properties), etc.

Strengths and weaknesses

Strengths

- World class scientists are on this project doing the fundamental characterization from STEM/TEM all the way to *in situ* fuel cell testing, which is vital for success.
- LANL has assembled an outstanding team that was recruited for their complimentary specialties.
- Novel approach of looking for interaction effects and interface degradation.
- LANL has already identified the impact of electrode ink solvent and durability, which is a surprising and significant finding, along with the analysis of why this may have an effect.
- There is ample relevant background knowledge.
- The project has very relevant research topics and renowned partners.

- There is a good team with the capabilities to get a host of useful data.
- There is an immense amount of experimental capability.
- The project intends to use low catalyst loaded MEAs where possible, which reflects the direction of the technology.
- A modeling effort should help to contain the experimental needs.
- The project has considerable capability for developing systematic studies. The study of carbon corrosion, both with Pt/C and Pt black is a great example. The studies of electrodes prepared with different ink solvents, in concert with SANS characterization, is another great example.

Weaknesses

- The shotgun approach of trying to do everything which could lead to a dilution of heads, dollars, and effort is a weakness, and perhaps a more focused approach could be beneficial.
- There is a lack of clarity as to who is doing what with the various subcontractors.
- There needs to be a flow chart of how materials and information flow between the numerous groups.
- There could be a “one size fits all” degradation model that would benefit a highly diverse fuel cell community.
- Too many research topics seem to be patched together. The future work is inconsistent to the previous work.
- LANL must show effective coordination with other durability projects.
- The synthesis of results into a predictive model is uncertain. No details about the modeling tasks were presented, so it is impossible to know what is planned.
- The project scope is large. There is a need for prioritization and for further definition of experimental designs.
- Reports of experiments should contain all details, including operating conditions, material sets, and cell assembly parameters, if needed.
- The collaboration is National Laboratory-heavy. More extensive industry collaboration may be useful. One specific example would be in terms of defining what GDL degradation occurs from light-duty automotive drive cycling, in terms of material properties.

Specific recommendations and additions or deletions to the work scope

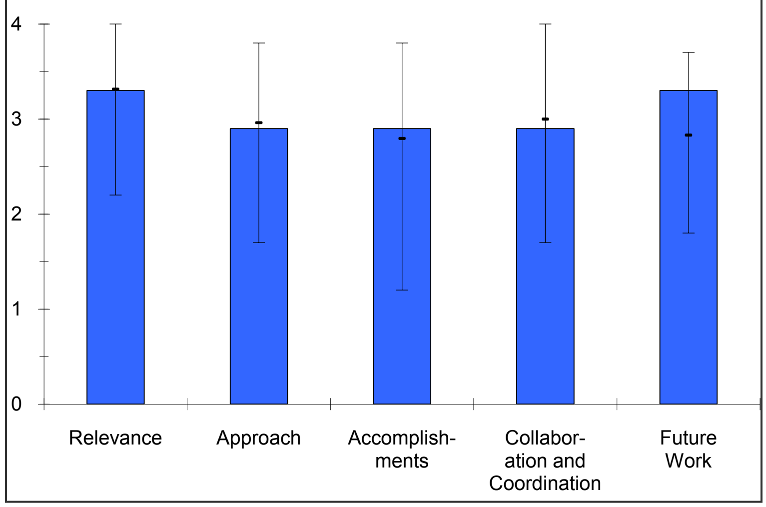
- Consider streamlining and consolidating some of the projects.
- Make the work of the collaborators more transparent. Perhaps to help clarify this, a Global Gantt Chart should be created, clearly stating who is doing what and when.
- The reviewer is looking forward to the output from this prestigious group.
- The scope is fine, but overlap with other durability projects must be considered.
- It may be best to truncate a project after more progress is made to see where efforts can be most effective.
- LANL needs to maintain focus on going beyond cell-level characteristics and making sure to drill down to relationships with material characteristics.
- A water transport model (from such a project) would help in the event that realistic degradation of GDL material properties is known. Then perhaps a model would be able to predict whether performance losses could be expected from the observed material changes.

Project # FC-14: Durability of Low Platinum Fuel Cells Operating at High Power Density
Scott Blanchet; Nuvera Fuel Cells

Brief Summary of Project

The objective of the SPIRE program is to study decay mechanisms and identify strategies to assure the durability of fuel cells capable of achieving DOE's 2015 cost target. The most significant enablers for achieving stack cost are increased power density and reduced platinum loading. The technical approach of the SPIRE program is to elucidate the critical durability mechanisms for a stack operating at a power density and platinum loading that can achieve DOE's 2015 cost target.

Overall Project Score: 3.0 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Cost and durability are among three of DOE's most important targets.
- The project is truly relevant for achievement of durability and cost goals.
- The project is focused on meeting goals and supports the overall modeling effort.
- Understanding durability at high current density operation is important to enable the achievement of DOE cost and durability targets. Nuvera must be careful that projects provide universal value and not just value for their flow field design and materials set. I was glad to see that the plans included testing of more conventional plate designs.
- The project is looking at the durability of fuel cells using current technology, but operating at conditions necessary to meet the DOE 2015 cost target.
- The project is very relevant to the DOE objectives to reduce the cost of fuel cells to meet the DOE target of $30/kW.
- Achieving durability and performance at the DOE cost target (2015) are two factors of critical importance to moving the technology to commercialization. This is a good program, but the presentation was difficult to follow and the single cell approach, with a claim that it is not an isothermal cell, was hard to rationalize.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- Reducing the cost by reducing platinum loading is planned. There is, however, no proposal for reducing the platinum loading.
- The project's approach to performing the R&D is pragmatic, straightforward, and well structured: good!
- I have serious questions concerning correlating sub-single cell testing to stack testing (heat rejection, flow concentration, etc.). The Principal Investigator (PI) presented neither data that indicates testing will be valid nor any information on the number of samples to be tested or statistical sampling (although some information was provided in supplemental slides).
- The proposed durability cycles cover reasonable ranges of current density and relative humidity (RH). I was pleased to see Nuvera take advice from the FreedomCAR (Cooperative Automotive Research) and Fuel Partnership's Fuel Cell Technical Team (FCTT) and run more testing at 2 A)/cm^2, rather than focus on 3A/cm^2. I'd like to see testing at higher temperatures included, but I am concerned that the available material set cannot

handle it. It is imperative that postmortem analysis of the membrane electrode assemblies (MEAs) used in these studies be shared, or results will be of little value to the community. W.L. Gore & Associates (Gore) does not have a great track record of sharing such analyses. The degradation model developed must also be universal to a range of materials sets to be valuable.
- Interesting approach to optimize: 1) power density and 2) lower platinum loadings to achieve the 2015 cost target and then analyze durability under those conditions.
- The project's use of multiple cell designs is good and extends the relevance of the project.
- The PI has a planned, integrative approach to selecting relevant model parameters phenomenologically, and it seems like a good approach.
- The approach seems systematic and well planned.
- Nuvera is taking a bold approach to reduce cost by operating at higher current densities and low platinum loading. This project is investigating the durability of low loaded platinum electrodes at the higher current densities. The work is balanced between modeling and experimentation. The project is developing a decay model that will attempt to determine the impact of current and local conditions on the catalyst. Single-cell experiments will be conducted with different material sets over different test cycles to accelerate degradation.
- Nuvera's interpretation of low loading appears to be different from most interpretations: low loading at a targeted power density. Nuvera may be onto something in using higher platinum loading per cm^2 while achieving sufficient power density to achieve the total cost target.
- The ideas of using a single cell area will be very difficult to provide the corresponding thermal distribution in the cell.
- The independence on compression (slide seven) was demonstrated by Ballard Power Systems five to ten years ago.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- The initial activities seem adequate.
- The project is only half a year old, but achievements and arrangements among the partners look convincing.
- The project is reasonably on schedule. One major accomplishment is the development of a sub-single-cell test jig.
- It's too early to judge Nuvera's progress. To date, Nuvera has shown the ability to run some of the durability protocols. Until durability results and analyses come in, there's not much to base a ranking on.
- Nuvera developed a test fuel cell to simulate a realistic stack cell to share with partners.
- Nuvera is developing a phenomenological model to describe observed fuel cell durability.
- Progress has been good for the project which began in October 2009. A single-cell test fixture has been fabricated and tested. With this test fixture, Nuvera expects to be able to preserve local gradients seen in a full area stack. The performance model has been verified against Nuvera data. The test matrix has been defined.
- It was not explained if *in situ* performance mapping was a model or actual experimental data (page 11). If it was actual data, Nuvera should explain how it was generated. If it was a model, Nuvera should explain how it validated the model. The single cell does not have segmented areas, so it would appear the data is a model. The problem was they put data-point markers in current density and MEA RH plots rather than smooth lines. I took the data point markers to represent data.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.9** for technology transfer and collaboration.

- A strong, collaborative effort is planned.
- The small group of competent partners promises to be very efficient.
- Nuvera has strong national laboratory partners. As mentioned earlier, it is unclear if their results will be meaningful.
- Dependence on Gore limits material sets to expanded polytetrafluoroethylene (ePTFE) supported membranes and dispersed carbon catalysts. The interaction with FCTT is encouraging. There is concern that this project could become a mechanism to fund Nuvera's Orion Stack development.

- The collaboration with national laboratories is good.
- Collaboration partners are very strong and there is good evidence of effective interchange among the partners. Nuvera has incorporated suggestions from the FCTT into their future plans.
- The collaboration team has several strengths. It appeared that the only efforts reported are for Nuvera. It is unclear what the Los Alamos National Laboratory (LANL) did during this seven month period. I saw supplemental slides referring to LANL, but nothing in the main body of the presentation. Similarly Argonne National Laboratory (ANL) work was not presented, although supplemental slide 21 indicates thermal modeling was done, but the results were not presented.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- A call for a "durability task force" seems appropriate.
- The project will produce a large quantity of test data; however, a better definition of how test data will be analyzed is needed.
- I'd like to see temperature explored more explicitly.
- The inclusion of the impact of different materials (catalysts, membrane, electrode) properties should be included to test the universality of the models.
- Future work will build on what is learned from accelerated stress tests (ASTs). It is a good plan to focus the project on the most important parameters to optimize for iteration back into the final planned testing and development.
- Future work plans are well laid out. If successful, the project output (validated model and data set) should be useful to the community, if the impact of different cell architectures can be elucidated.
- The future work is going to be the most interesting part of this program. The plan looks ambitious, but possible. There was little discussion about catalyst degradation or membrane degradation.

Strengths and weaknesses

Strengths
- The project has a good team with good funding.
- Nuvera has strong national laboratory partners.
- The ability to get accelerated durability data using DOE recommended protocols on stack and small scale platforms with different flow filed designs is a strength. Nuvera is considering heat rejection constraints in the test matrix. Nuvera's tie-in with LANL for their AST program is also a strength.
- Multiple cell platforms for testing extends the relevance of the project to non-Nuvera cells. Nuvera has good national laboratory partners.
- MEAs are being provided by Gore; commercial and pre-commercial materials (down to 0.2mg total platinum loading) are being provided.
- Both land/channel and open flow field configurations will be tested.
- Single cell testing will be done at LANL and stack testing at Nuvera.
- ANL will develop a durability model.
- The cell stack developed by Nuvera appears to be very robust and the concept of operating at much higher CD and lower voltages is very interesting.

Weaknesses
- Nuvera has no plan for platinum loading reduction.
- The correlation of sub-single cell to actual stack performance.
- Without knowing the specific MEA materials sets used, the value to the fuel cell community will be limited. The depth and details of material characterization before and after testing is not clearly defined. The approach to develop a predictive life model is not clearly described and is difficult to evaluate.
- Durability findings may be specific to the cell platform and may not apply as broadly as hoped.

- There is some uncertainty regarding the ability of the decay model to account for and capture the relevant decay mechanisms. Not sure how the temperature effects on durability will be captured. Operation at higher power densities will tax the ability to reject waste heat, at least in conventional vehicles.
- It may be difficult to maintain materials characteristics (carbon support, ionomer, platinum particle size, carbon/ionomer ratio, etc.) while varying catalyst loading.
- The single cell has a low probability of yielding a true temperature distribution. The cell will most likely be isothermal and need to have the temperature varied in a series of experiments to map the performance profile.
- The lower voltage of the high current density stack means more current and bigger buss bars and a lower voltage to the power conditioner; both of which are considered negative. I would like to see an explanation of why Nuvera does not consider the lower voltage to the power conditioner and the larger bus a negative.

Specific recommendations and additions or deletions to the work scope

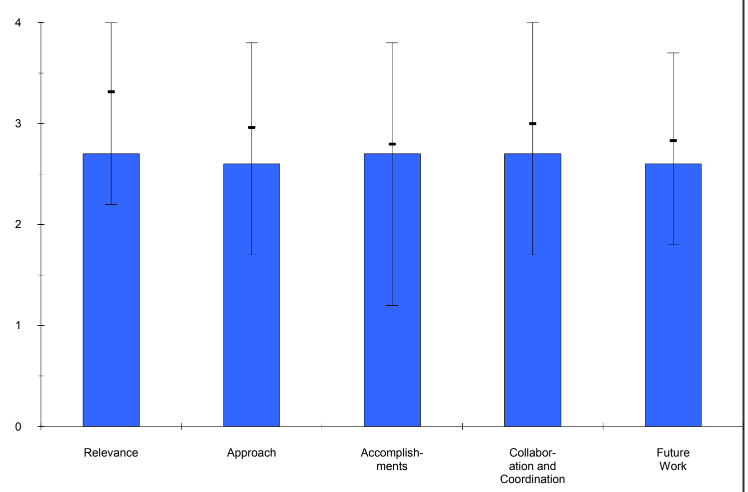
- Need to show, with data, that approach of comparing sub-single cells operating at high current density within the test jig to stack data has any validity.
- I'd like to see temperature and MEA material type explored more explicitly.
- Experimental results and detailed model should be published in a peer reviewed journal.
- The thermal issue will be the biggest for original equipment manufacturers. Even at 2 A/cm^2, heat rejection will be an issue. Nuvera should plan to determine sensitivity to current density coupled to temperature.
- Consider how to relate the thermal behavior of the different architectures (land/channel and open flow field).
- Near the end of the project, Nuvera should be encouraged to look at materials (alloy catalysts and/or membranes) that can enable higher temperature operation.
- The team should discuss with ANL (Myers) the electrochemical surface area and mass activity data that indicates little correlation.

Project # FC-15: Improved Accelerated Stress Tests (ASTs) Based on Real World FCV Data
Tom Madden; UTC Power

Brief Summary of Project

The objectives of this program are to: 1) compare conditions and materials in bus field operation vs. DOE ASTs; 2) develop acceleration factors for DOE AST mechanisms and recommend modifications; and 3) identify life limiting mechanisms not addressed by DOE ASTs and recommend new ASTs. Tasks are to: 1) analyze performance data and characterize degraded materials from 2,850 hour stacks in bus service; 2) analyze data and degraded materials run in DOE ASTs (same as in bus stacks); and 3) correlate results for all current DOE ASTs, including platinum group metals (PGM) decay, carbon corrosion, membrane mechanical and membrane chemical.

Overall Project Score: 2.7 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- The need for such a test is outstanding.
- The "real world" data used here to assess and potentially modify the ASTs for automotive applications of fuel cells are limited to high loaded membrane electrode assemblies (MEAs) in heavy duty bus duty cycles. The DOE automotive objectives are for light duty vehicles at much lower loadings. The PI is aware of the risks in missing key stressors and failure modes using this data. Also, stress tests for the other fuel cell applications are not addressed in this project. There may be "real world" data from these other applications that may be more relevant to where that technology is expected to go.
- It is important to attempt to compare on-road testing to accelerated stress tests.
- The project plans to correlate ASTs with observed real-world degradation and to look for gaps in the currently used ASTs. This work is relevant to the DOE's durability targets and could provide an important connection between real-world and laboratory-world testing.
- Certainly, these ASTs should be part of membrane/catalyst/MEA development projects, but just having United Technologies Corporation (UTC) running these tests on commercial materials is of limited use. I wouldn't expect accurate acceleration factors to be derived from them, especially given the variability of the real-world cycles run on them (not to mention busses vs. automotive). These programs should run various cycles to attempt to gauge the influence of temperature, relative humidity (RH), and catalyst loading on durability, as well as the impact on the acceleration factor.
- This UTC project will determine if the DOE accelerated stress protocols can account for the relevant decay mechanisms as seen in bus fleet field data. If so, the accelerated tests can reduce the need for costly durability testing in the field by a factor of three or four. This project is very relevant to the DOE program objectives.
- Product durability is important in commercialization. Clearly, engineering studies that result in enhanced durability are an important part of the commercialization activity, but the results of this study might support only a specific commercial technology.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- A very comprehensive plan is proposed with the real-world fuel cell vehicle (FCV) data to come to the test.
- The reliance upon field data from busses that are made with technology that is old and does not meet the DOE targets is a huge effort. I would prefer that the brass board system they are building be used to test more relevant MEAs or stacks under conditions that more closely resemble light duty.
- It's not clear that one stack is a good representation (good or bad) of typical results. UTC needs to have more data presented on the stack (number of faults, time at idle, etc.) to better judge. No data was presented on the number of cells to be tested and whether there will be any correlation to cell performance; i.e., how do weak cells show degradation as defined by stress tests.
- Significant effort might be required to validate short stack results to full-scale results.
- The project includes comparing real-world bus stack failures to degradation in the laboratory, based on ASTs. If a correlation exists, then the project will continue. Presumably, numerous gas diffusion layer (GDL) and MEAs will be tested, but only one, the MEA by W.L. Gore & Associates (Gore), was mentioned. The stack being used to test is proprietary, so it is not clear how the information obtained will be considered generally useful, as the stack conditions will not be published.
- It was not made clear how the testing will allow for isolating the four different proposed (and "other") mechanisms from each other to enable developing acceleration factors for each of the four DOE test procedures.
- Plans regarding lower catalyst loading and correlating results versus loading should be put into place, if they haven't been already. This will be key for automotive commercial viability.
- The operating conditions of bus stacks (air and hydrogen stoics) was never made clear, as well as the likelihood of anode starvation.
- The modeling goals were ambiguous.
- The approach is good. UTC is gathering real-world degradation data from their bus fleet and is attempting to match the conditions and failure modes in a laboratory environment in a short stack running the accelerated tests.
- The PGM loading in the bus fleet data is higher than the DOE targets. UTC will consider using different materials in the stack to better characterize the performance of newer state-of-the-art materials.
- The real world data may highlight several failure modes, but it may not reflect all modes. UTC needs to guard against introducing new failure mechanisms that are not seen in the real world.
- The project has a go/no-go decision point to determine if UTC can correlate the observed degradation with process conditions.
- The procedures seem obvious and expected.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The beginning of the work seems good.
- Progress against the plan and milestones is fair, but I fear the project is falling a little behind.
- The project appears to be on schedule.
- Go/no-go decision point will be reached in July 2010, after which more solid progress assessments can be made.
- The cell voltage range in the bus operation was not revealed. The plot on slide five doesn't show values on the y-axis, therefore, the severity the real world bus voltage cycles was not presented.
- Regarding slide eight –explain the discrepancy in particle size and electrochemical area (ECA) results between X-ray diffraction (XRD) and ECA tests. How they correlate the measured 15 mV loss at 200 mA/cm^2 is an important question that was never answered. It seems like an ECA loss would result in greater mV loss, whereas an ECA calculated from XRD is more in line with mV loss.
- Referring to slide 11 – an ECA loss of 70%, but only an 18mV loss @ 0.8 a/cm is surprising. Can these results be explained further with fundamentals?
- UTC appears to be making good progress in the short time the project has been funded. Characterization of the PGM decay in the bus fleet has been completed. ECA loss on the cathode is greater than the DOE targets, but the performance loss is less than the DOE targets. More analysis is needed to understand these results. UTC should subtract out the contribution to decay for startup and shutdown.
- The short stack has been fabricated and is installed on the UTC test stand.

- Catalyst decay seen in the accelerated testing at Los Alamos National Laboratory (LANL) agrees with that observed in the bus stacks after 2850 hours.
- The project is in its early phases. The results shown appear typical, similar to many earlier data pertaining to fuel cell performance loss.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Several experts are listed as collaborators.
- Apart from failure analysis at national laboratories, there is no collaboration outside of UTC and the relevance of their analysis of UTC field heavy duty data is limited outside of UTC.
- The project has a strong team; however, more information concerning LANL test capability (comparison to in-house UTC data) is needed.
- Their collaboration with national laboratories is good. It is not clear from the presentation what role LANL will play in this project. Presumably, some of the AST testing will be done at LANL.
- Collaboration is evident and it seems the primary expertise is well leveraged.
- The collaborations are strong. LANL is performing the accelerated tests and the Oak Ridge National Laboratory (ORNL) is characterizing the degraded materials.
- Collaboration with the National Renewable Energy Laboratory (NREL) to compare bus fleet durability with light-duty vehicle (LDV) durability should be encouraged to the extent possible.
- This is an "internal" investigation (internal to UTC), and the collaborators seem more like contractors.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- The future plan reflects the good initial plan.
- It seems to me that the future work planned here is the implementation, within UTC, of an AST to enhance their capability for development of their heavy-duty product.
- It would be very valuable if additional stacks can be included in study. UTC needs better analysis of individual cell (within the stack) performance versus degradation.
- If the project is successful to make a correlation between the laboratory ASTs and the real world, then extensive ASTs will be performed or developed on the a UTC laboratory stack. It is unclear if there will be benchmarking of test data with other cell designs.
- It is not obvious how various experiments are going to be linked together.
- Future work is intended to lead to the development of a system-based test protocol that accelerates all relevant decay mechanisms in bus stacks.
- It is possible that improved understanding might result in identification of "danger signs," or other metrics that result from undue stress on working systems. Accelerated testing can be useful.

Strengths and weaknesses

Strengths
- UTC has a good team, with a good source of real world FCV data.
- The approach for reconciling and validating ASTs with real field data is commendable.
- UTC has a strong team using on-road tested hardware.
- The focus and approach of the team using real world degradation issues from bus fleet is a strength. Connecting the real world to the laboratory world is extremely important.
- Overall, they've done a nice job developing stack materials for their bus application, and this project is indirectly describing the key factors involved.
- The project is focused on providing a method to expedite material qualification for use in bus stacks that can meet the UTC target of 15,000-hour durability.

- The project will determine the applicability of the DOE accelerated test protocols for LDVs to bus operating conditions.
- To minimize the risk that the accelerated tests do not introduce a new mechanism that is not seen in the real world, UTC is trying to make the potential, temperature, RH, and time as realistic as possible. UTC also plans to screen the test results for a “reality check.” The FreedomCAR (Cooperative Automotive Research) and Fuel Partnership (FreedomCAR) Fuel Cell Technical Team (FCTT) indicated a willingness to validate the UTC results.
- The go/no-go decision on task 1 will be made on the basis of whether UTC can correlate the degradation mechanisms to process conditions.
- UTC probably knows its stack well, and UTC has some excellent technical support.

Weaknesses

- The use of high loaded heavy-duty data for this means that the value is internal and does not filter down to meet the DOE research, development and demonstration targets.
- The project has produced questionable test results given only one stack. UTC indicated next-generation stack lifetime approximately double that of stack used in this analysis. If these stacks were tested, it would be interesting to know if whether or not the results would change substantially.
- The real-world stack is proprietary and little of the testing information will be shared. The project would be stronger if a connection to an open cell or stack design could be made.
- It is not clear to me that this is at all viable for light-duty automotive: high catalyst loadings, minimal RH and temperature cycling, no information on the number of starts or stops, air starts, freeze, etc. And it is not clear what the maximum voltage was on the voltage cycles. It is likely a very benign environment compared to what one would expect for passenger vehicles.
- The sample size is limited, will results be statistically significant?
- It is not clear how much of this work will apply to LDVs.
- Developing accelerated tests to project lifetimes for old material sets may not be totally applicable to newer materials and lower PGM loadings. Newer materials may have different failure modes.
- Existing engineering of similar fuel cell systems frequently showed that stack failure (or performance loss) can be the result of component failure, and a failed component stresses the stack in unanticipated ways. Consequently, there needs to be through system-wide system analysis, component by component. Components with short durability need to be exchanged, as required. It is also well understood that degradation processes proceed at a rate that vary with the log of voltage; i.e., a few minutes of poor system control can result in the equivalent of years of degradation, problems that would be totally avoided if appropriate conditions had been maintained. Therefore, accelerated testing indeed might work, but only after there is a considerable knowledge of chemical dynamics at the stack level, and the anticipated durability of all system components.

Specific recommendations and additions or deletions to the work scope

- To be relevant to the DOE and not just UTC, this project should be rescoped to light duty data acquired on itsbrass board with light duty relevant cycling and low loaded MEAs. UTC should partner with their catalyst-coated membrane supplier to carry out this work.
- UTC needs to add more stacks, if possible, and UTC needs to compare cell performance within overall stack and clearly identify stack history (number of faults, percentage of time at idle, etc.).
- Since the bus stacks are proprietary (including the planned laboratory stack for AST), there still needs to be a clear connection made to cells used for laboratory testing using standard hardware. The project’s members should consider other cell/stack designs as well.
- Incorporate FCTT durability protocol (load and RH cycling).
- UTC should be encouraged to work with NREL to compare the durability results from the bus fleet with the durability for the LDV fleets in the Technology Validation Program.
- UTC should consider working with lower catalyst loadings. The higher loadings in the current fleet may mask the effect of airborne contaminants. However, the absence of recovery cycles in the test protocols will be a problem with lower platinum loadings. UTC should reconsider including performance recovery cycles, since this can help keep low platinum materials from being unnecessarily eliminated from consideration.
- UTC may need to run multiple accelerated tests with different stresses and failure modes in order to really get a fundamental cumulative damage model, in order to predict lifetime.

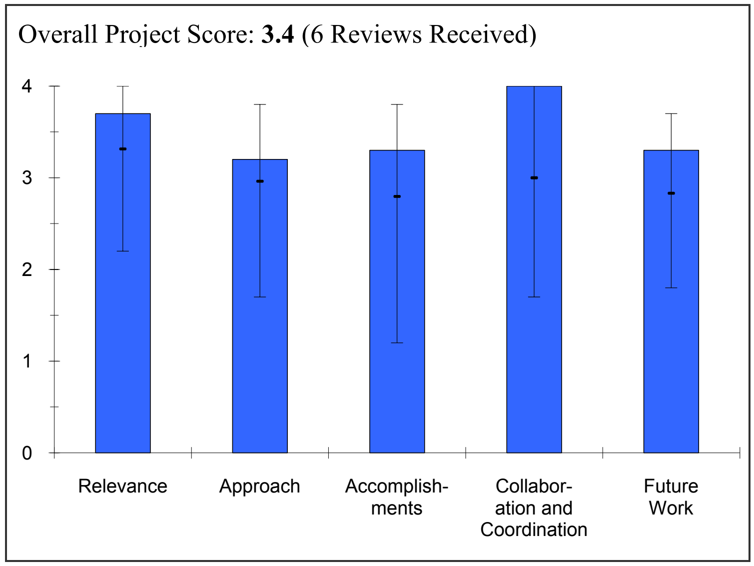
- There needs to be some standard testing done to back up the proposed tests. Certainly, one year of quality testing takes the entire year, and that involves costs. However, without such a baseline, results might be of limited use. Other developers have learned this. Clearly, a company as well-endowed as UTC has necessary resources for product testing, especially because potential customers will be eager to review such results.

Project # FC-16: Accelerated Testing Validation

Rangachary Mukundan; Los Alamos National Laboratory

Brief Summary of Project

The accelerated stress test (AST) allows faster evaluation of new materials and provides a standardized test to benchmark existing materials. The objectives of this project are to: 1) correlate the component lifetimes measured in an AST to real-world behavior of that component; 2) validate existing ASTs for catalyst layers and membranes; and 3) develop new ASTs for gas diffusion layers (GDLs), bipolar plates, and interfaces.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- The objectives of the project are very important to be able to better predict the degradation of the fuel cell (fuel cell) and to propose improved materials.
- The relevance to DOE objectives is well defined. If the project is successful in validating ASTs by comparison with real-world data and understanding mechanisms, then there will be a good foundation for developing mitigation strategies which will help to achieve DOE's durability targets.
- This project is utilizing relevant materials. However, it is not clear yet how degradation in buses correlates to automotive applications.
- Having standardized AST protocols is very useful for the development of advanced materials, and it is important that these ASTs be validated with real-world data.
- This project, while new (August 2009), combined with Tom Madden's project at United Technologies Corporation (UTC) and Rod Borup's project at LANL (many of which have the same collaborators, particularly LANL), is critical to the Hydrogen Program as it evaluates AST for membrane electrode assemblies (MEAs) from Ballard Power Systems pressurized gas bus systems. Further, since only bus systems are available, the program is working with W.L. Gore & Associates (Gore) and Ion Power to develop automotive MEAs and subject them to an LANL-developed accelerated drive schedule to determine if current AST standards are sufficient. ORNL is providing metal stacks and LBNL is performing the modeling. Madden's program at UTC looks at ASTs developed from buses operated at atmospheres of pressure. Therefore, the DOE R,D&D objectives are met with all currently commercially operating fuel cell vehicles, other than forklifts.
- This project is critically important for the fuel cell developer community.
- Validating ASTs with real-world data and correlation will enable future testing to provide more confidence to developers that they are emulating the major degradation mechanisms observed in the real world.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The approach is in accordance with the project targets.
- The parallel approach between "real-world" data and laboratory testing is adequate.
- Real-world fuel cell data is expensive to obtain, so it is understandable that this project cannot rely on a fleet of cars to get real-world durability. However, a bus system may have significantly different stressors compared to a light-duty vehicle (LDV). For this reason, it will be important to thoroughly compare the operating protocols

of the real-world bus with the target operating protocols of light-duty vehicles for which the current ASTs were developed.
- The overall approach is logical and the scope seems reasonable. The use of low catalyst loadings is critical to the development of ASTs. The justification for developing GDL ASTs is not clear, especially if one looks beyond LANL results. Also, the bus data seem limited. It will be interesting to note if the results are statistically significant.
- This project appears to have a good plan to start to validate the ASTs. This will be challenging due to the many differences in real-world applications; e.g., stack and system designs, operating conditions, material sets, etc. Therefore, it is probably good that a single laboratory, LANL, is involved in both AST-validation projects.
- The project is sharply focused on technical barriers, as degradation mechanisms within operating MEAs are not well understood. LANL is trying to develop mitigation strategies, while simultaneously meeting cost and durability targets.
- The approach seems solid, although there is some question of whether applying ASTs developed for LDVs and validating them with fuel cell bus data is the best approach.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The obtained results are numerous and of quality.
- It will have to be checked that the correlation between electrochemical surface area (ECSA) and performance loss is not only true for the tested material as other presentations showed much worse correlation.
- The project is in its first year and the electrochemical-diagnostics data presented demonstrate that the project is off to a good start.
- The team has made good progress for less than a year of work. The relative humidity (RH) cycling results have good correlation with experiments by other groups.
- Excellent amount of results presented for a project that recently got underway. Results are also well presented.
- The program has just started and progress to date has been concentrated on the bus data. Some AST data have been verified, while others, like catalyst cycling, impedance and carbon corrosion, impedance and RH cycling metrics, and characterization need further work to verify or develop new ASTs.
- Despite the recent start on this project (August 2009), it appears as though a significant amount of the laboratory work has already been completed at LANL (unless results presented were from a previous project).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **4.0** for technology transfer and collaboration.

- The collaboration between the partners seems to work well.
- PI should maintain close communication with Ford and General Motors (GM) to make sure that progress remains focused on DOE goals for LDVs. Start/stop durability can be highly dependent on stack design and system protocol in addition to MEA materials. LANL should solicit input from industry and consider implications of different operating strategies while developing a start/stop AST.
- The project appears to be well coordinated, with a good balance of work for the various partners.
- It is excellent that the project includes a MEA supplier, Ion Power, that allows full analysis of this key component. It is unfortunate that there are restrictions on analysis of some key components that may be the actual materials used in fielded power plants.
- The collaborations are excellent; with great partners and other LANL investigators such as Rod Borup, Ion Power, Gore, LBNL for modeling, Ballard, ORNL for metal stacks, and GM provided protocols for cell life testing, etc.
- The PI has assembled an outstanding team, including Ballard, LBNL, Ion Power, ORNL, and Gore.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The proposed work is in accordance with the project objectives.
- The work plan is well laid out and focuses resources on the appropriate activities.
- The impact of temperature and RH on the acceleration factor should be looked at, if it has not been already.
- It is not clear how dropping freeze investigations and substituting forklift data improves the scope of the project. Adding forklift data will most likely add more uncertainty.
- This reviewer feels that if the team stays away from freeze data points while sticking to AST testing by initiating combined mechanical and chemical testing of membranes, catalyst cycling, and GDLs, it will have a successful project. LANL should continue cell-life testing (accelerated drive cycles), correlate ASTs to life data using LBNL modeling (already underway), and develop new ASTs where appropriate, keeping in mind the goal is to develop mitigation strategies while simultaneously meeting cost and durability targets.
- As this project is still ramping up, there is much future work to be done.
- Details of future work were light (only listed at task level), but based on the clear approach of the project they appear to be appropriate.

Strengths and weaknesses

Strengths
- The project is well organized with a good partnership.
- There is a strong team with coordinated experiments that is focused (in part) on relevant materials.
- There is a good plan and a good team, and it is good that additional components (metal plates, Ion Power MEAs) are included.
- Excellent collaborations, goals, and objectives.
- Good partnering with industry.
- The project is focused on a key enabler for fuel cell developers to validate accelerated testing of new materials in the laboratory and proceed to the marketplace more quickly.

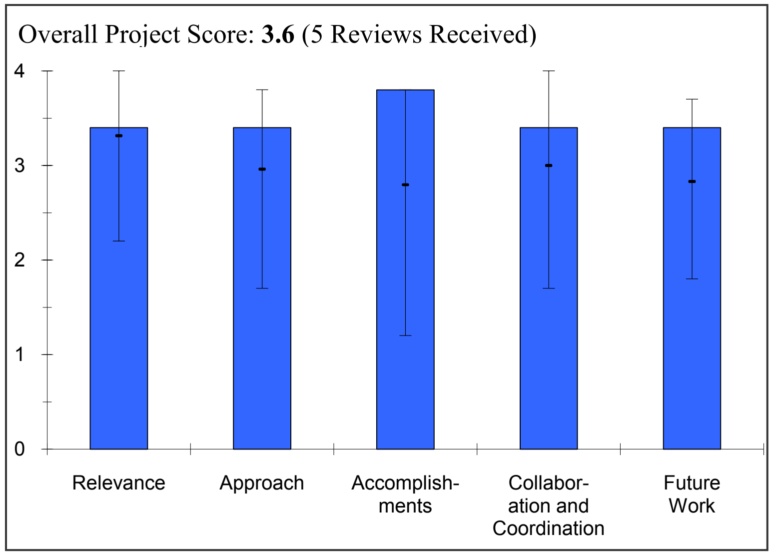
Weaknesses
- Activity on bipolar plates should be more precise.
- Real-world data should deal with a bus application. As the lifetime of systems in buses is longer than that in LDVs, how will it be ensured that the results will also be applicable to automotive cars?
- It is not clear if bus data is relevant to automotive ASTs and this project partly depends on this.
- It is unfortunate that some of the future "field data" will be just from running simulated bus cycles, not real buses.
- It is unfortunate that the catalyst loadings are even higher (1 mg/cm^2) than the other AST-validation project. No real-world data will be obtained on low loaded MEAs.
-
- Using bus on-road data to validate ASTs designed for cars is a weakness.

Specific recommendations and additions or deletions to the work scope

- Stay away from freeze data.
- As freeze data are no more concerned regarding the tech team proposal, analyzing fork lift data is fine.
- As many stationary systems are commercialized today, comparable data should also be obtained under stationary conditions dealing with either neat or reformed hydrogen.
- There should be less emphasis on GDL degradation.
- Forklift data may complicate an already difficult task.
- The project should include additional diagnostics (such as IV curves with different oxygen partial pressures, as suggested by an audience member) to obtain additional insight into source of performance losses, e.g., oxygen transport vs. ohmic losses in cathode.
- Follow the recommendations of the fuel cell TT.
- Recommend partnering with a light-duty fuel cell manufacturer (Automotive Fuel Cell Cooperation, GM, etc.) to obtain related on-road data from cars to do similar validation of the ASTs.

Project # FC-17: Fuel Cells Systems Analysis

Rajesh Ahluwalia; Argonne National Laboratory



Brief Summary of Project

The overall objective of this project is to develop a validated system model and use it to assess design-point, part-load and dynamic performance of automotive and stationary fuel cell systems. Objectives are to: 1) support DOE in setting technical targets and directing component development; 2) establish metrics for gauging progress of research and development projects; and 3) provide data and specifications to DOE projects on high-volume manufacturing cost estimation.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This is perhaps the best fuel cell model developed. The objective of this project is to develop a validated system model and use it to assess design-point, part-load and dynamic performance of automotive and stationary fuel cell systems. This project is critical to the Hydrogen Program and DOE RD&D objectives as it addresses the key targets for performance, system thermal and water management, start-up and shut-down time, transient operation, and cost.
- The relevance is rated "good," not "excellent," for the reason that, unlike many other projects in the program, the results of this project will not be directly useful to end developers of fuel cells. Such end developers already have their own system analyses. However, this project does fulfill an informational role that benefits DOE, and, to that extent, it is relevant. The project feeds information to the cost analyses, and therefore has a "second order relevance" to end developers.
- The PI has been responsive to reviewer feedback and has revised its component selection for this year. The PI's willingness to do so has enhanced its relevance.
- The project has made very good progress over the last few years and offers an integrated-systems look at the fuel cell and importantly contributes to the overall understanding of the fuel cell system problem. This project provides a base for comparing most of the DOE program goals and objectives.
- This project probably has the most value in how it establishes a detailed fuel cell that can be used by Directed Technologies, Inc. (DTI) and TIAX, LLC for their annual fuel cell cost projection updates.
- This project may not be very relevant to the industry partners, as it relates to a generic fuel cell that does not represent (in detail) the system of any particular company.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- The technical barriers are cost, performance, system thermal and water management, air management, startup and shutdown times, and energy/transient operation. The approach to solving these barriers are to develop, document, and make available versatile system design and analysis tools existing at ANL, to validate the models against data obtained in laboratories in collaboration with external organizations, and to apply the model to issues of current interest by working with the Fuel Cell Technical Team and with DOE-requested contractors. While validation of system models still needs to be done, this project's approach has made a considerable improvement in the past year. First of all, the system concepts are much more up-to-date, as evidenced by the

removal of anode humidification and the option to study lower stack inlet pressure. Temperature differences across the stack are now recognized as well. Second, the models now recognize that low power conditions play a role in determining system components and sizes (which applies to air compressors, recirculation blowers, and humidifiers), which has made for a considerable and long-awaiting improvement in the methodology. The investigators are to be applauded for these changes.
- The approach is solid and attempts to bring the best data available and approaches to understand the system issues. The validation of the impurity models and data is very important. The breakout of the different operating conditions for pressure, Pt loading, water management, etc., is informative. I think that the approaches being used are very good and demonstrate a systematic, stepwise evaluation of the system.
- The approach of validating GCTool with in-house testing data is good.
- Interactions with Fuel Cell TT and DOE contractors are necessary to keep this project focused on relevant fuel cells and issues of interest.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.8** based on accomplishments.

- The model has been successfully utilized to operate a stack at 1.5 atm without a cathode humidifier, but with hydrogen recirculation ejectors and a hydrogen blower using 3M's new 20 µm membrane with 0.05 g/cm^2(anode)/0.10 g/cm^2(cathode) PtCoMn catalyst. The membrane results agreed well with 3M's single cell testing. Also, the model predicted the water and air management results with turndown, the size of the radiator resulting in an increase in stack operating temperature at maximum power, except for the S-3 stack. The model also predicted that lowering the full load efficiency to 40% decreases the peak efficiency by only 1% and increases the battery charging mode fuel economy by 4%. There was a general consensus in the room that the project met all of the concerns expressed last year.
- Even though validation still remains to be carried out, excellent progress has been made. Compressor turndown is now defined in the context of surge limitations. The model provides the flexibility of numerous fuel subsystem configurations with dual ejectors and variable geometry ejectors included. Furthermore, the model attempts to provide detail on which power densities ejectors and/or blowers are needed. With proper assumptions, this will be useful to DOE to be able to figure out whether fuel cell developers are using blowers or ejectors or eliminating certain components.
- There is still progress to be made in validating component assumptions, extending the model to other operating regimes, e.g., low temperature, and reviewing battery charging mode assumptions, but it can still be said that a large improvement has been made in this project.
- It was good to see the enthalpy wheel finally removed from the system. The data are defining the compressor expander motor (CEM) module and providing a system view of the effect of catalyst loading. Good progress appears to be made on all topics; however, the level of activity is so high that I am having difficulty assimilating all of the progress.
- Significant updates to the system model were made this year, enabling the system to be more representative of a typical fuel cell in industry.
- Updates were key for TIAX and DTI to have improved fuel cell cost projections.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Collaborators have been expanded to include 3M, LANL, and DTI. The collaboration is appropriate and well coordinated and the partners are full participants. Vairex International need not be consulted regarding the CEM unit.
- The collaboration with two particular partners--3M and Honeywell--is very good, but the PI must be careful not to become too dependent on these organizations. In principle, there should still be inputs from alternative component suppliers (although given the uniqueness of nanostructured thin film (NSTF), alternative suppliers are not necessary for now to understand the catalyst layer).
- The project could benefit from seeking alternative inputs on humidifier membranes. The focus should be on finding performance for lower cost membranes, so this could be done collaboratively with DTI or TIAX.

- The experience of ANL results in recognition of the top groups to team with.
- There is a high level of interaction and collaboration with Honeywell.
- It would be nice to see more direct and/or ongoing interaction with the original equipment manufacturers (OEMs) outside of the FreedomCAR (Cooperative Automotive Research) and Fuel Partnership's Fuel Cell Technical Team (FCTT) forum. The possibility that ANL could model GM's actual next generation system and add value into the process for the subsequent generation is intriguing.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- The future work, of course, builds on prior results. The system analysis supports the DOE/FreedomCAR development efforts. The model has been used to verify the performance of 3M's membrane/catalyst at low loadings and pressure, but collaborations continue with 3M regarding durability of their electrode structures. The future work remains focused on the barriers listed for the project. The project remains critical to the Hydrogen Program and the relevant DOE RD&D objectives, as this is the relevant systems model that others are adapting. It is also the model that DTI and TIAX use for future costing of fuel cell systems.
- ANL has the ambition to take on drive cycle and durability modeling, but further validation and optimization of the present model (most likely at component level) should come first.
- A drive cycle or durability model will require much deeper knowledge of the stack that has been presented here, but might be possible in collaboration with some of the new durability projects. For this, a work plan is in order.
- It is understood that stack performance mapping with 3M single cell data has been done. A prerequisite to some of the future work should be that this performance map should be presented and reviewed to see whether the map can extend to operating regions that might be experienced during transients.
- The future work is consistent with the objectives and there was no information presented that indicates some of the efforts should be terminated.
- Of all the future work, the most important is the drive cycle simulations for durability enhancement. Unless the real-world cyclic nature of fuel cell operation is included, changes to the model could be making dramatic (and negative) impacts on the durability of the system. OEMs will already know this, but it will lead to a modeled projection of cost that is cheaper than what is actually possible.

Strengths and weaknesses

Strengths
- The system model is very powerful and applicable to the work of other collaborators and non-collaborators. Stack and system designers have been brought on board.
- ANL has made the system concepts much more contemporary.
- ANL has begun to model components throughout the range of power density required during operation. As a result, the PI has developed a much better understanding of system component selection based on phenomena that occur at low power operation.
- The PI's experience and the progress this project has made over the last several years demonstrate the continued growth in expertise and fuel cell leadership.
- Backbone of defining a reasonable and detailed fuel cell to enable parallel cost projections to be made by TIAX and DTI.
- Some interesting insights can be learned by exercising the model in various different ways.

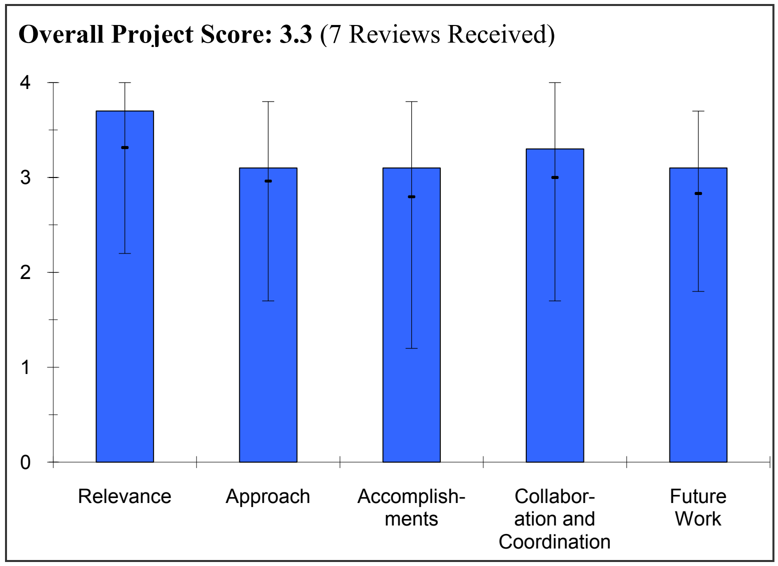
Weaknesses
- Perhaps a little more modeling on cathode air-borne contaminants should be included.
- The model still requires validation in many areas and some system components should be studied further to see whether the representations in the model are the most competitive versions possible.
- Use of 3M NSTF provides a low loading catalyst, but it also implies some limitations in terms of operating conditions, particularly at low temperatures.
- The future work section indicates that the project will attempt to address durability and drive cycles. However, some restraint is needed to be sure the present model is validated and that collaboration on such matters will occur with the new durability projects.

- The model does not represent an actual fuel cell operating anywhere, so complete model validation is not possible.
- ANL needs to make sure model queries are focused on the key issues facing fuel cell developers within the OEM's programs.

Specific recommendations and additions or deletions to the work scope

- No deletions but perhaps a little more emphasis on modeling the effects of cathode air-borne contaminants.
- Other collaborators on system and stack should be added beyond Honeywell and 3M. The project should be able to gain a competitive understanding of what components could be available.
- Durability and drive cycle modeling should be restrained until after performance model is validated.
- Some analysis should be made of the stack performance map to see whether it agrees with a more phenomenologically-based performance map (with science-based relationships with partial pressures, relative humidity, temperature, etc.).
- Some analysis may be of interest to see whether the stoich / voltage relationship from 3M single-cell testing would be applicable for a stack. Single-cell stoich sensitivity is usually different (less sensitive) than that for a full stack module.
- The complexity and maturity of this project makes it hard to evaluate using the AMR format. This project has progressed so far that a separate review is recommended (a review that could take several hours and would include a preliminary report to assist the reviewers' understanding of the project progress).
- Closer collaboration with OEM programs on actual system designs and tradeoffs within the model that will affect future product design would be helpful.
- Make a version of the model to enable analysis of fuel cell forklifts, as this is an area where the fuel cell developers could probably use some help with the issues they are finding from real-world product placement.

Project # FC-18: Mass-Production Cost Estimation for Automotive Fuel Cell Systems
Brian James; Directed Technologies, Inc.



Brief Summary of Project

The overall objectives of this project are to: 1) identify the lowest cost system design and manufacturing methods for an 80-kW direct-hydrogen automotive polymer electroylte membrane fuel cell system based on two technology levels, current technology and 2015 projected technology; 2) determine costs for these tech level systems at varying production rates from 1,000 to 500,000 vehicles per year; and 3) analyze, quantify and document impact of system performance on cost, using cost results to guide future component development.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- It is the only publically available source for discussing costs. DOE needs these programs to discuss future R&D directions and policies.
- The goals of this project are in line with the DOE objectives. This project provides valuable insight into the cost projections of current and future technology. These costs would otherwise be very difficult to estimate based on the limited amount of information that is disclosed by industry.
- This is one of two projects that seek to realistically assess the mass production cost for a prospective automotive fuel cell systems. The highly detailed research of this project is essential for informing overall policy on the realistic prospects that fuel cells will be an affordable alternative to other power plants; e.g., batteries and internal combustion engines. This project integrates the best available engineering and design knowledge it has developed over the years, particularly with respect to the membrane assembly and the balance of plant (BOP), to provide a reference fuel cell system. This focuses planning and elucidates what additional research might be done to further reduce costs.
- The cost model is useful for cost-benefit analysis.
- This attacks key problems for implementation and does so in a well-aligned way.
- This is a continuing program. Directed Technologies, Inc. (DTI), unlike TIAX LLC, uses extensive interactions with industry and researchers to solicit design and manufacturing metrics as their input to their cost analysis. TIAX, on the other hand, uses the latest model from Argonne National Laboratory (ANL) for their cost analysis. Thus, the Hydrogen Program and the DOE research, development and demonstration (RD&D) objectives have two independent cost studies to draw upon. DTI uses the cost study to guide future component development.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Both cost studies are converging on more automotive representative systems. Generally, DTI does a great job of aggregating all the known manufacturing techniques and suppliers. They do a good job of using the limited and highly contrasting information provided to them. If improvement is possible, PI could offer suggestions for advanced, new, or upcoming techniques. The PI should not invest much effort into this analysis or include it in the cost summary since it could be highly speculative, but offering thought provokers would be helpful.

- It is helpful that they switched to 3M nanostructured thin film (NSTF) design in 2009 instead of an average system.
- Assumptions regarding the practicality of "current (2010) status technology" should be more clearly stated in the presentation.
- The approach seems to be good, but it is not transparent. It would be beneficial to know what came out of the talks with industry and what those talks were about.
- The PIs laboriously developed a reference design in consultation with the national laboratories; e.g., ANL, and leading firms developing fuel cells (fuel cells) and their components.
- Well-established methodology (e.g., DFMA) is used to estimate materials and manufacturing costs.
- They are taking a reasonable approach. There are some differences between ANL mechanization and the mechanization used for this study.
- There is a good level of detail in price estimation. It could be better if at least an order of magnitude costing were provided for the excluded cost items.
- They use suitable assumptions where they must.
- They did not look into improvements in supplying industries and assembling techniques, which means estimates of true steady state cost may still be high. This is a place for continuous improvement.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- The model is now entering into the refinement stages. They continue to converge on realistic automotive designs.
- The model has improved the accuracy of its system and performance assumptions since last year.
- For this project, accomplishment is an accurate assessment of the state of the technology, regardless of the DOE goals. They seem to be doing a credible job of it. Their findings are that the DOE goals are “nearly” met for 2010, but 2015 goals are overly optimistic (slide 23). Their continued refinement of the model (slide seven) demonstrates excellent progress towards their charge of pricing the lowest cost design consistent with good engineering practice.
- The major components of cost model are complete and have been refined.
- Their progress is quite suitable, and it is in useful areas.
- The effect of scale is useful in avoiding work on areas that will not matter at scale.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- PI regularly seeks input from vehicle and stack OEMs. They seek information from the appropriate suppliers and manufacturing experts
- DTI could benefit from collaboration.
- DTI has done well with the challenge of working with industry partners and publicly available process information to build accurate cost models.
- By its nature, this work is highly collaborative. The PIs seem to be doing an excellent job of seeking out information from laboratories; e.g., ANL, and relevant firms (fuel cell producers and their suppliers; e.g., Nuvera, Ballard Power Systems, 3M, and Honeywell) and, in turn, vetting their results with them before publication.
- This work could have much more impact if the workshops were held where the original equipment manufacturers (OEMs) interact with the model on their own and use the model to improve their system tradeoff studies. This should be done not at a gross-cost number $70/kW, but at a detailed component and subsystem level.
- They chose good institutions to work with and worked with them closely.
- The work with ANL is critical, too.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- Lifecycle cost analysis would be a useful addition to the project. Particularly, results from the recycling studies could be integrated into understanding true total cost of ownership of these systems.
- It is not clear that the 2010 technology would work as predicted. Practical demonstration of robust performance and degradation rates; i.e., freeze-start, and voltage cycling stability of alloy catalysts, will be important to validate the assumptions needed for a meaningful lifecycle cost analysis.
- Investigation of recycling cost and issues might be added.
- The PI's attention to detail extends to their future plans, which are laid out on slide 25. They intend to explore design tradeoffs such as operating pressure vs. catalyst cost, and the effect on the system price of quality control in more depth.
- One would like to see a workshop where OEMs interact directly with the model.
- The approach and relevance of proposed future work is suitable.

Strengths and weaknesses

Strengths

- The project successfully integrates a wealth of detail, developed through intensive investigation and interaction, into succinct reference designs with highly credible cost estimates. These are essential benchmarks for the current state of the program. Further, the detailed analysis highlights what areas continue to need attention to further reduce costs.
- The technical approach and use of OEM input are both strengths.
- Working with manufacturers ties the program to the real world.
- Have one of the best DFMA analyses of all the contractors. They are sharply focused on costs and barriers.

Weaknesses

- One may consider the reference design separate from the pricing of it. Further validation of the pricing can be had by encouraging others to price the reference model.
- There appear to be no apparent provisions for detailed use of the model by OEMs for developing lower cost system architectures.
- They are not looking far enough toward possible improvements in component and raw material costs, and so they are not getting as good an estimate of the long-term high-volume cost as possible, which is what is really needed.

Specific recommendations and additions or deletions to the work scope

- A collaboration with a national laboratory could provide the data needed to validate the 2010 technology assumptions.
- A sensitivity analysis of membrane thickness is also recommended. Inefficiencies due to gas crossover should be balanced with the benefits of increased water back diffusion and reduction in the total ionomer cost.
- Strongly recommend adding workshops where OEMs can directly interact with the model to better understand internal subsystem and component cost trades.

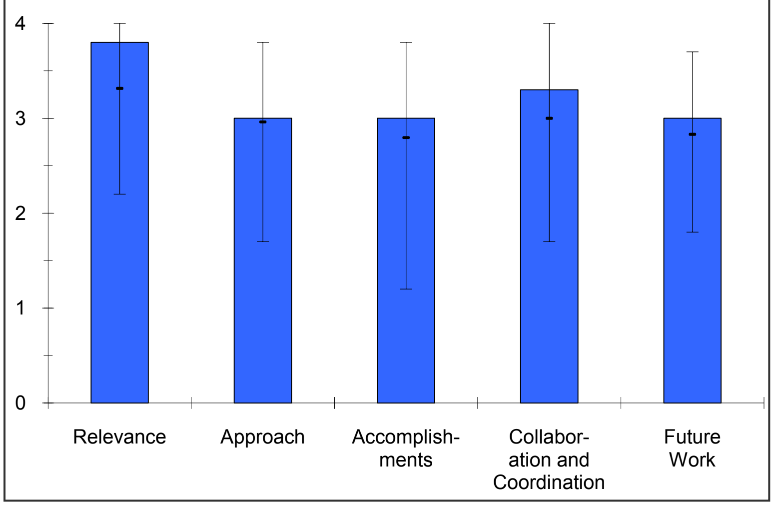
Project # FC-19: Direct Hydrogen PEM fuel cell Manufacturing Cost Estimation for Automotive Applications

Jayanti Sinha; TIAX, LLC

Brief Summary of Project

The overall objective of this project is to conduct bottom-up manufacturing cost assessment of an 80-kW direct-hydrogen polymer electrolyte membrane fuel cell system for automotive applications. Objectives for 2009 include: 1) a high volume (500,000 units/year) cost projection of a current (2009) fuel cell system configuration, 2) independent peer review of cost analysis methodology and results, and 3) comprehensive report on the 2008 fuel cell cost analysis. Objectives for 2010 include a high-volume cost projection of a 2010 fuel cell system configuration.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- It is the only publically available source for discussing costs. DOE needs these programs to discuss future R&D directions and policies.
- This project provides valuable insight into the cost projections of current and future technology. These costs would otherwise be very difficult to estimate based on the limited amount of information that is disclosed by industry.
- This is one of two projects that seek to realistically assess the mass production cost for a prospective automotive fuel cell system. The highly detailed research of this project is essential for informing overall policy on the realistic prospects that fuel cells will be an affordable alternative to other power plants; e.g., batteries and internal combustion engines. This project integrates the best available engineering and design knowledge as it has developed over the years, particularly with respect to the membrane assembly, the bipolar plates (stamped and nitrated, as per ORNL), and the balance of plant (BOP), to provide a reference fuel cell. This focuses planning and elucidates what additional research might be done to further reduce costs.
- The cost model is useful for cost-benefit analysis.
- TIAX LLC works closely with ANL; in fact they collaborate with ANL on the system configuration and modeling and they receive feedback from the FreedomCAR (Cooperative Automotive Research) and Fuel Partnership (FreedomCAR) Technology Team (FCTT), Developers and Vendors. TIAX uses a bottom-up DFMA® approach, using a high volume scenario. This reviewer believes that it is necessary to have two independent companies working on the cost as they now use approximately the same system and the work is highly relevant to the DOE RD&D objectives and the Hydrogen Program.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Both cost studies are converging on more automotive representative systems. Generally, TIAX does a great job of aggregating all the known manufacturing techniques and suppliers. They do a good job of using the limited and highly contrasting information provided to them. If improvement is possible, PI could offer suggestions for advanced, new, or upcoming techniques. PI should not invest much effort into this analysis or include it in the cost summary since it could be highly speculative, but offering thought provokers would be helpful.

- Collaboration with ANL to validate the stack performance assumptions is a strength.
- The PIs laboriously develop a reference design in close consultation with the national laboratories; e.g., ANL, ORNL , and leading firms developing fuel cells and their components. Solid methodology is used to estimate materials and manufacturing costs.
- The costing method used for estimating motor costs (catalog cost of dissimilar technology; e.g., single phase ac motor versus three phase dc brushless motor) is deficient and unlikely to produce meaningful, accurate results. This raises concern that: 1) other cost numbers may suffer from a similar inappropriate approach, 2) the cost report was not reviewed by suitable experts, and 3) the internal review process in the project team is insufficient.
- The utilization of the ANL system, which is approved by most of the OEMs and their vendors, lends credence to the TIAX selection and sizing of components. It was stated that the ANL system is thermodynamically correct and thus TIAX is costing an actual working system. TIAX also uses a sensitivity analysis now in response to one of the reviewer's remarks from last year. The ANL stack uses industry and vendor input and attempts to design a system using the minimum and most efficient components available. TIAX then costs this stack.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The model is now entering into the refinement stages. They continue to converge on realistic automotive designs.
- The model has improved the accuracy of its system and performance assumptions since last year.
- For this project, accomplishment is an accurate assessment of the state of the technology, regardless of DOE goals. They seem to be doing a credible job of it. This study emphasizes multiple "scenarios" and variability due to uncertainties.
- It is surprising to have such a significant approach error this late in the project.
- TIAX has made excellent progress toward meeting DOE objectives; using the ANL system as a costing mechanism suggests that the cost barriers are close to being met. Both the stack and the BOP components have been updated since the last review. This costing is not supposed to move the technology, but state what the best technology costs are. Good collaboration with ANL, OEMs, other developers, and vendors; keeps the costing accurate. Based on the 2009 ANL model, TIAX's cost was $55.2/kW. This cost is within the error bound of DTI's cost of $51.30/kW for essentially the same system. DTI has perhaps more experience with DFMA than TIAX does.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- PI regularly seeks input from vehicle and stack OEMs. They seek information from the appropriate suppliers and manufacturing experts.
- Partnership with TIAX is particularly useful since it provides better understanding of system sensitivities TIAX is working with several well-informed consultants: Ballard Power Systems (Ballard), Ford, the Oak Ridge National Laboratory (ORNL), and ANL. Collaboration with ANL is crucial to achieve good results—well done!
- By its nature, this work is highly collaborative. The PIs seem to be doing an excellent job of seeking out information from laboratories; e.g., ANL, ORNL, and relevant firms regarding fuel cell production and supply (such as Ford and Ballard), and vetting their results with them before publication.
- The project results do not appear to have been vetted by an expert review panel prior to presentation at DOE AMR.
- This work could have more impact if workshops were held where the OEMs interact with the model on their own and use the model to improve their system tradeoff studies.
- There appears to have been no communication between this cost study team and the other nearly identical cost study team. In the future, such interactions could benefit DOE by uncovering gaps and inconsistencies as noted, as well as lead to better cost models for the customer (DOE).

- TIAX has a close relationship with the ANL system and costs it. They also receive feedback from DOE and the FreedomCAR Fuel Cell Technical Team, developers, and vendors.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Their approach and it's relevance is appropriate. I believe that increased focus will be needed on BOP components as they are increasingly representing a larger percentage of the system cost. This may prove challenging, as BOP components are increasingly dependent on proprietary system designs.
- They should focus on completing the BOP cost analysis.
- Should we be considering different configurations for different degrees of configuration–ultimately yes, but for now it is important to stick to the targets until the baseline configuration changes (partnership working on targets currently).
- Bottom-up cost projection for stack conditioning was the only major task proposed in the future work. Quality control analysis and lifecycle cost analysis could be valuable future tasks.
- Investigation of recycling issues and cost might need to be added to the project.
- These cost estimates are a continually evolving thing as reference designs improve. The next steps (slide 32,33) indicate that future work will integrate recent information about reinforced membranes, non-woven gas diffusion layers, and stamped metal bipolar plates. In addition, cost projection for stack conditioning is a priority item.
- There appears to be no discussion in the future work of where said work will be published, what reports are forthcoming, and there are no presented plans for having OEMs interact with or utilize this model. This is a significant deficiency of the stated future work.
- TIAX will use the newest available ANL stack and BOP system and cost it using DFMA and input from DOE, the FreedomCAR Fuel Cell Technical Team, developers, and vendors.

Strengths and weaknesses

Strengths
- The project successfully integrates a wealth of detail, developed through intensive investigation and interaction, into succinct reference designs with highly credible cost estimates. These are essential benchmarks for a current state-of-the-art (SOA) program. Further, the detailed analysis highlights what areas continue to need attention to further reduce costs.
- The development of bottom-up cost for stack components is a strength.
- TIAX uses the latest ANL stack and BOP system (which is thermodynamically correct) and performs a DFMA cost study on this stack. This DFMA is reviewed by DOE, the Fuel Cell Technical Team, developers, and vendors.

Weaknesses
- The format of the data presentation makes it difficult to see the yearly trend in price predictions.
- There were inappropriate/insufficient costing methods for some components.
- I think that TIAX is not as strong in DFMA as is DTI.

Specific recommendations and additions or deletions to the work scope

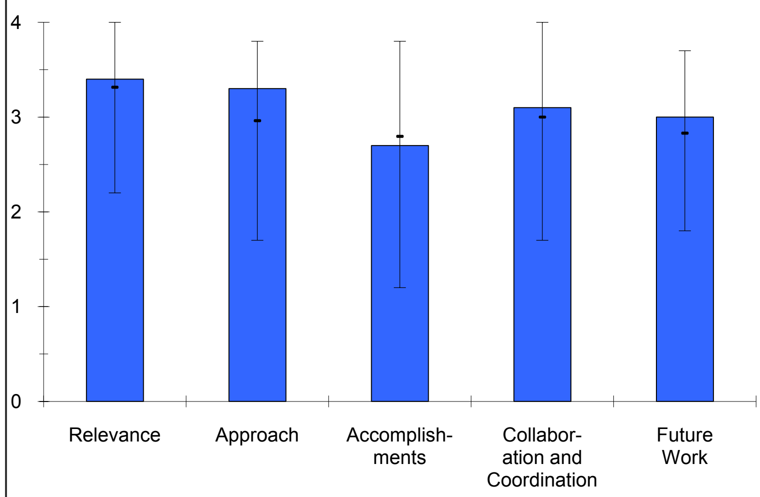
- A sensitivity analysis of membrane thickness is recommended. Inefficiencies due to gas crossover should be balanced with the benefits of increased water back-diffusion and reduction in the total ionomer cost.
- Add workshops where OEMs can interact with the model and use it to improve system and subsystem architecture tradeoffs.
- TIAX could perhaps bring on personnel who have a strong background in DFMA.

Project # FC-20: Microstructural Characterization of PEM Fuel Cell Materials
Karren More; Oak Ridge National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) identify and optimize novel high-resolution imaging and compositional/chemical analysis techniques, and unique specimen preparation methodologies, for the μ-Å-scale characterization of the material constituents comprising fuel cells (catalyst, support, membrane); 2) apply advanced analytical and imaging techniques for the evaluation of microstructural and microchemical changes that correlate with fuel cell performance; 3) elucidate microstructure-related degradation mechanisms contributing to fuel cell performance loss; and 4) make techniques and expertise available to fuel cell researchers outside of the ORNL.

Overall Project Score: **3.0** (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project provides a vital service to the DOE projects in developing and refining high-resolution transmission electron microscopy (HRTEM) and scanning transmission electron microscopy (STEM) methods for characterization of fuel cell materials.
- This project is very relevant to DOE's RD&D plan objectives. Most of the work in this project is focused towards DOE's goal. However, the relevancy of statistical data analysis methods for nanoparticle analysis is a bit farfetched for this project. The analysis of carbon corrosion and Pt migration through the membrane to form Pt bands is a known phenomenon. Analytical method development to understand these behaviors is necessary to understand MEA decay/durability effects and is relevant to DOE's goal.
- Novel imaging techniques accelerate our learning of fuel cell degradation mechanisms and novel materials characterization.
- Clearly interesting—but I still don't think I have heard anyone say, "because of what we learned in the TEM, we made XYZ change to our materials/processes/operations and now our performance/durability is X% better than it was." The new batch of durability programs is clearly providing an opportunity to change this.
- The characterization of fuel cell materials is very useful for materials' durability and performance analysis. It provides guidance to the catalyst and support synthesis, as well as membrane development.
- The comprehensive techniques really help to correlate fuel cell performance with material structures.
- For the core technology of fuel cells, i.e., MEAs, it is extremely important to explore the catalyst active sites, to maintain the active structure of catalysts, and to know how to avoid the degradation of fuel cell components.
- The fuel cell community will benefit from the achievements of the project.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- I feel that the focus on carbon supports is lagging behind a considerable effort in alternative, non-carbon supports. This would require the project to embark upon leading research on its own rather than wait for a client to ask for something that would be a significant paradigm shift.

- The use of electron microscopy techniques for the analysis of MEA components is a good approach. Investigation of carbon corrosion mechanism and Pt migration is necessary to understand the durability behavior of MEAs. However, the reason for the approach of developing a method for statistical data analysis to locate elements within the nanoparticles is not evident from the presentation. Further explanation of the future use of this statistical data analysis method would aid the team in understanding the benefit of this approach.
- The PI has many characterization methods, which is good.
- ORNL develops tools and conducts measurements to meet the needs of the various projects it supports.
- This user facility provides needed analysis for the fuel cell community.
- I strongly suggest dropping the *in situ* TEM effort. From my point of view, I see two years of method development with no result, and even if they are successful in the future, I highly doubt the relevance of the results. It will not be *in situ*. You may be able to apply a particular potential to a sample prepared for TEM, but that sample will not be the original intact material in the real three-dimensional fuel cell environment. Also, since you really cannot see the ionomer in the TEM, I think you are missing any information that might be gained by an *in situ* study. I think we are better off accepting that post-mortem analysis is the best we can do and focus on ways to make those studies yield the maximum information that they can.
- The development of the techniques allows us to collect valuable information about the fuel cell components. However, it is also critical to recognize industry needs and apply the techniques to address the issues in industry.
- The application of statistical analysis is extremely important for the nanoscale characterization. The case study of failure mechanism is strongly recommended to be high priority.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The images and data from this project are globally state-of-the-art (SOA). No other group is currently producing such high quality data, although several other groups now have the same technical capability.
- The team consists of a good mix of industry, academia, and National Laboratory partners, and good interaction between these partners is expected in this project. However, not much has been mentioned or discussed about the joint activities that had been done last year. A brief explanation of the contributions from and to the other partners will help in understanding the nature of partnership and the extent of collaboration between the partners in the team. Any opportunity for technology transfer between ORNL and other partners should be envisioned and explored.
- There were many results, but not many conclusions.
- No novel tools were developed this year, but ORNL's support has been critical for several projects, particularly those focusing on understanding carbon corrosion and Pt distribution in the ionomer.
- The images are excellent and they identify the physical mechanisms occurring during the carbon corrosion on the cathode.
- Based on the data presented, the PI should be able to begin at least speculating on the chemical mechanism occurring for Pt migration. ORNL limited it to the physical process.
- The mechanism for carbon corrosion was proposed.
- It would be interesting for the activity's PIs to hypothesize on how carbon corrodes and Pt migrates.
- While the results presented are interesting, looking at one Pt alloy catalyst or one carbon support isn't good enough to say you have gained real understanding. Without comparison to either the same materials after different treatments, or comparisons to different, but similarly treated materials, what have you really learned? I know this team wants to do those studies, and instead of trying to do a little bit of work with everyone, they may need to focus their efforts instead.
- Liquid STEM technique development has encountered the understandable obstacles.
- The project has really moved in the right direction; i.e., using statistical data analysis methods for nanoparticle analysis. The sample selection should also use a statistical method.
- The carbon corrosion mechanism study has been carried out using electrochemically-aged MEAs. It would be beneficial to continue creating new protocols to mimic the cases of MEAs in the cells, in particular, the non-uniform temperature distribution or current distribution.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- As what is essentially a service project to the industry, this project meets its objectives in providing that service to all.
- Opportunities for more cutting edge, exploratory research should be explored and encouraged.
- The team consists of a good mix of industry, academic, and National Laboratory partners, and good interaction between these partners is expected in this project. However, not much has been mentioned or discussed about the joint activities that had been done last year. A brief explanation of the contributions from and to the other partners will help in understanding the nature of partnership and extent of collaboration between the partners in the team. Any opportunity of technology transfer between ORNL and other partners should be envisioned and explored.
- The project has a very well-balanced team.
- ORNL supports many other fuel cell projects with valuable images and analysis.
- How the project collaborates is unclear. There are a large number of partners and it is unclear what are they doing. The presentation suggests that all the work is being done at ORNL. If this is the case, there doesn't seem to be a need for the long list of collaborators. If it is not the case, then the PI needs to give credit to what the partners are doing. I would have liked to rate it higher, but there is not enough information on what the partners are doing.
- They have a good number of publications.
- This team clearly needs to be more closely integrated with the new efforts on durability headed by LANL and ANL. Dr. More and her team have the tools to greatly assist those efforts, and from them, one can receive the range of materials needed to truly gain some insight.
- No doubt, both industries and academic institutions have been involved in the project. The owner of the facility may want to provide training to users so that the innovative users can easily use the facility or work with ORNL to facilitate their progress.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- It would be helpful to see more studies of metal/support interactions on non-carbon, e.g., oxide supports. The interface between Pt and conductive oxides is a region of interest in catalysis that could be aided greatly by the expertise at ORNL.
- The plans for future work address some of the gaps present in the project. Planning for further development of an *in situ* liquid holder suggests that the team has overcome past obstacles or has new ideas to overcome the obstacles. A brief explanation of past learning and new ideas will be helpful in justifying the continuation of liquid holder development activities. More emphasis in technical collaboration between all partners should be continued and opportunities for technology transfer between partners should be explored.
- Adding capabilities to the *in situ* liquid holder for nm-scale microscopy of PEMFC material constituents operated under relevant operating conditions (liquid electrolytes, temperature, potential cycling) should be extremely beneficial.
- The project has the information and capability between the members such that they should be able to identify the chemical mechanisms for corrosion, as well as the physical mechanisms, and they should be able to propose pathways to mediate the problem.
- The future work seems to be "continue what we're doing." With the facilities and the team, they should consider offering some recommendations on how to improve fuel cell performance.
- Bullet points 1, 2 and 5 on the future work slide are fine, but to do them justice, omit bullet points 3 (*in situ* TEM) and 4 (more new collaborations).
- Developing appropriate protocols for sample preparation or selection is important in parallel to the characterization techniques' development.
- The PI has proposed the right directions and topics.

Strengths and weaknesses

Strengths
- SOA equipment is driven by some of the best in the business.
- The project is being carried out by a great team comprised of a good mix of academic, industry, and National Laboratory partners. Due to the diverse mix of partners, there is an immense opportunity for obtaining MEA samples aged under many different kinds of fuel cell operational conditions. ORNL has a great set of analytical tools and a relevant knowledge base for conducting MEA component analysis.
- ORNL provides unique microscopic tools that support numerous EERE fuel cell projects.
- ORNL has excellent equipment and facilities.
- ORNL has already developed a suite of analytical techniques to aid fuel cell developers.
- The project has good tools and good people who are trying hard to be useful to a wide range of customers and/or collaborators.
- The equipment and technical skills of the team are exceptional to explore the microstructural information from the MEAs.

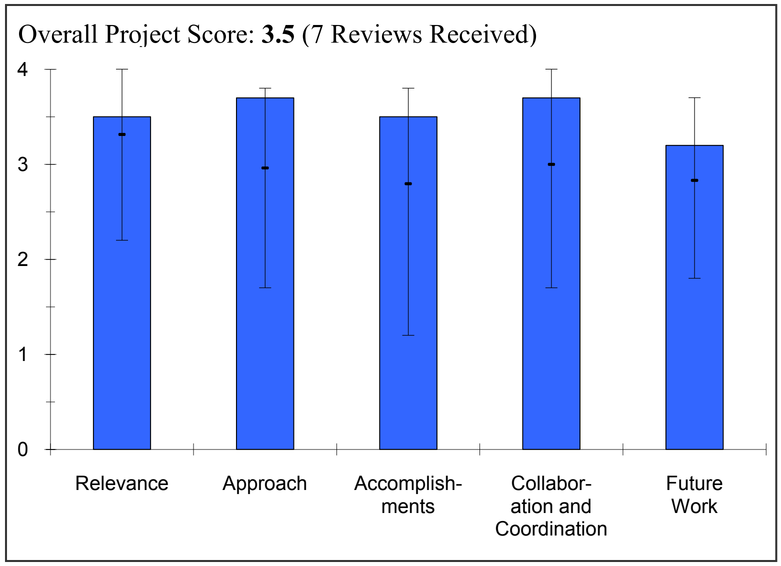
Weaknesses
- More interactive collaboration between the partners is needed. More emphasis should be given to the analysis of a variety of aged MEA samples using existing analytical methods. MEA samples aged under different, such as, automotive, stationary, and portable, fuel cell operational conditions, should be studied to determine the impact of fuel cell operational conditions to MEA degradation.
- Speculations not based on direct evidence, i.e., Pt particles lead to mechanical membrane failure, where water flows in carbon particles, impurities accelerate graphitization, can mislead other researchers and should be avoided.
- The focus has been solely on understanding what is happening. There seems to be little effort directed towards how to improve the materials.
- The progress has been slow. *In situ* TEM efforts have lasted at least two years without result. The carbon corrosion study has been mentioned as a milestone in 2008, 2009 and 2010 presentations, and is now complete for XC72 only.
- The team needs to discuss with their colleagues in the fuel cell community defining the analysis metrics.

Specific recommendations and additions or deletions to the work scope

- The project focuses upon providing a service to others, rather than pursuing its own research. I would like to see this approach change so that the expertise is used to undertake more independent exploratory research in collaboration with other groups.
- Look for ionomer in corroded electrodes.
- The PIs need to be clearer on what their partners' contributions to the work are.
- The PIs need to speculate on "why" things are occurring, not just report what has happened. For example, why does the Pt nucleate preferentially on the surfaces of graphitic domains?
- It would be nice to see the progression of the carbon corrosion. So far there seems to be fresh and completely spent studies. Intermediate studies would add a lot of information on the mechanisms.
- Drop work on *in situ* TEM. Make assisting the LANL-led durability study the number one priority.
- It will be extremely important to characterize the carbon corrosion and catalyst particle changes around proton exchange ionomers in the MEA catalyst layers.

Project # FC-21: Neutron Imaging Study of the Water Transport in Operating Fuel Cells
David Jacobson; National Institute of Standards and Technology

Brief Summary of Project



This National Institute of Standards and Technology (NIST) project aims to develop and employ an effective neutron imaging-based, non-destructive diagnostics tool to characterize water transport in proton exchange membrane fuel cells (PEMFCs). Objectives are to: 1) form collaborations with industry, National Laboratory, and academic researchers; 2) provide research and testing infrastructure to enable the fuel cell (fuel cell) industry to design, test and optimize a prototype to commercial grade fuel cell devices; 3) make research data available for beneficial use by the fuel cell community; 4) provide secure facilities for proprietary research by industry; 5) transfer data interpretation and analysis algorithm techniques to industry to enable them to use research information more effectively and independently; and 6) continually develop methods and technology to accommodate rapidly changing industry and academia needs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- It is highly relevant that NIST provides the mechanism to study water transport in operating fuel cells. This can be an important tool for optimizing fuel cell performance and durability.
- *In situ* water imaging gives valuable insight which can be used to validate models, improve membrane electrode assembly (MEA) robustness, and design better flow fields, all of which are relevant to DOE performance objectives.
- This project is very relevant to DOE's RD&D plan objectives. Direct visualization of water in various parts of fuel cell hardware is of great help in understanding the water distribution phenomenon and in developing a water-management strategy for fuel cells. Understanding the effect of gas diffusion layer (GDL) properties on water distribution within the cell is very relevant to developing good water-management strategy, and hence developing durable stack technology.
- This is an outstanding project that really gives insight into water management within the fuel cell system (fuel cells). It is outstandingly relevant.
- This is excellent work. The mix of collaborators from laboratories, industry, and academia clearly shows that this project is relevant and useful. This is a tremendous bargain for DOE at the cost.
- Monitoring the water transport in operating fuel cells is extremely important. Fuel cell durability and polarization performance at high power density are determined by catalyst activity, component stability/conductivity, and water transport properties, in particular water transport, is more important for the fuel cell stack and system design.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The NIST approach of close collaboration with users allows each partner to contribute in their specific area of expertise. This is a very effective and efficient approach for fuel cell investigative studies.

- The approach of mapping the high frequency resistance (HFR) with the amount of water present in GDL is interesting. This approach is good as it provides a direct correlation between the amount of water vs. HFR in a fuel cell. The approach of using graded anode GDL and flow field channels with different hydrophobicity is also unique. Overall, all the approaches taken in this project to study water transport within fuel cell components are valid, and their technical feasibility has been addressed in this work.
- The approach was outstanding in addressing the technical barriers of neutron scattering and two-dimensional imaging.
- The enhanced capability for freeze/thaw studies is an excellent addition.
- Neutron imaging is an excellent technique for investigating water transport. The NIST team has shown the capability to utilize the technique to address the water transport issues in fuel cells. Better understanding of the techniques' resolution and improving the resolution allow the team to have deeper insight into water transport in fuel cell components.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- The large number of users (fuel cell researchers) that NIST has worked with in the past year is truly impressive. At the same time, NIST has made significant improvements to its diagnostic tools.
- 13-μ spatial resolution is a major accomplishment, which enables imaging water content in distinct GDL, membrane, and most electrode layers.
- It is encouraging to see that the presence of 20 μ of water layer in GDL has significant influence in the HFR of the cell. The study should be extended to see what level of cathode inlet humidity is required to maintain such water level in the GDL. This information will help system engineers to develop an adequate system for retaining such level of water in the GDL. The study of the thermal conductivity of the GDL and its effect on the water saturation point in fuel cells is very informative. It is interesting to see that higher thermal conductivity induces more water deposition into the GDL. The effect of hydrophobic flow field channels on water accumulation in GDL is very convincing, and is observed in the neutron radiograph. The radiographic evidence of temperature-gradient-mediated phase-change-induced flux and capillary action is very interesting.
- This presentation was excellent. Now that we are building up a substantial body of results with different materials and flow field designs, I think we are seeing some real understanding grow, especially if we look more at comparing these results with computational fluid dynamic (CFD) model predictions and see where the CFD models are lacking.
- High resolution and high scan rates help to reveal the instant and dynamic water transport information. Individual cell diagnosis for water transport in stacks is really helpful for the stack design engineers.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- The number of fuel cell collaborators that NIST has worked with is outstanding and includes academia, National Laboratories, and the private sector. It is important to note that proprietary services can be provided to industrial partners.
- The long list of influential collaborators demonstrates that this project is valuable to many fuel cell researchers.
- Since this is a unique facility, there seems to be lots of users of the instruments, where NIST is providing an analytical service. However, not much description has been given about the collaborators. The improvement of imaging techniques and instrument operation was done solely by NIST. More direct collaboration will be beneficial for the research community.
- Publishing fifteen papers in refereed journals this last year is excellent. In addition, I do not hear anyone complaining about the beam time allocation process. The team has worked extensively with industry and academic groups in universities and National Laboratories.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The plans to continue the study of water transport in partnership with fuel cell experts is excellent. In addition, the plans for continued improvement in the NIST diagnostic tools are important.
- Water attenuation and other causes of beam hardening are very important to understand so that researchers can have confidence in neutron imaging results. All common fuel cell materials should be checked for beam hardening interference.
- The future research plan is in line with the development needed to explore the limits of the neutron radiography technique for *in situ* fuel cell evaluation. It's encouraging to see the plan of building a new facility.
- Bringing the larger field of view detectors on line next year can only help. Continuing to look into systematic errors will also be beneficial.
- Further development of advanced imaging methods will be appreciated. However, it is also very important to work with industry to address the issues in industry and define the fundamental challenges for future research.

Strengths and weaknesses

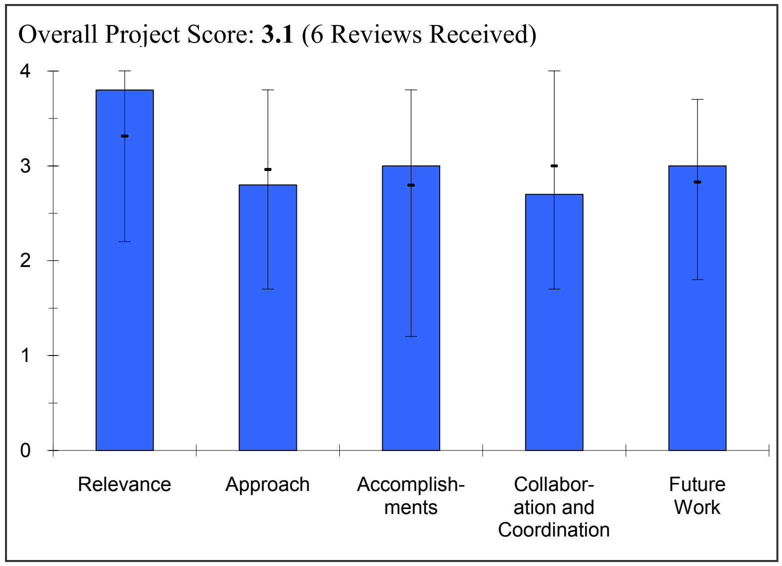
Strengths
- They employ unique diagnostic tools.
- It is an effective approach to work in partnership with fuel cell researchers.
- Neutron radiograph is a unique method for visualizing the water transport through various components of the fuel cell. Involvement of a large number of research groups, from academia, industry and National Laboratories, makes this program very active and effective. This is a very effective, non-destructive method for visualizing inside the operational fuel cell.
- This is a nice project that should be a model for DOE. This group has really expanded the boundaries of fuel cell understanding by providing unparalleled insight into water management within a fuel cell. They have an excellent approach to operating the instrument and using it to probe fuel cells. They work seamlessly with various groups on other DOE projects.
- This is relevant, useful, and a good balance between industry and academia.
- The excellent facility, equipment and technical skills make the team very competitive.

Weaknesses
- The experiments can be carried out in small foot print only. It will be advantageous if this method can be implemented to visualize cells with larger dimensions.
- Further improving the resolution and scan rate has technical challenges. The team needs to consider how to use the current equipment and methods with the realistic resolution to address reasonable issues in the fuel cell stack and system development.

Specific recommendations and additions or deletions to the work scope

- Further improvements in the spatial resolution would be useful to study water management in MEAs with thin (<10 μ) electrodes.
- I think that detectors with modest spatial resolution but higher temporal resolution may be of interest to the community in the future.
- The team may want to help address the water transport and distribution in the fuel cell stacks, in particular, to help diagnose the failure mechanism of the individual cells in the stacks.

Project # FC-22: Nitrided Metallic Bipolar Plates
Peter Tortorelli; Oak Ridge National Laboratory



Brief Summary of Project

The objective of this project is to demonstrate nitridation to protect stamped metallic bipolar plates (BPP). The overall goal is to demonstrate potential for metallic BPPs to meet automotive durability goals at cost of < $5/kW. Milestones include: 1) no significant warping or embrittlement of stamped, 15-cm^2 active area plates by nitriding; 2) single-cell fuel cell test behavior for 15-cm^2 stamped and nitrided metallic BPPs equivalent to that of graphite; and 3) complete automotive original equipment manufacturer (OEM) BP manufacturing assessment and single-cell, drive-cycle test protocol.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- One of the keys to achieve the cost, power density and volumetric density targets for the successful commercialization of fuel cell vehicles (FCVs) is the ability to make cheap, chemically robust, ultra-thin bipolar plates. This project attempts to do just that.
- The development of low cost bipolar plates is of great interest considering the stack durability and cost. Therefore, it is critical to achieve the proposed objectives.
- Bipolar plate improvement and cost reduction is critical for the Fuel Cell Technologies Program to meet its cost and durability targets.
- Metallic bipolar plates offer vital advantages over graphite bipolar plates. Corrosion protection for metals as bipolar plates proved to be mandatory.
- Metal bipolar plates can contribute to overcoming the cost and durability barriers that prevent commercialization of polymer electrolyte membrane fuel cells (PEMFCs) for many applications. For this reason, the study of metal bipolar plates is relevant to DOE's objectives.
- This project has not avoided the targets that are relevant to the study of bipolar plates. It focuses on cost, corrosion resistance, and conductivity. In principle, the other targets (mechanical strength, formability, weight, and low gas permeability) could possibly be met with the candidate materials.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- From a materials perspective, the approach of this project is very strong.
- When looking at the size/area of the manufactured plates, this reviewer wonders if the ability to scale up to full-scale stack-sized bipolar plates is feasible.
- It would be worthwhile for the PI to demonstrate the ability of producing plates larger than 400 cm^2, which are uniform in thickness, are flat, etc., to demonstrate the viability of this project.
- This project contributes to overcoming some barriers such as formability, performance, and to some extent, cost.
- The approach is correct, but it could have been improved by considering bipolar plates with representative active areas (on the order of 200-300 cm^2 instead of 15 cm^2) and considering stacks instead only single cells.

- What about the sealing? The gaskets can be put either in the membrane electrode assemblies (MEAs) or in the bipolar plate. This option has not been integrated into the project.
- This is a somewhat academic report. It does not address low cost manufacturability.
- A comparison with competing technologies should be made to check viability.
- There has been a lot of work in nitrided metals.
- The use of cycle, as opposed to steady state conditions, to evaluate performance is good, but the test cycle used does not seem to represent the drive cycle of a car, which would be what the bipolar plate would experience for this application.
- The plates were fresh (no welds). The plates should have had a part welded to be able to evaluate the impact of the welding.
- The simple flow field used does not seem representative of what is expected. For the stamping test, the team should have included more details and tried more complex geometries.
- With the new material being developed, will there be joining problems? This question should be addressed.
- Though this approach has been pursued over a couple of years, new challenges, like using stainless steel instead of more expensive Ni-containing metals, were taken up recently, and the requirements for the metals could be lowered substantially, leading to further cost reduction.
- In terms of the targets that DOE has identified for bipolar plates, this nitrided stainless steel project has generally had a decent concept for achieving interfacial contact resistance requirements. However, cost, corrosion resistance, formability, and joining have always represented the primary doubts.
- Cost estimation should have preceded the material development. Now that the project is in its final stages, cost has been shown to be within range of targets, but the sequence should have been reversed.
- Corrosion still remains a question mark. The cycle used in *in situ* testing does not represent startup/shutdown cycling. Officially, the tests prescribed by DOE, including the *ex situ* tests, will never be able to address high voltage corrosion. While the materials in this project passed the tests, there is still some desire to know what would happen under realistic cycling.
- Formability and joining were addressed late. Formability necessarily followed the change in base metal, while joining still has not been evaluated.
- There has never been an interest in investigating corrosion through coolant channels.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **3.0** based on accomplishments.

- The PI has obviously done a lot of work optimizing the materials used, the chemistry of the treatments, and the optimization of the surface treatment. The technical accomplishments and progress are high and commendable.
- The ability to produce large-scale, high quality, thin, uniform, bipolar plates still remains to be demonstrated. Also, the cell potentials, which the PI uses to investigate the chemical robustness of these cells, are a bit modest. Spiking the cell potential to 1.3-1.5 V as experienced during startup and shutdown, will eventually be needed to validate the chemical robustness of these cells.
- The different characterizations performed in this project (nitridation process, polarization curves, membrane electrode assembly (MEA) contamination) are interesting and indicate that the proposed nitridation solution might be interesting.
- Questions are still open, like the formability of this ferritic stainless steel regarding real GM bipolar plates with small channels (in particular in the curves), the welding of the plates, and the real cost.
- The cause of the 35-µ MEA failure was not made clear.
- The 1,000-hour test is a significant accomplishment.
- For the size of the team and the amount of funding, one would expect more accomplishments.
- The stamping of the simple flow field design was good, but more complicated and larger designs would be more representative of the industry.
- The nitridation process for stainless steel has been developed well. The process seems to be adequate for mass manufacturing.
- The material exhibits good longevity under normal operating conditions. Failure-mode conditions should be introduced later in the project or reserved for a follow-up project.

- Despite concerns about the approach, the PI has managed to consistently respond to concerns throughout the project's life through testing.
- Low interfacial contact resistance has been demonstrated.
- Formability has now been partially demonstrated, although it would still be interesting to see if depths beyond approximately 250 μ could be achieved with a channel span of approximately one micron. At present, the formability shown is likely not sufficient to enable robustness to a wide variety of cell components, particularly gas diffusion layers (GDLs).
- Corrosion remains a mystery. Before and after cycling, *in situ* high frequency resistance (HFR) and *ex situ* through-plane resistance measurements need to be done. One slide showed evidence of some discoloration and some explanation is needed. The X-ray fluorescence measurements could still have produced the same data with corrosion, especially with high cell humidification (inlet RH not noted).
- Cost analyses performed in other projects. Although the cost does not meet the official DOE target, the cost is within striking distance. Some decrease is still preferred.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Research collaborations with all of the various entities are at a high level and are transparent. Adding GM certainly makes this program stronger. If the technology proves to be feasible and practical, it will be interesting to see who would scale it up and commercialize it. ATI Allegheny and AGNI GenCell are two companies that come to mind.
- The collaboration between the partners seems correct.
- More stack manufacturing expertise should be included in the project
- The addition of GM to the team was good.
- The PIs have a large team. They defined the roles and responsibilities of each partner well. Communication between the partners is unclear.
- With GM and other industrial partners, it was surprising to see that intellectual property was available for license.
- With GM, an OEM is part of the project team. Though industry input is generally appreciated, they should be sure that other companies still have sufficient access to the technology.
- Collaboration has been done with many partners and has been improved with the incorporation of an automotive OEM (GM).
- The roles of Allegheny Ludlum, GenCell, NREL, and LANL are clear.
- The role of Arizona State University was not made entirely clear from the presented slides, but one could guess that they played a role in the cycle tests.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Future work is very strong, especially now that GM has been added to the program. GM's most robust drive cycle test protocols will do a more thorough investigation of the bipolar plates' chemical robustness under a more extreme variety of cell potentials. It is good to see the area of the single cell be increased to 50 cm^2, but ideally it would be even larger.
- The proposed further work is in accordance with the main objectives of the project. One might just ask if all the jobs will be done by the end date of the project (September 2010).
- This project is ending, so future work is unclear.
- Completing GM's manufacturing study is very important.
- As the project is ending soon, the proposed work is appropriate. The activities basically focus on cost reduction.
- The project is near completion and is now directed towards those activities that should be its final focus. These activities include laser welding, realistic drive cycle evaluations, and further efforts to reduce cost through quartz lamp nitriding.

- It is hoped that the GM single cell drive cycle will include startup / shutdown cycling. It is also hoped that further stamping activities will test formability limitations and investigate the draw depth that can be achieved given a narrow channel span.

Strengths and weaknesses

Strengths
- A lot of work optimizing the materials used, the chemistry of the treatments, and the optimization of the surface treatment has taken place. The technical accomplishments and progress is high and commendable.
- The project strengths are its partnership covering the whole need of production, and characterization.
- This is a large team with sufficient funds.
- Adding GM strengthened the project.
- Good materials approach, which has been pursued with perseverance. The focus on normal operating modes other than failure operating modes is positive, since convolution of too many criteria was avoided. Good materials have been identified. Further optimization to harsher conditions should be reserved for future projects.
- The PI has been able to respond well over the years to considerable feedback from reviewers and OEMs. The project has added activities related to formability, joining, and cost estimation, and has changed base metals to meet targets.
- The project has made use of an intense amount of collaboration amongst approximately a half-dozen partners.
- The future work is well focused on remaining activities.

Weaknesses
- The ability to produce large-scale, high quality, thin, uniform, etc., bipolar plates still remains to be demonstrated, and, as of now, no plans are included in this activity. Also, until now, the cell potentials that the PI uses to investigate the chemical robustness of these cells are a bit modest. Spiking the cell potential to 1.3-1.5 V, as experienced during startup and shutdown, will eventually be needed to validate the chemical robustness of these cells. This activity probably will be investigated by GM.
- The main weakness is that they only worked on small-size, single cells. Even with the larger 50-cm^2 GM bipolar plate may not be really representative with nitridation process of real-size bipolar plates for automotive applications. Warping remains a potential issue.
- Stack testing would have been appreciated to evaluate the performance and durability of these bipolar plates.
- The cost analysis could have been more detailed, to be more convincing in achieving the cost target.
- Stamping is much more difficult than people realize. The designs and plate size are extremely important. They needed to examine more complicated flow fields over a larger plate (at least 25 cm^2; preferably 50 cm^2) for stamping size matters.
- The cycle testing seemed very tame compared to what the fuel cell will experience in a vehicle.
- Doubts about corrosion under realistic drive cycles remain, as well as doubts about the effectiveness of the cycle that was generated by the project to test the plates. Hopefully, the GM collaboration will help to address this issue.
- More *in situ* diagnostics needed, e.g., HFR.
- Formability and cost evaluations have improved considerably, but the limits of each are still unknown.
- Throughout the project, development carried forward without first answering critical questions about cost, corrosion, formability, and welding. For the most part, the project has serendipitously succeeded, but there was perhaps more risk than necessary in getting there.

Specific recommendations and additions or deletions to the work scope

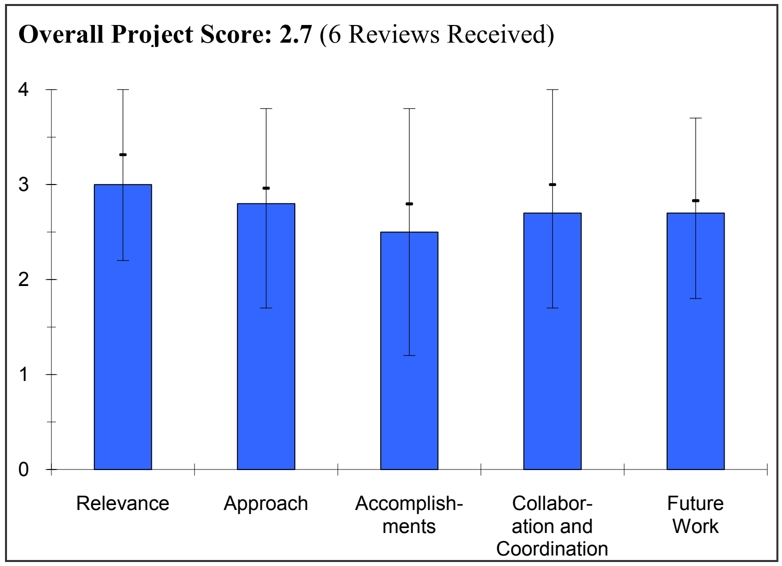
- Large scale (400+ cm^2) plate making would help the work scope.
- More extreme cell potentials need to be included in the work scope.
- The PIs needed to examine more complicated flow fields over a larger plate (at least 25 cm^2; preferably 50 cm^2) for stamping size matters.
- The cycle testing seemed very tame compared to what the fuel cell will experience in a vehicle.
- Needed additions, e.g., realistic drive cycling, cost estimations, welding, *in situ* diagnostics, are being addressed in either ongoing work or future work.

- For apples-to-apples comparisons, it would have been good to have seen cycling on an untreated $Fe_{20}Cr_4V$.
- Unless better evaluations are to follow, it would be good to understand what the discoloration on slide 13 represents.
- Some information about coolant channel corrosion would be beneficial.
- *Ex situ* evaluations before and after cycling are needed.

Project # FC-23: Low Cost PEM Fuel Cell Metal Bipolar Plates
Conghua Wang; Treadstone

Brief Summary of Project

The objective of this project is to develop low cost metal bipolar plates (BP) to meet 2015 performance target at a cost of less than $3/kW by: 1) developing carbon-steel; 2) reducing or eliminating the use of platinum; and 3) demonstrating TreadStone metal plate applications in portable, stationary and automobile fuel cell stacks.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The development of low cost BPs is of great interest, considering the stack durability and cost. Therefore, it is critical to achieve the proposed objectives.
- They have a strong focus on reducing cost, which is a critical step for fuel cell implementation.
- The project seeks to produce a low cost BP base material to meet the DOE 2015 cost target.
- Innovative concepts for low cost BP development are critical. However, the cost basis for this concept wasn't clearly established in the presentation. It also wasn't clear how the coolant layer or gas manifolds are integrated.
- The TreadStone Project addresses the BP durability and cost, and is very relevant to the DOE Fuel Cells Technologies program.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The approach to insert some conductive vias in a non-conductive coating is original. The characterization means are classical.
- Task two is referring to carbon steel and aluminum based plates, but only carbon steel plates are detailed.
- No details are given regarding the gaskets. As gaskets may be put on the BP, it should have been addressed.
- The cost objective is mentioned, but no corresponding milestone is presented.
- They employed an interesting approach to utilize only a portion of the surface as conductive material. Fabrication cost analysis will be critical information.
- Unique approach using non-corrosive, electrically conductive vias to support conduction through a non-conductive surface treated plate. Such an approach allows the use of cheaper material for the bulk of the plate and utilizing a small amount of expensive material to support conduction through corrosion resistant layers.
- The via concept is not clear for stainless plates.
- This technology should be compared with gold-plated, stamped stainless steel that has the thickness of gold minimized. This could be considered a baseline and enable direct comparison. If the via technology is capable of five nm equivalent thickness, the demonstration metrics should be compared.
- The approach is unique to TreadStone—vias that represent a small fraction of the surface of the plate that conduct electrons through the plate while the rest of the plate is covered by a non-corrosive, non-conducting coating. TreadStone is investigating lower cost materials for use as conductive vias, as well as carbon steel and aluminum base materials.
- There have been other attempts to coat low cost metals with corrosion-resistant coatings. Some sort of conductive additive/particle is needed to ensure electrical conductivity. TreadStone indicated that the expensive

via materials need cover only 1.4% of the surface area for acceptable conductivity and thus contribute a small part of the material cost.
- Their efforts are not high focused. The work on carbon steel should be eliminated.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- The presented results lack precision to be evaluated in a relevant way. For instance, on slide four contact resistance values are presented, but without précising the clamping force, or if the measurements have been performed on stamped or flat samples.
- Data have been presented neither on carbon steel nor on aluminum based plates, but only on stainless steel (SS)316L! In the discussion, we learned that corrosion rate on carbon-steel was between 5 and 10 µA/cm²; i.e., far over the target.
- Costs are given on SS, but no indication of the assumption of the number of plates is considered.
- In the presentation, it is not précised if the coating can be stamped or if it has to be processed after stamping and welding. And if it is before stamping, how will be the welding be done?
- No active peaks with the coating is underlined, but even on bare SS 316L, there is no active peak.
- For the 10-cell stack, 2.5 kW for 3,000 cm² is less than the target. As the stack is running for 800 hours, polarization curves or performance graphs would have been appreciated.
- Not clear how many samples have been tested for performance repeatability.
- The experimental data looks promising, and the cost per kilowatt is close to the 2015 target. More extensive testing is necessary to provide convincing evidence that this approach can be successful.
- The stack demonstration should include a resistance measurement (high frequency resistance/internal resistance drop). Moreover, it would be interesting to monitor changes in this resistance over more time.
- TreadStone earlier proved the concept of their metal plate technology by achieving 8,200 hours in a long-term stability test. They have recently completed initial performance testing of a 1-kW stack for stationary fuel cells and Ford is testing TreadStone coated SS plates that contain gold dots. The testing includes realistic driving cycles. The stack has achieved 800 operating hours.
- A manufacturing cost analysis indicates that the current technology can meet the 2010 DOE targets, but not the 2015 targets.
- There were different via materials developed. Stack current-voltage performances were tested. A similar curve should be given with graphite BPs. Authors should give contact resistance curves (instead of the number). Moreover, corrosion testing for other via materials should be given. The influence of defects on the corrosive behavior should be monitored.
- As an add-on coating, the thermal coefficients of different materials and their effect on the corrosion of the substrate material should be addressed. This is very important since proton exchange membrane fuel cells are expected to operate at a wide temperature range.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- The collaboration inside the project seems correct.
- It is not clear from the presentation level of interaction between partners; as they simply list tasks each will complete.
- For a project like this, there should be even more automotive original equipment manufacturers (OEMs) involved as partners. The project should look into collaborating with other National Laboratory groups.
- This reviewer recommends reviewing the cost analysis with OEM.
- The collaborators are all experienced and are experts in specific areas. The collaboration appears to be very good and covers the needed skill sets to provide TreadStone with an independent assessment of their technology. Ford brings automotive expertise to the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The proposed future work is in accordance with the project targets. Fabrication cost using aluminum based plates should be inserted.
- They will not develop aluminum plates until 2011. If possible, this task should be moved earlier.
- Other via materials are slated to be studied. Utilizing carbon nanotubes (CNTs) might be a way to further reduce cost. Corrosion studies that are ongoing or planned for the future are critical to provide convincing proof of the applicability of this approach.
- Future work seems ambitious based on the current rate of progress.
- TreadStone is focusing on developing lower cost conductors such as CNTs, palladium dots, and metallic carbides. They are also looking at carbon steel and aluminum plate materials, upon which their coating will be applied.
- The work plan appears reasonable but lacks decision or down-selection points.
- The project should be focused on a stainless steel substrate.

Strengths and weaknesses

Strengths
- The main strength of the project is the partnership.
- They are focused on cost.
- This is an innovative approach to bipolar plate design. The material and fabrication costs are quite attractive.
- The concept is unique and warrants evaluation.
- The project has demonstrated feasibility and the ability to meet the DOE 2010 cost target. Lower cost approaches are being investigated, which give some hope of meeting the 2015 targets.
- Ford is subjecting one design iteration of the plates under rigorous automotive test conditions.
- The project is target oriented, includes stack testing, and exemplifies team work.

Weaknesses
- The main weakness of this project is that no performance and cost comparisons are done with alternative BP materials or coatings, like composite or carbon based coatings on metallic plates. For the latter, the coating cost should be comparable, but with a lower-cost material.
- As the number of contact points is reduced, operation at high current densities may be an issue, due to thermal hot spots where the current is passing.
- They were not clear on the number of samples tested or test repeatability.
- They were not clear how robust the plates will be.
- It is not clear what this technology is being compared to, and a baseline should be established. It isn't clear how this will be accomplished.
- It is not clear if the paths being followed will be able to meet the DOE 2015 cost target. The ultimate target may need to be even lower to enable widespread commercialization.
- The presentation lacked clarity on how the conducting vias are formed.
- The efforts are not very focused.

Specific recommendations and additions or deletions to the work scope

- Performance and cost comparisons with alternative BP materials or coating, in particular carbon based coatings on metallic plates, should be added.
- Cost evaluation should take into account the real stack performance and not the assumption of 1000 mW/cm^2, which artificially lowers the number of plates.
- Move aluminum plate development forward to ensure adequate time for testing.
- Bring in more automotive OEMs into the collaborators. The project should demonstrate performance and corrosion resistance under real world conditions and for greater time periods. Perhaps accelerated stress testing should also be performed.

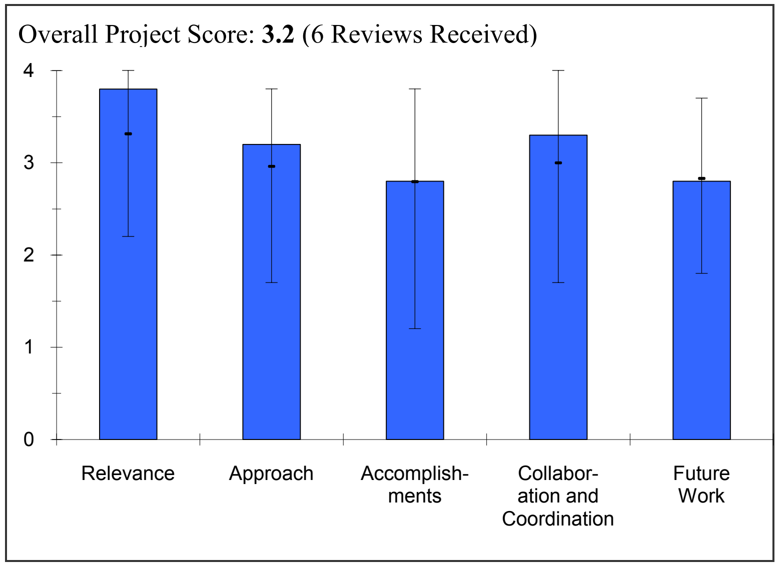
- Add down-selection points and appropriate metrics for the decision.
- Work with stationary and portable fuel cell developers to qualify the plates for those applications.
- Focus on stainless steels. Testing with some 200 steels may be helpful.

Project # FC-24: Metallic Bipolar Plates with Composite Coatings

Jennifer Mawdsley; Argonne National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) create a coated aluminum bipolar plate (BPP) that meets the DOE performance and durability targets for BPPs that are thinner and more durable than machined graphite BPPs and up to 65% lighter than stainless steel, and 2) develop a composite coating that is electrically conductive and corrosion resistant using a mixture of a fluoropolymer and inorganic filler.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- One of the keys to achieve the cost, power density, and volumetric density targets for the successful commercialization of fuel cell vehicles (FCVs) is the ability to make cheap, chemically robust, ultra-thin bipolar plates. This project attempts to do just that.
- The development of low cost bipolar plates is of great interest, considering the stack durability and cost. Therefore, it is critical to achieve the proposed objectives.
- The project has the potential to develop low cost, lightweight bipolar plates.
- The project addresses the DOE barriers of performance, cost, and water transport within the stack by forming a hydrophobic surface of the bipolar plate.
- The project is in alignment with the intention of the DOE program. It seeks to lower bipolar plate weight through use of aluminum, while keeping costs lower through the expectation of stamping. Corrosion and conductivity are also accounted for in the coating selection.
- Bipolar plate work is still relevant within the DOE program since plates can contribute to the cost and durability barriers that most applications experience. Within the context of DOE work, these barriers have not yet been cleared with regards to bipolar plates.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The approach of using aluminum as the base material instead of stainless steel is a valid approach thanks to its low cost and weight savings. Also, the concept of using fluoropolymers as a coating makes sense from a chemical robustness perspective.
- The weakness of this approach, however, is the poor selection of electrically conductive dopants, TiB_2 and CaB_6. While both chemicals are reported to have higher conductivities than graphite, their incredibly weak tolerance to acid and fuel cell (fuel cell) potentials is a huge detriment to this project and approach.
- The approach to develop composite coating is correct. The characterization means are classical.
- No details are given regarding the gaskets. As gaskets may sometimes be put on the bipolar plate, it should have been addressed.
- The cost objective is mentioned, and, even if the low cost methods are already accepted by the original equipment manufacturers (OEMs), a corresponding milestone should have been presented.
- The approach is sharply focused on the technical barriers.
- The possibility that, if the coating fails, the aluminum can react with water to form hydrogen and aluminum hydroxide should be explored.

- The approach is smart and the production cost for such a painted surface looks reasonably low.
- While the team acknowledges that the acid stability of the borides needed to be checked, a more thorough understanding of both boride and polymer stability in acidic oxidizing/reducing conditions should have been investigated prior to the beginning of the project.
- In general, corrosion tests are not following protocols that have recently been specified by the Fuel Cell Technical Team. Potential sweeps were not used at low voltage, and NaF was used in place of HF. The temperature was not reported.
- For both the boride rotating disk electrode (RDE) work and the plate studies, no cyclic voltammetry was reported that could be used to show whether currents were faradaic or related to other charge transfer processes. It would be interesting to see this.
- Given the instabilities that have been seen in fuel cells for aliphatic C-H bonds, it will be interesting to see ethylene tetrafluoroethylene copolymer (ETFE) stability in the presence of an operating fuel cell where Pt presence can account for higher levels of peroxide. Polychlorotrifluoroethylene (PCTFE) stability is questionable as well, and any loss of chloride might affect Pt.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The amount of high level, thorough work at this point in the project and for the dollars spent is quite impressive.
- While TiB_2 and CaB_6 have both been shown to not be viable for this application, the synthesis and characterization of these materials clearly demonstrates top-level research.
- The apparent quality of the uncoated aluminum plates, which were stamped and tested for hydrogen permeation, is very high.
- Presented results are not in total agreement with the milestone table presented on slide 6. Milestone: "Fabricate a composite coated aluminum plate": okay for the achievement, but 24-hour testing is too short for making any conclusions. Milestone: "Synthesize TiB_2 and CaB_6 using low cost process": okay for the achievement but no indication on the real cost of the powder fabrication and spraying. Milestone: “Regarding CaB_6 and TiB_2 low conductivity and corrosion resistance”: there may be no interest in using them in the future.
- There was a significant amount of progress made in a short amount of time.
- The conductivity of TiB_2 and CaB_6 is significantly larger than that of carbon, but the carbon composites have yielded conductivities that are comparable to what has been seen with the TiB_2 and CaB_6.
- The conductivity of the coatings is much lower than might be expected from the conductivity data of the fillers. The conductivity of the layers is not out of range, though. A conductivity of 5 S/cm of the layer is about 50 times higher than that of the electrolyte. As a 100 µm coating is needed on either side and the electrolyte thickness may be between 50 and 100 µm, the resistance of the layers would contribute approximately 4% to 8% to the overall resistance, provided all other resistances were negligible compared to these two. If other resistances are to be considered, the effect would even be lower. Nonetheless, a higher volume fraction of the filler, up to 90 %, should be tried to achieve a better conductivity.
- Many of the results obtained so far are as expected. For example, uncoated aluminum plates have been shown to be corrosive and lack conductivity. These data may serve as a good baseline. Furthermore, the borides were not suitable for the coatings.
- Despite the recent start of the project, more results should be expected when considering the project's short timeline. The project needs to move more quickly towards coating materials that are more promising, and particularly, towards more challenging tests. Only two slides show positive results on composite coatings, and the two coatings represented are different (hydrophilicity on graphite/PCTFE and corrosion resistance on carbon fiber (CF)/ETFE).
- Electrochemical techniques should be validated versus a standard. The currents shown could be affected by infrared compensation if not addressed.
- During the presentation, the thickness of the coatings was mentioned to be about as thick as the web thickness (~100 µm). The coatings need to be thinner!!
- Room temperature corrosion testing cannot represent the results at PEMFC operating temperature. There seems to be some technical challenge on measuring the contact resistance of the coatings.
- A stack testing with graphite bipolar plates should be used for comparison.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaborations with key partners are clearly stated, reported, and easy to understand. It would be good to know whether Orion Industries (Orion) will begin their development for coating the aluminum sheets when the electrically conductive dopant is finalized, or will start before that.
- Collaboration between the partners seems correct.
- The collaborations appear to be working well.
- The work share and expertise of the partners is clearly defined and the partners supplement each other's skills.
- The collaboration partners appear to be providing input to the project. The design and stamping of aluminum substrates through the Gas Technology Institute (GTI) have been shown. Coatings have been provided through Orion and borides were provided by Southern Illinois University. No partner has been uninvolved.
- The project also deserves merit for appearing to justifiably let go of the efforts from one partner, which is difficult to manage.
- If coatings are too thick, further work needs to be done with Orion to produce thinner coatings. If this cannot be done, an alternative coating partner may be needed.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Plans for future work with their various collaborators are clearly stated and are logical.
- The lack of having a viable electrically conductive dopant for their protective film is clearly a major weakness which could eventually derail this project. At this time, it is unclear if a viable candidate will be discovered, and, if it does, how will it affect the pending cost analysis?
- The proposed future work is in agreement with the project targets.
- Preliminary cost analysis should start from now and not in fiscal year 2011.
- Stack testing should be inserted in the work program.
- Future work is targeted at the key issues.
- Investigation of alternative filling powders is strongly supported. The borides might also be cross checked to see whether the low film conductivity might be a property of the specific powders used and not the chemistry in general, or even depend on pretreatment of these powders. Systematic check of one of the boride powders is advised to clarify, for instance, whether they form thin oxide layers reducing the conductivity.
- Some of the future plans are exactly what is needed: corrosion testing at 80°C, through-plane resistance, *in situ* testing, and cost analysis.
- Investigators should be able to provide some estimation of plate formability, including the present plate status (channel span, channel depth, radius, land width, etc.), as well as some effort to understand the limits of local elongation. Finite element analysis modeling can help in this regard.
- More information about alternative filler powders would be beneficial. The project does not have much time to waste with materials that are high risk. It would be interesting to know whether alternative fillers could be other carbons or metal carbides.
- *In situ* test plans need to be discussed.

Strengths and weaknesses

Strengths

- Management of this project appears to be very high with good coordination between the various groups. Stamped aluminum plates also appear to be very high quality.
- The project strength is its partnership, which covers production and characterization needs.
- The project has the potential for a high payoff.
- The project uses a new and innovative approach, introducing aluminum as a new material which is highly conductive and very ductile as a bipolar plate.
- The project is using a low weight material for a substrate (aluminum).

- The PI has managed to use collaboration opportunities well to deliver the results from each partner.
- The PI was able to move nimbly from the boride concept to carbon-based fillers once the boride materials showed their deficiencies.
- Good effort to provide baseline data for uncoated aluminum, materials synthesis, and stack testing with untreated aluminum plates.
- Team work is a definite strength.

Weaknesses

- The lack of having a viable, electrically conductive dopant for their protective film is clearly a major weakness that could eventually derail this project. At this time, it is unclear if a viable candidate will be discovered, and, if it is, how it will affect the pending cost analysis.
- No stack testing is foreseen. Referring to the total project budget, there might be no financial issues.
- The benefit of TiB_2 and CaB_6 materials for composites as compared to carbon has not yet been demonstrated.
- Project needs to eliminate materials faster if they are not meeting requirements, and do so with the philosophy of using a reasonably aggressive test first. Hopefully the carbon fiber/ETFE composite coating will be tested in a cell soon.
- The *in situ* test plan should have already been formulated.

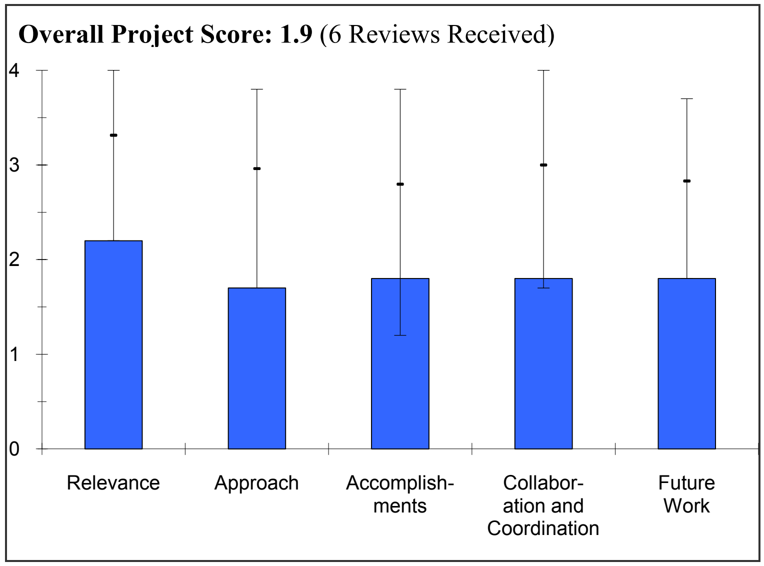
Specific recommendations and additions or deletions to the work scope

- Corrosion testing at high temperature and contact-resistance testing should be performed.
- Cost analysis is needed.
- Find a dopant as soon as possible.
- Performance comparison with usual bipolar plate (graphite, composite or metallic gold coated) using the same design and operating condition should be added.
- Evolution of the contact resistance at long term may be an issue in particular on the cathode side with oxidation of the aluminum plate. Coating permeability could be measured before single cell testing.
- This PI may want to examine issues associated with reactions of aluminum with water in areas where the coating is not protective.
- A systematic analysis of the reason for the low conductivity of the borides needs to be carried out. The team could also embark on a development of paints with a higher degree of filler.
- Focus on aluminum and use lower risk coatings.
- Formability estimates for the plate should be provided, as well as detailed cost estimates.
- Material status should be reported consistently for all candidate coating composites.
- Some test protocols (particularly with respect to corrosion) need to be updated to fit DOE guidelines.
- Electrochemical testing should be reviewed to be sure it is validated.
- Borides appeared to have been removed and should remain so.
- More collaboration is needed.

Project # FC-25: Air-Cooled Stack Freeze Tolerance
Dave Hancock; Plug Power, Inc.

Brief Summary of Project

Project objectives are to: 1) advance the state-of-the-art for air-cooled stack technology by determining stack failure modes and root causes, developing baseline understanding for freeze tolerance, validating mitigation strategies for failure mode root causes and design improvements to improve freeze tolerance; 2) test and evaluate air-cooled stacks and components developed for increased freeze tolerance and durability; 3) evaluate failure mechanism mitigation in stack and/or system design; 4) perform life-cycle cost analyses for freeze tolerance strategies; and 5) document and publish summary of stack freeze failure analyses.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- The key topics concentrate on advancing and understanding major issues and barriers of fuel cell (FC) technologies. They address technology development aspects, cost analysis, and increasing understanding of real-world operating conditions; i.e., improving freeze start behavior of stacks and system components. The overall project, however, appears to be tailored too much to one particular application. Instead of concentrating on air-cooled stacks alone, the inclusion of stack systems with alternative cooling technology would widen the scope of the project and increase its value to the community.
- The development of freeze tolerance and air cooled stacks can help DOE with early market penetration in some markets, but the vast majority of higher power applications will require liquid cooling. The DOE program cannot be justified by low power applications.
- This project is aimed at the forklift market–in/out of freezers.
- Freeze tolerance and operation at sub-freezing temperatures, due to forklifts operating in refrigerated areas, seems like an incredibly small niche market. There doesn't seem to be a substantial market for fork lifts that operate at low temperatures. As the market is very small, the technology for air cooling and freeze tolerance, does not apply to other higher power applications where removal of waste heat is critical.
- Increasing FC tolerance to freezing conditions is important.
- It is not clear how an air cooled stack will fit with DOE targets. The volumetric power density needs to be identified. The market niche proposed seems very narrow.
- Although this is a Topic 4B (Transport within the PEM Stack-- Freeze Effects) project, and is 40% complete, this project is not addressing freeze effects. Only two slides (10 and 11) had any sub-freezing data, and these did not provide any insight or anything new.
- This project addresses very important aspects that are limiting FC commercialization and holds relevance to the DOE Hydrogen Program goals. More specifically, the relevant DOE objectives addressed are sub-freeze conditions, durability, and cost.

Question 2: Approach to performing the research and development

This project was rated **1.7** on its approach.

- The approach was presented almost like a trial and error development of a stack system. According to the presenter, much iteration is needed to improve the system, which seems very insufficient and incomplete regarding the limited amount of two iterations proposed for the course of the project. This approach concentrates on a single FC design only, which offers very little potential for improving the freeze tolerance of FC systems in general. Instead, a scientific approach that helps to develop a general understanding of the processes involved in freeze start is missing completely.
- Approach uses multiple design, build, and test (DBT) cycles. However, two DBT cycles cannot be considered multiple.
- The approach is broken into three phases:
  - PHASE 1: Stack and Module Baseline Testing and Failure Modes Identification,
  - PHASE 2: Freeze Effect Design Mitigation Strategy, and
  - PHASE 3: Freeze Effect Failure Mode Mitigation Testing.
- In terms of freeze tolerance design and air cooling, there is little to no information to understand what the design iteration is.
- One would need more information of how the project will be focused, not simply DBT.
- They need to present much more information on test procedures, such as how the system is "frozen."
- There was no discussion of cost or how to achieve a 25% reduction metric.
- The DBT approach is effective, but expensive. The project team should plan to do modeling to determine the limits of the technology. It would be interesting to see whether the same air-cooled design that works and starts in a sub-frozen environment could work in a 40°C ambient temperature. Go/no-go decision point 1 requires a 25% cost reduction, but it is not clear how this can be evaluated, as the cost analysis was not presented.
- This project is not addressing freeze, to date it has primarily focused on start/stop strategies. They have not focused on freeze start/stop strategies, just normal start/stop decay. This is not supposed to be the focus of this project. Additionally, it does not appear that much freeze work will be done based on the slides. Their goal does not even include developing cold-start capability! It is, therefore, not clear why this project addresses DOE's Topic 4B goals.
- The project approach includes multiple DBT iterations to address the key issues relevant to freeze tolerant air cooled stacks. However, the lack of information regarding test and design plans raises concern on whether the project objectives will be accomplished within the project timeline.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.8** based on accomplishments.

- The presented data only described the problem, but it did not show any advances in developing a more freeze-tolerant system. While defining the subject matter of such a project is very important, a thorough scientific study of the subject matter was entirely missing, and merely a compilation of data describing the topic was presented. No information was shared with respect to failure modes. No information was given with respect to the improvement of the design iterations. Very little, if any, data was presented that would help the FC community to understand and develop a startup procedure from freezing temperatures. If any useful information was gathered during the project, it was not shared with the audience. The impression created was that all useful information that may have been created during the project was company owned intellectual property. This is a big conflict, and thus I believe the work should not be funded with DOE funds.
- Significant progress was made in the one year of the project to date.
- There is voltage degradation as a function of stack hours from 2,500 hours to >4,500 hours (still running). There is stable performance with >1,000 start/stops (still running).
- The durability cycle shows membrane leak at 1,200 cycles.
- Testing at sub-freezing conditions shows that the system cannot maintain operation at low power levels (<65A) below -20°C, but only have a 2% power de-rate to -20°C.
- Limited accomplishments were produced for one year of effort.
- The data does not back up the statement that stack performance is 100% recoverable.
- Given that this project recently began, the accomplishments are difficult to evaluate. However, the baseline data set that is forming seems to be favorable. It would be interesting to note what metrics will be improved in the next generation design.

- It appears the focus to date has been primarily on system mitigation strategies to normal (above freezing) start/stop decay, which are known (and have been published in both the open and patent literature). The broader FC community has gained nothing from this project to date.
- There is insufficient evidence of project advancement. There is only one graph that shows the test results generated during this project. In addition, there is limited information given on the test conditions, and that makes it extremely difficult to evaluate the merit of the technical accomplishments.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- It seems collaboration consists of two companies only. Neither a national laboratory, nor a university, nor any other research institute contributes to the work. This may explain the lack of scientific observations and may have led to an engineering approach that is based on a multitude of iterations to develop a product.
- To shift this focus to an approach that would create results that are of value for the entire FC community, the collaborations would have to be extended, the scope would have to be expanded to a less product-oriented objective.
- Partners include Plug Power (Plug), Ballard Power Systems (Ballard), and the National Institute of Standards and Technology (NIST).
- NIST was never mentioned other than on a partners slide.
- It appears that most of the design and development work is being done by Ballard with testing arranged by Plug.
- There does not appear to be close coordination between collaborators. The presenter did not answer any stack questions.
- The collaboration between Ballard and Plug seems strong. However, in the interest of the broader community, and since there is no collaboration outside of industry, it is uncertain how the findings will be disseminated.
- Their collaboration has been limited to Ballard to date. It is not apparent what is intended with their NIST partner, since NIST is never mentioned, and there does not appear to be any fundamental freeze-cell testing planned.
- The team includes Ballard, Plug Power, and NIST as described in slide 2. The individual companies' roles and their collaboration are clearly defined. However, the NIST role is not mentioned, as well as either when or how it will participate in the project. In addition, it is not shown what data NIST will generate, and who will be the primary data beneficiary, Ballard or Plug.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- The project pretty much proposes to continue with the general approach. Since this approach does not create any benefits to the FC community, the presented future work does not offer any improvements. Although some points such as "develop mitigation strategies based on failure analysis" seem valid things to do, no failure analysis was presented. If the failure mechanisms are not shared, not even in a common way, and are only used for product development of the two collaborators, again, no benefit will be given to the FC community and thus, I personally think that such a project should not be funded by DOE.
- Future plans include operable air cooled stack subsystem and commence freeze testing using real motive power application loads and environmental conditions, and then performing failure analysis. Future work should include completing failure analysis of baseline and advanced stack concepts for durability assessment and developing mitigation strategies based on failure analysis, and then building stack systems with mitigation design changes and performing mitigation verification testing.
- Considerable effort will be required in the final year of the project.
- The future work description is very broad, but all elements will contribute to project goals.
- It does not appear that any fundamental freeze testing is planned. Although there will apparently be some additional testing of stacks and a subsystem in a sub-freezing environment, it is not apparent how this will result in improvements in understanding or mitigations, much less how any findings will actually be shared with the community.

- Given the lack of specific test plan details, insufficient experimental data related to durability, and no data on failure analysis, it is not possible to evaluate the relevance of the proposed future work. Furthermore, it is not clear whether the team will be able to accomplish proposed future work within the project timeline.

Strengths and weaknesses

Strengths

- Use of real FC stacks is a strength.
- They are experienced partners.
- They have a clear understanding of market needs, system requirements, and system limitations. Although it was not presented, I expect that the cost benefit of an air cooled system is driving the design. These elements are often lacking in projects not tied to industry.
- None. It has contributed nothing relevant to the topic of interest, and it does not appear to intend to do so either.
- The project has a good mix of team strengths.

Weaknesses

- No sharing of any information that would be of real value to the FC community.
- No science aspect was presented.
- Product development oriented project with only one to two companies as beneficiaries.
- This project has limited ability to advance overall FC technology beyond a small niche market.
- The project does not appear to be focused; and adopts a very general approach.
- It seems like the pendulum between academia and industry swung too far toward industry on this project. It is not clear how this project will benefit the FC research community. This project seems as if DOE is funding the development of a product with no clear mechanism.
- Many. One of the main partners is not a U.S.-based company, which seems particularly egregious when the project appears to provide minimal impact on the broader FC community.
- Results presented to date are insufficient, and there is a concern that the proposed future work will not lead to significant improvements or accomplish the project objectives.

Specific recommendations and additions or deletions to the work scope

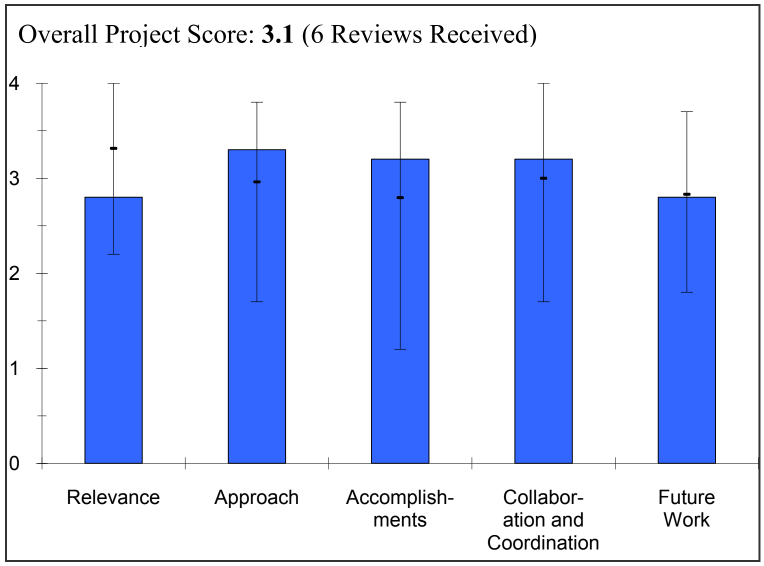
- Add a scientific aspect to the project.
- Redirect work so that it increases general understanding of freeze start phenomena.
- Share findings with FC community.
- Do not waste DOE money for company-owned product development.
- The addition of more DBT cycles would make this a stronger project.
- The project presented is very general. A better focus on the project goals is required.
- Please consider the value of this project to general FC research and adjust such that findings are transferable, not exclusive.
- Stop this project. At a minimum, insist that the balance of the funding be dedicated to freeze-relevant work. However, even this re-direct option would probably not result in a very satisfactory result, since the fundamental issue is that the application which Plug is focused on, does not require developing cold-start capability.
- The team needs to intensify the work in order to be able to accomplish the project objectives within the given timeline.

Project # FC-26: Fuel-Cell Fundamentals at Low and Subzero Temperatures

Adam Weber; Lawrence Berkeley National Laboratory

Brief Summary of Project

Project objectives are to: 1) obtain a fundamental understanding of transport phenomena and water and thermal management at low and subzero temperatures using state-of-the-art materials by examining water (liquid and ice) management with thin-film catalyst layers, and enabling optimization strategies to be developed to overcome observed operational and material bottlenecks; and 2) elucidate the associated degradation mechanisms due to subzero operation to enable mitigation strategies to be developed. Improved understanding will allow for the DOE targets to be met with regard to cold start, survivability, performance, and cost.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Fuel cell start-up and operation in sub-zero temperatures is vital for the automotive sector, but perhaps of less relevance to other market sectors. Perhaps this is a legacy of the automotive focus of the program in prior years.
- The understanding gained may only be relevant to the one company involved.
- Start up, operation, shutdown, and storage at low (freeze) temperatures are important to understand and address for devices that use or produce water, like fuel cells. In the fuel cell, many components might freeze. Evaluation of the effects of freezing wet graphite components and developing possible mitigation strategies is an important and timely investigation.
- The freeze topic is important for automotive applications. I only hope the same level of excellence in execution of an R&D project can be applied to the more urgent water transport issues in operating stacks.
- Understanding transport fundamentals at low and sub-zero temperatures is important for achieving fuel cell performance and durability targets. Understanding physical properties that limit thin electrode performance is most important, as it is difficult to link gas diffusion layer (GDL) water to performance.
- The project addresses the need for more theoretical understanding of phenomena in fuel cells at low temperatures.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Well integrated and planned across the various partners involved.
- The approach is suitable and detailed. The delays in the timeline are unfortunate.
- PI and team have an impressive command of theoretical principles and experimental methods needed to understand the salient phenomena; this project has a very high likelihood of success.
- Stack temperature gradients at freeze shutdown and end cell ice are design-specific and should not be the area of focus.
- It is not clear how all the various experiments link together.
- In general, this project should focus more on "what parameters does the model need" rather than "what can we measure."
- The approach, in principle, is a valid and reasonable one.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- In the short timeframe, much has been done; however, it is unclear how much presented is previously existing data and how much is new. A lot of the data shown is from the industrial partner's own work.
- The accomplishments are in line with the expected completion points within the schedule.
- Very good progress for a project less than one year underway.
- The design map is an excellent graphic and informative tool.
- It is unclear why ice affects electrochemical surface area (ECSA) and the conclusions are debatable.
- Imposed temperature gradients show valid fundamental phenomena, but may not be relevant in real systems, where stacks are insulated.
- Conclusions of the neutron-scattering water distribution experiment upon shutdown were complicated by purging.
- Isothermal start measurements have already been done. Common membranes are required in testing to isolate catalyst effects.
- Several milestones have been significantly delayed and are incomplete, such as the baselining of cells.
- No cyclic voltammograms showing the underpotential deposition of hydrogen peaks, used to determine the ECSA loss, have been shown. Considering the extremely low ECSA of 3M catalysts (10 m^2/g), it is questionable as to how accurately one can measure the ECSA loss due to ice in the catalyst layer. A 10% change in ECSA is about 1 m^2/g, and is within the error of typical ECSA measurements. More details should be presented to demonstrate the validity of this method.
- Progress is a bit behind due to the delays with getting partners under contract, but nothing serious. Progress on the water movement during shutdown and the diagnostics on ice formation in the GDL and the catalyst layer is interesting.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The partners are well coordinated, but there is only one industrial partner. This is a weakness.
- The level of collaboration is appropriate to the topic.
- The roles of the partners are stated, but it is not entirely clear what organizations have conducted which pieces of the work to date.
- This is a very strong team; it is important to show the contributions of all involved.
- Collaboration with LANL and 3M seem to be going well. It is too early to evaluate other partners.
- Catalyst layers from UTC should be used for this project as they may represent a more general, valid, and useful system that deserves detailed study.
- Good balance of partners and resource distribution in the project. This is key to the success of the project, as the partners are providing the input data for the models.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- I am concerned that the desire appears to be to optimize and understand the impact of all other membrane electrode assembly components on the thin-film catalyst layers from the industrial partner without focusing on the source of the problem, which is the ultra-thin catalyst layer itself, with its own unique structure and composition. I feel that the focus should be the catalyst layer, as the project seeks to engage the industrial partner that supplies the thin film catalyst layer and who stands to benefit most from a successful project.
- The proposed future work appears to be appropriate for this topic.
- The vision of the ultimate testing needs to be more fully explained.
- Much work on modeling has already been reported on start-up/shutdown and cold starts. It is not clear what is new in the future work proposed in this project besides the fact that the catalyst layer is atypical.

Strengths and weaknesses

Strengths

- Focus is on a critical issue for the automotive fuel cell sector.
- Good team with input from an industrial stakeholder.
- The strength of this project is the approach to the problem.
- The PI has the team and member capabilities to successfully execute.
- Well resourced, strong team with strong collaborations. Their focus on water transport issues of NSTF is encouraging.
- The general approach is good.
- The team put together for the project is well rounded.

Weaknesses

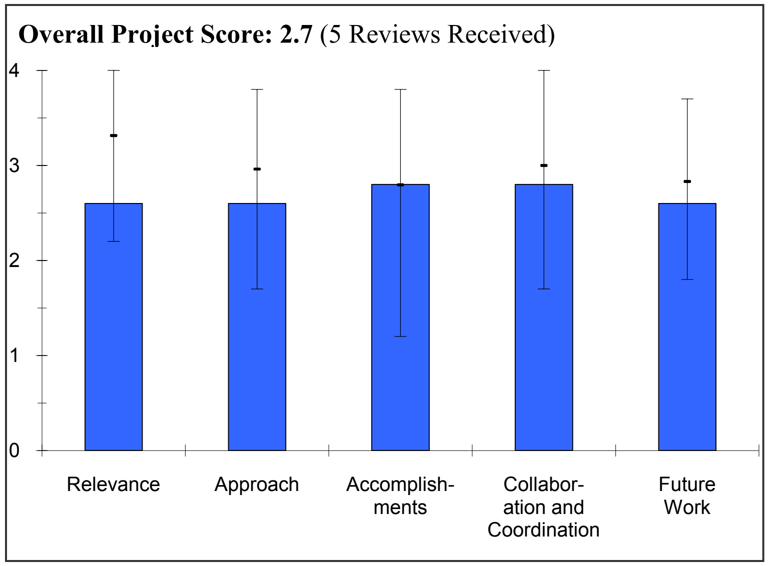
- The focus is on every component but the one with the problem, the catalyst layer.
- The project has limited value to any other party except the single industrial partner. It is possible that this DOE project will give a competitive advantage to a single company, which is undesirable.
- A key weakness may be the lack of re-evaluation of the pore structure of the components after repeated freeze-thaw cycles. Answering the question of whether forces generated by the expansion of water while freezing increase the pore sizes with time, changing the water distribution within and between components after repeated freeze thaw cycles, would help to clear this up.
- The test plan is weak.
- Scattered results from a wide variety of experiments may overcomplicate the modeling effort. Not enough input from OEMs regarding real-world shutdown and startup freeze parameters is a definite weakness.
- The choice of 3M NSTF catalyst layer is highly questionable in terms of usefulness to the fuel cell industry.

Specific recommendations and additions or deletions to the work scope

- Many complicated experiments will require complicated models. Reduce the number of methods and improve rigor with statistics.
- The focus should include the understanding of water and freezing within the thin catalyst layer structure.
- Identifying whether the hydrophobicity of the components change with time in an oxidizing or reducing atmosphere could be useful.
- Clarification of issues at extreme low temperatures (<-35°C) is needed.
- Provide more clarity around the link between experiments and models, and how it all fits in the big picture.
- The project should be changed to evaluate a standard Pt/C catalyst layer currently used in the fuel cell industry rather than the 3M NSTF.
- This reviewer would like to see a bit more work put in the scope for non-NSTF catalysts.

Project # FC-27: Development and Validation of a Two-phase, Three-dimensional Model for PEM Fuel Cells
Ken Chen; Sandia National Laboratories

Brief Summary of Project



Project objectives are to: 1) develop and validate a two-phase, three-dimensional transport model for simulating polymer electrolyte membrane fuel cell performance under a wide range of operating conditions; 2) apply the validated fuel cell model to improve fundamental understanding of key phenomena involved and to identify rate-limiting steps and develop recommendations for improvements so as to accelerate the commercialization of fuel cell technology; and 3) employ the validated fuel cell model to improve and optimize fuel cell operation. Consequently, the project helps to address the technical barriers on performance, cost, and durability, and to achieve DOE's near-term technical targets on performance, cost, and durability in automotive and stationary applications.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- A two-phase, three-dimension transport model backed by cell/stack data will help elucidate transport phenomena in the fuel cell, including the microporous layer (MPL) and interface discontinuities.
- It is intended to provide an executable model to the industry.
- The project addresses the modeling of the performance so far in a one-phase model with future plans to move to a two-phase model. It is not clear how the model will address the objectives of improving cost and durability at this point, except as related to some possible improvement in performance which will depend on the choice of material parameters used in the model.
- The attempts to address performance, cost, and durability are all relevant.
- It is not clear how this project will support DOE's objectives. The PI claims that it will address "performance, cost, and durability," but not a single specific example of how this will be done is provided in this presentation, not even on the future work slide. Perhaps something useful will eventually result from this project, but the PI does not appear to have any clear objectives. If he has any, they are not stated or shown.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- Continuity equations serve as the governing equations. Presumably, these equations will be simplified to make the model manageable.
- It is not clear what is different from other previous modeling efforts.
- Experimental information from Ford and Ballard Power Systems (Ballard) will support the model.
- It is not clear what diagnostic tools will be used to verify the three-dimensional aspects of the model.
- Transient capability (or the possibility thereof) is not evident.
- The modeling approach seems to be a sound development of the parameters for the model moving in a stage wise approach to the two-phase model. The team seems to be seeking good input parameters to develop a model that will be applicable to real systems.
- Development and validation of model progress has been acceptable to date.

- It appears that the major contribution of the PI is to simply incorporate sub-models provided by others (The Pennsylvania State University and Lawrence Berkeley National Laboratory (LBNL)) into a big three-dimensional model. However, it is not clear what additional value this three-dimensional model potentially offers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The project is less than a year old.
- A single-phase, three-dimensional, single-cell model has already been developed.
- The model correctly predicts trends, but absolute values are not always in line with experimental data.
- Progress on the model seems to be good with a good fit to the performance parameters. While progress in modeling has been accomplished, there is no mention of error or error magnitudes, and there is essentially no verification discussion or line-on-line comparison between the model and direct measurement.
- Although a lot of pretty slides were presented, it is not obvious what work done by the subcontractors was actually supported by this project (vs. results obtained on other projects and simply incorporated here). More importantly, it is not clear what anyone was learned (or, for that matter, is going to be learned) from the project.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Use of Ballard's experience to set model inputs and boundary conditions, as well as material properties, is helpful.
- The participation of Ford to provide real-world operating conditions is beneficial.
- The program has good collaboration efforts, bringing together the modeling effort with good experimental measurements of materials properties such as the gas diffusion layer (GDL) and MPL properties, fuel cell performance, and vehicle performance targets.
- Collaborative efforts could be a little stronger. Partners seem academia heavy.
- This project obviously requires a lot of collaboration, since all of the team members must provide substantial input to generate the complex model of the sort envisioned here, especially if the model is also going to be validated (instead of just being used to "predict general trends"). It is also good that the project has an original equipment manufacturer (OEM) like Ford participating with no funding from DOE.
- The PI also takes credit for what seems like a lot of dissemination to date; e.g., two journal publications, two proceeding papers, and three conference presentations were generated so far. A team member also served as co-editor of a book on fuel cells). However, it is not clear that any of this had anything to do with this project (all of the subcontractors have multiple other projects and funding sources), much less what was actually communicated; i.e., what have we learned from this to date that is worth publishing.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- A logical progression of model development and validation is planned.
- Interim model validation will be undertaken.
- The proposed approach of continuing to the partial two-phase model with the validation of the current model seems to be sound. The continued incorporation of the DAKOTA approach to make the model predictive and allow for uncertainty is good. I think it is important to address the water flux as described in the future work.
- Future validation and data generation were specifically mentioned and noted as necessary.
- Not a single specific example of how this project will support the DOE objectives is given in this presentation, not even in the future work slide.

Strengths and weaknesses

Strengths
- The approach of modeling the behavior and trying to build in the uncertainty is an important step. The focus on generating good data for the model, under a range of conditions, as well as gathering fundamental data on the mass transport and the effect of materials properties, is a definite strength.
- It is very challenging to simulate fuel cells on such a detailed level. But it would be great if that works.
- The project has good subcontractors that can provide good sub-models and good validation data. It is not clear what the value of the lead is here.

Weaknesses
- Although the model development is at the early stages, it is not clear how the model's predictive capabilities will address the cost and durability goals. Some more information on how the cost and durability will be addressed is needed in the project work scope.
- Validation of the modeling to date is weak.
- The PI does not appear to be very familiar with what has been done in fuel cell modeling. For example, he states that MPL has not been treated by fuel cell models, which is not true (he appears to qualify this statement with a "multi-dimensional model," but it is not clear what has been, or can be, learned from a "multi-dimensional model" vs. the models that have already been published on MPLs).

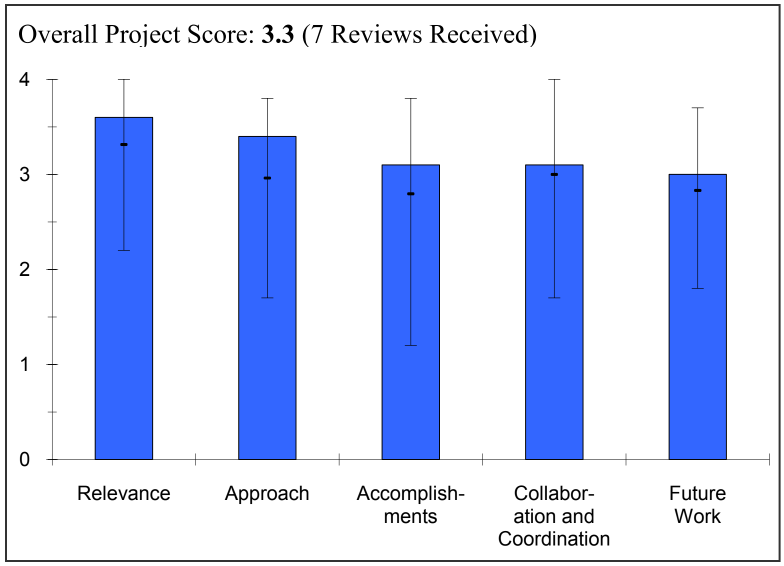
Specific recommendations and additions or deletions to the work scope

- I recommend comparing the simulation results with other simulation groups based on a more commonly used flow field design.
- More validation with possible line on line comparisons or discussion regarding errors and causes is needed.
- Make sure that this project has specific deliverables that actually add value; i.e., milestones other than just model maturity goals. For example, predict trends that are not obvious, or predict decay mechanisms. Otherwise, if the project does not have any clear goals, the project should be stopped.

Project # FC-28: Transport Studies Enabling Efficiency Optimization of Cost-Competitive Fuel Cell Stacks
James Cross; Nuvera Fuel Cells

Brief Summary of Project

The objective of this program is to investigate transport limitations at high current densities in order to optimize the efficiency of a stack technology meeting DOE 2015 cost targets. Goals for fiscal year 2010 are to demonstrate a 1 W/cm^2 stack with 0.2 mg_{Pt}/cm^2 and to implement a predictive model for a high power density version (version 1.0).



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- This project addresses critical barriers to be overcome to achieve DOE research, development and demonstration (RD&D) targets.
- The program is a good fit with DOE cost and performance goals. The program objectives are focused on the goals to address cost and performance, as well as water and thermal management.
- Operating at high current densities is essential to the practical application of fuel cells (FCs) in vehicles. Basic understanding of the cell and stack dynamics is necessary to improve operating performance.
- If heat rejection is carefully considered in the cost model, this project will remain relevant.
- The project is clearly focused on cost reduction and performance.
- The concepts of increasing stack efficiency and cost reduction are in the heart of the DOE program. The center of the program is a detailed predictive model, but this model appears to be only valid for Nuvera. If this is true, then the value of the program should be decreased.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- The overall approach is interesting as it studies high current densities in order to achieve the cost targets and does not take efficiency as the first aim.
- Starting the study on a stack level is appreciated as it should lead to shorter transfer time to a system level.
- The program team appears to be adopting a sound approach to a series of models of the single cell, stack, and the membrane electrode assembly (MEA) behavior. The models are being linked in the overall approach with good testing of the single cell and stack to validate the two independent models. Although the program is in the early stages, it looks like it shows some promise of contributing to overcoming some of the barriers.
- Well-designed, logical project plan. Good use of modeling and experimentation to achieve results.
- Common model development approach that will result in a model that is only as precise as the experimental database. Uncertainty analysis of experimental data is recommended.
- Maximizing power density, even at the expense of overall operating efficiency, is certainly worth pursuing, especially in transportation applications (where the time spent at maximum power is relatively low). The PI seems to have a good understanding of the objections may be to this approach; i.e., heat rejection and possible durability impacts.
- Chart 4 in the presentation is a series of loops that never go anywhere but around the development circle. The presentation did not indicate how the predictive model could be used by DOE. The model will be specific to Nuvera, which is of limited to value to the rest of the FC community.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- Results are correct regarding the start late in 2009.
- The comparison between the Nuvera model, which is based on experimental data fitting, and the new model, which is based on physics, is interesting.
- The difference between the tests in preparation and the first results indicates that the data will be useful in the model.
- The program is still in the early stages, but the initial modeling and data looks promising for contributing to cost improvements.
- Good progress has been made considering the early-stage of the project. Reviewer is looking forward to future presentations!
- Limited results thus far, but too early in the project to accurately score.
- The project has good progress for 15% complete.
- In chart 11, the slope of the Orion baseline has changed because of the thin membrane. The benefit of the Orion system, other than the membrane change, is not clear, since it starts at the JM Andromedia hardware.
- The Nuvera results in Chart 13 show 3M performance for an approximately 66% increase in catalyst content. It is not clear why this is beneficial.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- Collaboration between partners seems correct.
- The team seems to be well coordinated with good initial progress on the program in both the modeling and the testing of the materials properties of the MEA.
- Good collaboration with appropriately chosen partners.
- Collaboration and organization seems strong, but it is not clear how work from the Lawrence Berkeley National Laboratory (LBNL) doesn't overlap other projects.
- It is good that partners not officially included, like 3M, are being included as well.
- It was not clear if the partners were contributing to the program. The Pennsylvania State University (PSU) data was given, but LBNL effort is in future, and the program is 15% complete. Humidified cathode results appear to be new to Nuvera, but others have seen the influence of cathode humidification before. Not sure why this was such a revelation.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Proposed future work is in accordance with the targets of the project.
- The proposed future works is a good approach to combine the MEA model into the model. The program also incorporates goals for improvements of the MEA materials
- Good plans. Will need strong management and leadership to keep project focused. Somewhat unclear what specifics will be done as far as material development; i.e., how to decrease platinum loading without reducing performance and how to increase membrane conductivity (reduced resistance) without sacrificing cycling durability.
- It is not clear what fundamental understandings may result from this project, or how they will be shared. For example, will non-cell design specific barriers to mass transport at high current density be addressed and shared here? Perhaps challenging the targets on system efficiency is sufficient.
- Future work is proposed as activities. The benefit of the future work is unclear.

Strengths and weaknesses

Strengths

- Nuvera has adequate partnership.
- The comparison between a model based on experimental data fitting and a model based on physics is a strength.
- Testing and modeling at a stack level is a strength.
- Good modeling approach using multiple approaches to the modeling of the single cell and stack to understand the key performance cost drivers. The modeling also includes modeling of MEA and plans to incorporate this into the overall model. The goals include improvements in the material properties for improvements based on the model.
- Nuvera has a good team and a good plan.
- Validation of developed model with other simulation from the very beginning. Liquid water visualization in the FC is great to support modeling results.
- Competitive material sets are being evaluated.
- Nuvera has a good objective and approach to achieving the goal.
- Nuvera has a high performance FC system that operates at high power densities with usually higher catalyst loadings. If the design of the Nuvera polymer electrolyte membrane fuel cell (PEMFC) can be shown to operate at low catalyst loadings and also at high power densities, then this concept should be beneficial to the DOE automotive goals. Nuvera has a creative and experienced team.

Weaknesses

- Stack morphology is not a common design. Therefore, the results of this project will be difficult to be applied by the other actors in the FC community.
- Apparently, there is no link with this PSU model and the PSU model developed in the project "Development and Validation of a Two-phase, Three-dimensional Model for PEMFCs" (FC027).
- Material development details are somewhat weak and may get flushed out as project proceeds.
- It is not clear if the high current density goal is applicable to a wide range of FC applications.
- It is not clear what fundamental findings will be derived from this project that can be shared and can help the wider FC community.
- The program will benefit Nuvera if successful. It is not clear how a successful program will help the rest of the FC community.

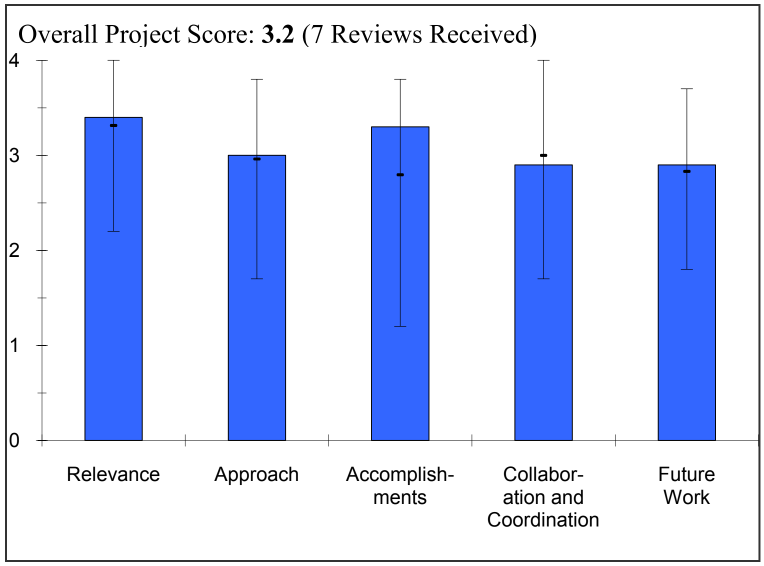
Specific recommendations and additions or deletions to the work scope

- The project covers single cell and stack level, but in order to fully validate the DOE stack targets, the system architecture and the system cost impact should be evaluated.
- Should have more emphasis on fundamentals of mass transport and sharing these findings with the community (via the "detailed predictive model" that is supposed to be central to this work).

Project # FC-29: Water Transport Exploratory Studies
Rod Borup; Los Alamos National Laboratory

Brief Summary of Project

The overall objective of this project is to develop understanding of water transport in polymer electrolyte membrane fuel cells (PEMFCs) (non-design-specific). The objectives are to: 1) evaluate structural and surface properties of materials affecting water transport and performance, 2) develop (enable) new components and operating methods, 3) accurately model water transport within the fuel cell, 4) develop a better understanding of the effects of freeze/thaw cycles and operation, 5) develop models that accurately predict cell water content and water distributions, 6) work with developers to better state-of-the-art designs, and 7) present and publish results.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Water transport deserves and has received a lot of attention lately. This project directly addresses the issue and demonstrates some approaches to managing water transport in the gas diffusion layer (GDL).
- This study targeted some of the central issues in fuel cell water management that are essential to understanding the material impacts on performance and, by association, durability.
- Understanding of water management in cells and stacks will likely lead to enhanced performance in commercialized fuel cells. The ability to accurately predict water content at various points and under various conditions will lead to a better understanding of failure modes, particularly for freeze/thaw. This reviewer would have liked to see an automotive or other large stack manufacturer take a role in the project. W.L. Gore is listed only as a supplier.
- Water management is very specific to a given set of materials such as GDLs, catalyst layers, membrane thickness, flow field coatings, as well as operating conditions of flows, inlet relative humidity (RH), etc. It is not clear how the results of such a general non-design project with varied materials can be combined to produce a high performing stack. Ideally, the project should focus on a particular stack and show how the understanding has improved its performance and durability.
- The understanding of water management is critical for fuel cell performance and durability. This is why this project is highly relevant to the overall DOE objectives and fully supports the Hydrogen Program.
- Water management phenomena are critical to understand to develop fuel cells with thinner, lower-loaded catalyst layers that are less prone to flooding. In this sense, water management facilitates achieving cost targets.
- This project is directly relevant to understanding water management phenomena since it attempts to link membrane electrode assembly (MEA) material properties and operating conditions to observable catalyst layer flooding or, at the least, lower performance that is attributable to greater presence of condensed water within a catalyst layer.
- Water management projects would lose relevance if their results were specific to a particular cell design, plate material, GDL, or other cell component. This project has been designed to avoid this and to provide a general perspective.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach effectively covers modeling, experimental validation, and successful application of techniques to improve water management and, consequently, cell performance by GDL design and materials selection.
- LANL is also attempting to design GDL properties to emulate the aged properties to enhance stability and constant behavior throughout the life of the cell or stack.
- The approach has been very clearly articulated. PI and team exhibit commendable focus and discipline in pursuit of their results.
- While process condition impacts are assessed, the role of compression, the overall mechanical state of electrochemical package, and the flow field structure appears to be absent. There was no analysis presented that suggested these could be neglected for the adopted experimental design.
- The PIs employ a good approach, with a combination of modeling with *in situ* experimentation.
- The use of an approach on a non-specific basis so that general conclusions may be spread across a broad segment of devices is significant.
- To study water management, the team uses advanced *in situ* and *ex situ* experimental techniques. The approach is methodical and scientifically sound. However, it lacks the explicit correlation between experimental results and the modeling effort.
- There are some very interesting experiments designed here, but they do not entirely connect with the fundamental water management "questions of angst" that a fuel cell developer asks, such as: “What material properties help me get out to higher current density?” or “How do I keep from drying out the membrane even though my operating conditions are dry?” For example, the studies of GDLs with different tetrafluoroethylenes are a good benchmarking study. However, a National Laboratory effort should probe deeper. If SGL 24DI is a good water-holding GDL, then why? Is it because the MPL has 10% TFE (hydrophilic relative to sample 25BC), or is it because of a lack of defects in the microporous layer (MPL) (slide 34 in the supplemental slides shows some hint of this work)? What is the water transport mechanism through the MPL and how do the MPL characteristics influence whether or not it happens? Are there contributions from GDL water vapor diffusivity and thermal conductivity?
- Regarding the Schroeder's Paradox experiments, more water is in the membrane when liquid water is outside, but the real question is whether water stays in the membrane when the operating conditions are dry. Does Schroeder's Paradox imply that it would be better to design electrodes with high thermal conductivity so that liquid water will more likely exist outside the membrane? Or will this be negated by the urgency to remove water from a thin catalyst layer to avoid flooding?
- Despite the need for experiments to be more focused on material properties, there are some nice overall trends shown in the work, exemplified by SGL 25BL versus 24BC contrast, where performance losses are clearly more associated with catalyst layer water, not GDL macroporous substrate water.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Modeling and experimental data have been used to design a graded-property GDL that results in a meaningful increase in cell performance, especially in the mass transport, high-current-density region of the polarization curve.
- The results offer fresh insights into gas diffusion media behavior in operating stacks.
- Good, quality work as expected from a top-notch National Laboratory. It is unclear how this knowledge will translate to real-world applications. Seems more like an Office of Basic Energy Sciences (BES) project. It seems like there is already a large body of knowledge around water transport, states of water, hydration in cells, freeze/thaw, etc.
- Although a significant amount of work has been shown on GDL/MPL characterization *in situ* using electrochemical impedance spectroscopy (EIS) and water profiles, a characterization of the different pores, separation of bulk diffusion from Knudsen diffusion, etc. and a thorough study of the limiting currents using different carrier gases for oxygen does not seem to have been explored. Most of the results are qualitative and correlations and valid for arbitrary sets of materials and operating conditions.

- Controversial claims on the validity of Schroeder's Paradox are questionable. The key to the Paradox is the equilibration time, which is not discussed by the authors. There is likely no paradox for long equilibration times.
- The technical accomplishments are outstanding in terms of quality and quantity. The team demonstrated an excellent progress towards water transport imagining under sub-zero conditions.
- For what the project intends to do, the progress is voluminous and outstanding. Although people have talked about ideas like "distributed GDLs," where performance is increased by GDLs that have dry/wet-amenable properties in different regions of the active area, this is the first DOE-funded project to actually show it.
- In many experiments throughout the project, there are examples where *in situ* diagnostics, particularly AC impedance, is used to derive relevant information.
- Successful completion of research efforts to address hypotheses about Schroeder's Paradox and membrane humidification versus compression were shown. It may be interesting to see whether compression redistributes the water between channel and land.
- Higher MPL polytetrafluoroethylene (PTFE) loadings were associated with lower degradation in freeze/thaw, but it would be interesting to find whether it was the PTFE itself that allowed water to exit the cathode catalyst layer, or another property of the MPLs. Recent literature has a lot to say about MPL defects.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.9** for technology transfer and collaboration.

- The team includes National Laboratories, universities, GDL and membrane suppliers, and a stack/system integrator.
- Deep and broad technical competency in the team is excellent. Testing at the stack level with an industry partner could have strengthened the project even further.
- The level of “strong” collaboration is unclear. The team could likely use a stack manufacturer as a close partner.
- I do not see much input from industry on this project in regards to fuel cell and stacks. I have not seen any modeling results and their contribution to this project.
- Very broad and comprehensive list of collaborators.
- The efforts of the team members are well managed and integrated. Collaboration with National Laboratories, academia, and some industry members (Gore and SGL) is well demonstrated. However, it is not clear how Nuvera is involved.
- Collaboration is very instrumental in the execution of this project. The roles of the NIST, SGL, and Gore are fairly obvious in the work that has been done so far.
- The project is taking on new collaborators, as evidenced by assistance to be provided to SNL and LBNL transport projects. ORNL appears to be newly involved in investigations of water in the catalyst layer.
- From the slides provided, the roles of Nuvera and the University of Texas are not entirely clear.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- The project is nearly finished.
- Model validation will be completed.
- Accelerated stress tests (ASTs) will be applied to the graded-property GDL to evaluate GDL property stability.
- Good considering the project is nearly complete.
- Quantitative characterization of mass-transport losses due to water management issues is needed. Input from industry should be solicited. Proof of the usefulness of this type of generalized work should be demonstrated by applying the understanding to a real fuel cell stack and showing overall improvement in performance with the recommended operating conditions and materials set.
- Future work planned completes tasks currently underway while launching appropriate new investigations based on observed data.

- The proposed future work seems logical and aims at generating data necessary to finalize the water profile and multi-phase transient models. However, there is no plan to evaluate different GDL materials and interaction of the GDL and bipolar plate configurations, which may affect the universality of the water transport models.
- The X-ray tomography is a welcome addition to investigate the water transport mechanisms that exist in GDLs. This technique is exactly what should be done to drive the project from near-benchmarking to a fundamental study.
- The intention is noted to share the models that were derived in this project. Some experts have talked about how DOE water transport projects have not yet delivered mathematical models that can be used by the fuel cell research community; hopefully, this project will break that trend.
- The segmented cell measurements are interesting, but they must be accompanied by material characterization and proper focus on water transport mechanisms.

Strengths and weaknesses

Strengths

- The use of a wide array of investigative tools, an understanding of water transport and correlations to performance are all strengths.
- The PIs perform good basic studies, and use powerful characterization techniques.
- Significant amount of work has been shown on GDL/MPL characterization *in situ* using EIS and water profiles.
- An excellent National Laboratory and university team with excellent analytical capabilities is complemented with experienced industrial partners.
- The project is able to deliver considerable volumes of data and experiments.
- The project contains a wide range of collaboration, in both industry and national laboratories.
- The project is able to formulate a hypothesis and deliver data to either prove or disprove the hypothesis. Unfortunately, there are not many DOE projects that are as adept at doing this as this project has proven to be.
- The investigators have considerable experience in the field and demonstrate cutting-edge knowledge of the issued involved.

Weaknesses

- The testing at larger active areas in stacks and lack of investigation of flow field/GDL interactions are both weaknesses.
- It is unclear how the findings will translate to real-world applications.
- Characterization of the different pores, separation of bulk diffusion from Knudsen diffusion, etc., and a thorough study of the limiting currents using different carrier gases for oxygen do not seem to have been explored. Most of the results are qualitative and correlative and only valid for arbitrary sets of materials and operating conditions.
- *Ex situ* measurements of water transport properties of porous materials need to be conducted and related to the improvements in the cell.
- The PIs need to demonstrate industry interest in this kind of work to improve stack performance, and conduct and demonstrate stack testing.
- The influence of the fuel cell flow field-GDL configurations is not considered in the project.
- The project has a large scope and more discipline is required to chase after the issues that are most meaningful to fuel cell stack developers.
- The cell-level experiments need to be better connected to water transport mechanisms and material properties. To be sure, there are examples within a project with as large a scope as this one where the connections are made, but on some important experiments, the connection is not made.
- Modeling could play a more significant role in explaining some of the data obtained and towards generating more useful experiments.

Specific recommendations and additions or deletions to the work scope

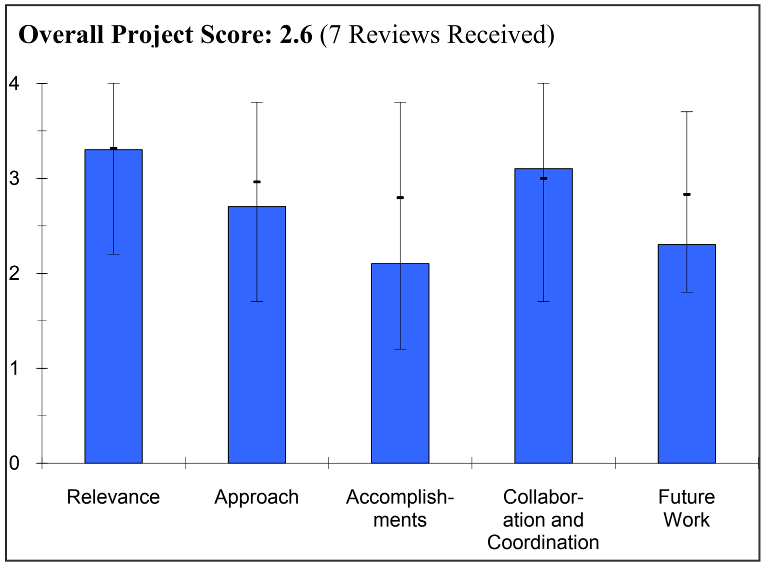
- More modeling results would be useful to see.
- The PI needs to summarize the relevance of experimental data to the overall project goals.

- The project could take the cell-level results that have been generated, and spend more time understanding component parameters and water transport mechanisms. This understanding might lend itself to improved water transport models.
- The small effort to understand how to measure water presence in a catalyst layer might benefit the research community tremendously, if successful.
- There should be some reporting on whether GDLs in segmented cells are all the same thickness or whether there might be unequal compression on the GDLs. It would be interesting to know whether this influences performance and impedance measurements.

Project # FC-30: Water Transport in PEM Fuel Cells: Advanced Modeling, Material Selection, Testing, and Design Optimization
Vernon Cole; CFD Research Corp.

Brief Summary of Project

The overall objectives of this project are to: 1) improve understanding of the effect of various cell component properties and structure on the gas and water transport in a proton exchange membrane fuel cell (PEMFC), 2) demonstrate improvements in water management in cells and short stacks, and 3) encapsulate the developed understanding in models and simulation tools for application to future systems.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The control of water transport in fuel cells (FCs) is a major performance issue; this project intends to address and optimize the water transport issue and is certainly relevant.
- Optimized water balance and water transport is critical to FC performance, cost reduction and commercialization.
- Understanding water transport fundamentals in FCs is critical for achieving DOE performance and durability targets.
- The project addresses water transport, which is a major technical barrier for automotive FCs.
- The project objectives are overly generalized and not specific. It is not clear what is being measured, determined, or how the results integrate with the overall goals of EERE's FC efforts.
- The project addresses water transport in a FC—a critical technology barrier that affects FC performance, durability, and cost. The project is relevant to the DOE Hydrogen Program and fully supports DOE objectives and goals.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The proposed approach is focused on using computational fluid dynamics (CFD) to model water transport in FCs with the emphasis this year on gathering experimental data and modeling water transport in membrane electrode assemblies (MEAs). This approach supports the project's goals and objectives of using CFD modeling to improve overall FC performance.
- The approach correctly develops the fundamental knowledge, models, and applications to improve water transport within PEMFC. This includes improved understanding of the effect of various cell component properties and structure, demonstrating improvements in water management in cells and short stacks, and encapsulating the developed understanding in models and simulation tools.
- The methodology to advance the science includes Lattice Boltzmann Modeling models, CFD models, *ex situ* characterization of components (gas diffusion layers (GDLs)), and *in situ* diagnostics including current and water distribution. All are excellent methods to advance the science.
- The relative permeability experiment is not clear. It is also unclear how liquid saturation is achieved and quantified.
- Water transport is best measured by doing direct water balance rather than pressure drop measurement.
- There was no mention on whether liquid water in the anode is considered.

- Water accumulation in u-bends of serpentine channels is already well established.
- It's not clear how wet flow distribution in a four-channel array is relevant. It is not clear whether the final model includes inlet liquid water.
- They need to look at extreme operating conditions (low temperature/low stoic).
- While the overall approach for experiments and modeling is good, the mathematical approach for solving the transport equations does not appear to be the correct approach. Model convergence and numerical stability are a problem.
- The approach of combining modeling work with experimental characterization and the introduction of improved FC component concepts sounds good on paper, but the actual accomplishments of the research team were below expectations, and I did not see the integration of project tasks and project results.
- The team has a good approach in studying water management. They use the *ex situ* characterization of key materials properties to develop a model that is verified *in situ* diagnostic tests. They also incorporated the GDL-flow field that is critical for water management. However, the approach is missing the effects of compression that is of a significant importance for water management through GDL.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.1** based on accomplishments.

- Although progress has been made in the past year, the project is behind schedule. Validation problems were noted as the reason for the schedule slip; validation of the CFD models developed in this project is critical to the ultimate success of the project. Thus, it is important that model validation be confirmed before water transport improvements can be identified.
- The gas as permeability was found to increase significantly below a water saturation threshold (typically 20%) for SGL Group media; however, in operating FCs, GDL water saturation rarely is above 20%. This does not appear to be the limit issue in terms of performance related to mass transport limitations.
- Significant work on droplet emergence analysis, prediction, and measurement has been produced; however, other projects have shown that this portion of the water transport is not the rate-limiting area leading to mass transport losses.
- The model evaluation for the operational cell and comparison to experimental data leaves much to be desired. The single-phase model and two-phase models do not appear to agree. Neither model appears to agree with the experimental data, which is likely due to the fact that the experimental data has issues. Current distributions along a cell length do not typically look like the experimental data used in this project. Therefore, it is relatively impossible to match the data.
- Identified "intermediate" polytetrafluoroethylene (PTFE) loading as optimal. This is a trivial conclusion, not quantitative, and not a substantial improvement, as this was known more than a decade ago, with actual loading numbers presented.
- Wicking channels have been around since fuel cells and the Gemini program. Hydrophilic surface coating for channels as well were widely discussed, but difficult to implement in mass manufacturing.
- Some caution is recommended to avoid the model improvement being based too much on test data and less on first principles. The task is modeling and not adaptation of a first model until it fits.
- One cannot validate the two-phase model. All studies based on the model are questionable without validation.
- Wicking channels have been shown to reduce pressure drop. One wonders if that can be done cost effectively. Also, wicking channels may create other problems such as water accumulation in the smaller channel that will be harder to remove.
- Code convergence and stability problems are slowing progress.
- The models do not do a good job of matching experimental data. One-phase flow model provided better projections than an improved two-phase model. The project needs to validate the models with *in situ* data and verify that the models can predict observed behavior and are applicable to real systems.
- Obtain some water balance data to validate water crossover.
- The microporous layer (MPL) water transport needs to be validated.
- More details on options being pursued to improve transport would be helpful. For example, the wicking channel design needs principles behind it to be of use to people with different plate designs and materials.
- The water management in FCs is an ongoing concern. It is not clear whether this project has gotten us closer to solving or understanding the water management problem in PEMFCs. It was difficult to assess the modeling

work because no details of the theory were presented. Plans and milestones listed numerous challenges such as difficulties, in the approach. How was water transport modeled in the membrane? How do the model results compare with experimental data? I do not see the value of flow visualization (droplet emergence) experiments. What important conclusions were drawn from such work? Also, the absence of water balance measurements was troubling.

- The team presented very good experimental results regarding water transport using screening tests at the component level. However, the results of the experimental model verification show discrepancy between the experiment and modeling results. The models developed require further refinement in order to achieve the project objectives.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- Good collaborations with industry and university partners. The partners appear to be playing an active role in the project and are making substantial contributions. Several publications in the technical literature were listed.
- The overall team and partners are an excellent list of collaborators. The team seems to be a good collaborative unit.
- Ballard Power Systems (Ballard) provides materials and data, but little details on materials or designs. The University of Victoria did poly(dimethyl siloxane) chip droplet dynamics studies for controlled experiment, mimicking water emergence at channel/GDL interface. The input of the other partners is not clear.
- The collaborations within the project partners are good. Collaboration outside the project partners is not evident.
- The project has numerous collaborative partners, but it appears that most of the work was carried out by Ballard and BCS Fuel Cells. The coordination of subproject tasks and the integration of subproject results are both questionable.
- The team involved is well balanced with credible members. However, it is not clear how the information exchange is managed within the team members.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Plans for future work are adequate—emphasis should be on completing model validation and then identifying water management improvements.
- There needs to be convergence between the experimental data and the different models.
- There was no mention or representation of the Lattice Boltzmann modeling. That was a portion that was novel to this project.
- They should not explore new channel designs and surface coatings unit the two-phase model is validated with the experimental data.
- The future work slide in the PI's presentation is far too generalized. The future work appears to be nothing more than a continuation of present project tasks. For example, it is highly doubtful that BCS, Techverse, and CFDRC will ever do a specific GDL modification.
- The proposed future work seems logical and in agreement with the project objective. However, the plan does not include the evaluation of different GDL materials and effects of stack compressive forces. They are not considering that these aspects can dramatically affect the model usefulness for predicting FC performance.

Strengths and weaknesses

Strengths

- They have experience and expertise in CFD modeling.
- Two-phase permeability measurement could be a useful test that others can employ.
- The overall project tasks (understanding and improving water management in an operating FC) are important and of relevance to the DOE. Some of the results are worthwhile.
- The team demonstrated excellent component screening tests.

Weaknesses
- Validation is lagging behind schedule—milestones have slipped.
- The experimental data used for model comparison does not appear accurate from other literature.
- The project examines a portion of water transport (droplet emergence from the outside of the GDL), which other developers have shown to not be the limiting issue with water transport and related to mass transport limitations.
- Model details were not provided, so there is little use for the community.
- Details of wicking geometries were not provided, so there is little use for the community.
- The studies focus on serpentine flow fields, which are not of interest to original equipment manufacturers.
- Overall, more experimental rigor is required.
- Model stability and convergence issues and the model have not done a good job of replicating data.
- Project accomplishments were not impressive.
- Model details were not presented and the accuracy of parameters in the model are not known.
- No specific recommendations were given for new flow channel designs.
- No preliminary data is provided, which material properties need to be modified to optimize water management and how they will be included into the model. On slide 16, they mentioned that intermediate PTFE loading is optimal for a particular GDL; however, the PTFE loading can affect not only the GDL wettability but also can change permeability, thickness, thermal and electrical conductivity, etc.

Specific recommendations and additions or deletions to the work scope

- Focus near-term on completing model validation.
- Perhaps a go/no-go decision is appropriate prior to beginning cell experimental activities.
- The experimental data for model comparison needs additional work, and is likely the cause of significant differences. The different model agreements should be better aligned.
- Revisit fundamental material parameters as part of two-phase model validation efforts. Perhaps do a sensitivity analysis of membrane water diffusivity or electro-osmotic drag coefficient, etc.
- In the future, the team plan must include the effects of compression on the GDL properties and the material anisotropy that can drastically offset experimental and modeling results.

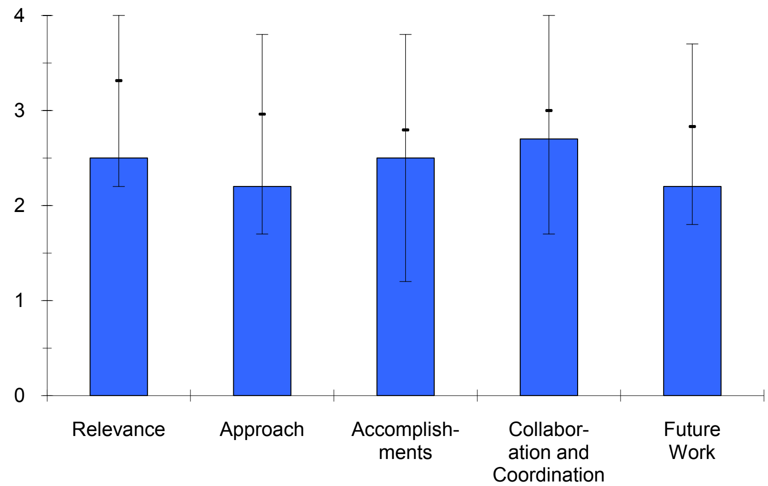
Project # FC-31: Development and Demonstration of a New Generation High Efficiency 10kW Stationary PEM Fuel Cell System

Durai Swamy; Intelligent Energy

Brief Summary of Project

The overall objective of this project is to develop a high efficiency 10-kW proton exchange membrane fuel cell (PEMFC) combined heat and power (CHP) system and to demonstrate it in an International Partnership for the Hydrogen Economy (IPHE) country (United Kingdom (UK)) outside of the U.S. Project objectives for 2009 – 2010 are to: 1) build, test and validate high efficiency (absorption-enhanced reformer (AER)) fuel processor bench-scale rig with 40% electrical efficiency, 2) build and test an integrated CHP system with multiple thermal recovery streams to achieve greater than 70% efficiency, and 3) perform 24-hour a day stack testing to demonstrate 40,000 hours durability.

Overall Project Score: **2.4** (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- The relevance is good: project to develop a high efficiency 10-kW PEMFC CHP System. However, it seems the Project Objectives for 2009 – 2010 to meet DOE targets should be updated to meet the new 2015 targets.
- The goal of this product is highly relevant. The published goals are aggressive and are approximate to the current, published accomplishments of commercial phosphoric acid fuel cell power plants. Electrical efficiencies should be achievable. Overall efficiencies and durability should be an interesting challenge, especially for direct hydrogen systems. It is unclear what the level of reclamation of heat is for the CHP. It could be envisioned as hot water or space heating.
- Should goals be met, the relevance would be good or better. There is not a compelling path to the stated goals in the content provided.
- This project does not offer any technology or system advancement to the program. The presenters did not demonstrate any reason why this technology integration would offer an advantage over existing projects if they were to be successful. The systems engineering was not present to drive component selection, cost management or market understanding.
- There isn't anything new in the project and there is virtually no R&D work, despite the starting point of the project severely underperforming relative to goals.
- The "AER" doesn't make much sense. It is very difficult to believe that carbon specie adsorption will exceed the current 95 % or so, and this isn't close to being sufficiently pure for a low temperature fuel cell (FC). They are stating that gas shift or pressure swing adsorber (PSA) is not needed and doesn't make sense. It is difficult to believe that ultra-high purity will be achieved without a PSA. Without shift, instead adsorbing carbon monoxide, which will always be in equilibrium with the carbon dioxide, it reduces the energy that can be extracted from the fuel.
- Project is in line with DOE 2011 targets:
    - Project objectives.
    - 2009 – 2010 objectives.
    - 40% electrical efficiency.
    - Build, test and validate high efficiency (AER) fuel processor bench-scale rig.

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- This project includes the phased development of an open architecture system with a pure hydrogen interface between the FC and the fuel processor.
- Assuming it is successful, there are advantages to this approach.
- Excess electricity is not going to the grid, but is being dissipated. This approach is not great for a high efficiency system.
- The approach appears sound. It is suggested that the design conform to EN62282-3-1 (the International Electrotechnical Commission (IEC) equivalent to ANSI FC1-2004).
- The approach appears to be a bit of a hodgepodge with some elements introduced to resolve issues (such as the AER).
- The project has selected steam methane reformer (SMR) + PSA + PEM for a heat and power application. This selection demonstrates a very poor understanding of the system application and integration.
- PSA is a poor choice for a low cost system, as it does not offer any advantages in such a system.—The presenter claimed that it would simplify the system and offer higher hydrogen utilization. However, a PSA unit is a very high cost component, especially at this scale, and even as established technology, it does not offer low cost or complexity. The presenter suggests that PSA would allow one to use all the product hydrogen for electricity production, while in reality, PSA units have a finite hydrogen recovery capability. High recovery ratio of ~85% may be practically recoverable with a PSA unit, which is similar to the innate recovery rate of a FC operating on reformate in the first place. The PSA thus does not provide any benefit in hydrogen recovery and overall electrical efficiency. The other point the presenter offered was that PSA units would block the carbon monoxide from getting to the stack. This would be true, but this function can be accomplished by significantly cheaper and simpler means; i.e., high temperature shift + selective partial oxidizer catalyst. Alternative solutions would have a small fraction of the cost of a PSA unit.
- The next question is the selection of PEM for a CHP system. Typically, hydronic systems operate in the 60°-80°C range. Therefore, coolant coming to the house water coming to the CHP unit is at 60°C, and the system has to heat up the coolant to 80°C before returning it to the house heating loop. When asked, the presenter offered that stack heat would be first applied to the coolant loop, followed by the higher quality heats from the reformate cooler and the system exhaust. However, in such a scenario, the heat management does not add up. Stack heat in a high efficiency system should account for a vast majority of the heat generated from the system and reformate heat and exhaust heat are a small fraction of that. However, the stack is operating typically in the 75°C to at most 85°C range. This means that transferring heat to a coolant loop may drop the heat quality to 65°-75°C, and then once more through a liquid heat exchanger to the house heating to 55°-65°C (house water cannot be run directly through the stack to gather heat since it has high impurities level and thus is electrically conductive and shorts the stack. Therefore, two temperature approaches have to be considered in transferring heat first to the stack to a coolant, and then from the coolant to the house water). The stack heat can at most heat the house water—best case from 60°C-70°C. With such a temperature rise consuming a majority of the heat available from the system, there is no longer sufficient heat available to accommodate the remainder of the temperature rise.
- Besides the system's useful heat rejection requirement, the system has another major issue. It requires that water be dropped before entering the PSA unit. PSA materials get damaged from moisture content. The presenter suggested that 40°C is required before the PSA unit. In a CHP application at this scale, such heat sink is not available unless they tap into a radiator heat sink or such. Commonly, however, this is not an option. At best, they may be able to sink heat into the water going to the house (which is intermittent). The system analysis is further double-tapping this resource, as they claim that they would use the heat for CHP applications!
- The presenters suggested that mass and energy balance modeling has been performed for this concept. However, they did not offer any of the analysis results and opted for a cartoon chart of arrows. This is very in appropriate for a technical review if adequate analysis was actually performed!
- There is virtually no development work in the project, so no real expectation that the system will perform better than the many others of this type that have been tested. The "AER" doesn't make sense from the start and appears to be the only development effort.
- A statement is made that the balance of plant (BOP) will use "fixed orifice devices," which are usually assumed to be eductors or something of that nature, of course it is difficult to tell, and is, for the review, a problem itself.

Typically, "fixed orifices" are a problem because of the narrow, if not "fixed", operating range. This usually causes problems for off-nominal operation, which will occur due to normal degradation, load changes, etc., and these types of systems typically require a blower or compressor for transient operation anyway, which increases the overall part count plus a motive system for the eductor. In almost all cases where an eductor is analyzed, it is rarely worth the negatives. If not an eductor then it's not clear what the reference to "fixed orifices" is, but that is the presenter's responsibility.

- Phased development of an open architecture system with a pure hydrogen interface between the FC and fuel processor is focused on overcoming the barriers of improved FC performance, increased FC lifetime, lower FC cost, smaller reformer, high fuel utilization, simplified integration, independent operation of the FC, and fuel processor plug and play."

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- The high efficiency promised by this project is lacking to date.
- The chemical efficiency of natural gas to hydrogen = (21574 + 2700)/35635 = 68.2%. The chemical efficiency of FC = (10,370 + 610 + 620)/21574 = 53.4%. Direct current power delivery efficiency = 10,370/(10,370 + 610 + 620) = 89.4%. End-to-end Efficiency = 32.6%. Combined CHP/electrical = 60.8%, which is not very close to any targets.
- The accomplishments to date are impressive.
- There has been some progress on life testing, but far less than is needed for practical application.
- The project has done some system integration, although in a misguided direction. This may be better suited to an education and technician training exercise than a systems development effort.
- It is claimed that a system was tested, but the data is inconsistent. The FC would not have survived more than hours at best on 95% pure hydrogen as indicated for the AES system. There are other data discrepancies. The polarization (IV) curve and area specific resistance (ASR) don't agree. Based on the IV curve, the ASR should be about 1.0 ohm-cm^2 versus 0.06 ohm-cm^2 or so.
- Lifetime, durability, and efficiency are all well below goals and, given the lack of development work, there is no expectation that this will improve. The data shown is typical for a low temperature FC system.
- Cost was not addressed other than saying that the unspecified cost would get lower with higher production numbers.
- A degradation rate is given for 7,000 hours but the data indicates that the stack (cells?, not sure what the system was) are failing catastrophically at that point, so a "degradation" rate isn't particularly meaningful with a 40,000 hour goal.
- Many accomplishments have been achieved, including detailed engineering completed June 2009, construction and shakedown tests completed October 2009, first power production achieved in November 2009, automated combustor startup and full system safety shutdown implemented in February 2010, system in continuous operation since March 2010 (350 hours) achieving 33% efficiency (end-to-end efficiency = 68.2% * 53.4% * 89.4% = 32.6%), and continuous hydrogen production at low temperatures with >95% purity achieved.
- However, the hydrogen generator efficiency is below expectation due to natural gas (rather than PSA off-gas) firing of the combustor.
- 4 of 8 metrics achieved during CHP test.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- There is a list of partners, including the California Polytechnic University, the University of South Carolina, Sandia National Laboratories, Intelligent Energy, and IPHE partners Scottish and Southern Energy, Logan Energy, Element Energy, and Nedap.
- It is clear what Intelligent Energy is doing in the project, but it is unclear what the other partners have contributed to date.
- The collaboration partners are suitable. Leveraging the expertise of Logan Energy is important.
- There is a good mix of partners, though their roles are a bit unclear.

- Collaborations were listed on the slides. If collaboration with these entities was effective, I would have expected much better results.
- Several collaborating institutions are mentioned, but the work plan doesn't indicate what, if anything, they are doing. There is no evidence of collaboration in the presentation. The building identified as the demonstration site in the UK that is under construction seems rather large to be built solely for a demonstration. No other details are given concerning the purpose of the building.
- Good technology transfer between a joint venture with Scottish and Southern Energy, system-installers subcontractor Logan Energy, site modeling and controls subcontractor Element Energy, and an inverter development in collaboration with Nedap.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.2** for proposed future work.

- System optimization (SMR+PSA+FC) and redesign to accommodate future plug-in AER fuel processor—this should be the priority, as the system does not currently have the efficiency required to be marketable. However, there was not a plan articulated to meet the DOE targets for efficiency.
- System optimization includes shorter PSA (so it fits through the door). Seems like a rather obvious development that is required.
- Demonstration of system with IPHE partners.
- The future work appears to be appropriate.
- The link between the future work and a quantitative estimate of potential progress to goals is unclear.
- It seems that a relatively standard PEM system that doesn't meet EERE goals will be operated in 2011. The AER, the only developmental aspect of the project, won't be operated and requires additional funding to complete and test, negating any actual advances associated with this project, assuming the AER is an advance. In 2012/13, all EERE goals will be achieved despite no actual R&D work indicated prior to that time and the current system well short of these goals.
- The hydrogen generator efficiency is below expectation due to natural gas (rather than PSA off-gas) firing of the combustor. Need 80% perhaps in future to develop a better AER. Intelligent Energy may need more funding.
- The six month field demonstration in UK is good.

Strengths and weaknesses

Strengths
- The strengths of this project appear to be the focus and the collaboration.
- The project has good focus on CHP at micro-scale.
- Overall, it is a good project.
- The project is interesting.

Weaknesses
- Overall system efficiency is not close to the targets.
- System is currently too big for intended applications.
- The potential weaknesses may include deodorizing (desulfurization) of the fuel gas and recovery of usable heat.
- There is an unclear link between tasks and gaps to current goals. Carbon monoxide content in AER exhaust is not specified and may require added clean up.
- See comments.
- It needs more funding to achieve 40% efficiency.

Specific recommendations and additions or deletions to the work scope

- System optimization (SMR+PSA+FC) and redesign to accommodate future plug-in AER fuel processor should be the priority, as the system does not currently have the efficiency required to be marketable.

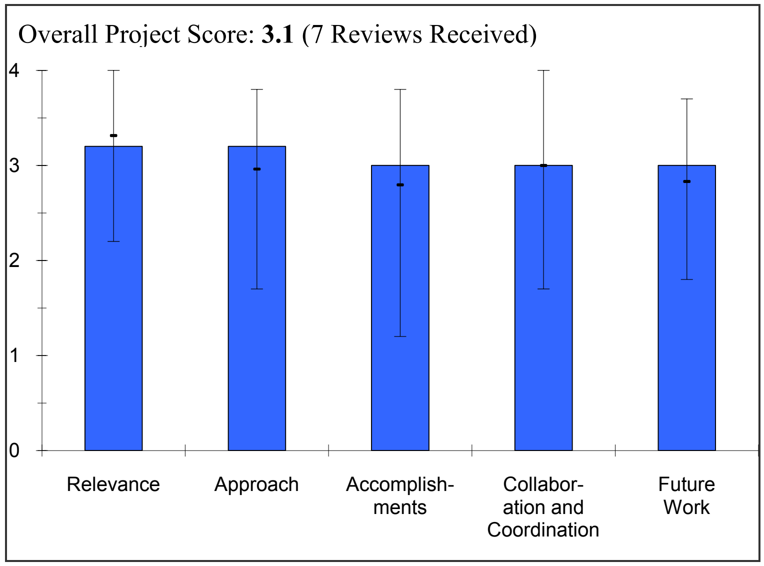
- This reviewer suggests a detailed review of the AER approach to ensure that it should stay in the program. Also, the project team needs to ensure that detailed benchmarking is conducted against the Japanese EneFarm activities.
- The group leading this project does not appear to be capable of systems development. This funding could be applied in other projects and provide actual technology development improvements.

Project # FC-32: Development of a Low Cost 3-10kW Tubular SOFC Power System

Norman Bessette; Acumentrics Corporation

Brief Summary of Project

Acumentrics is developing SOFC systems for a variety of applications, including remote power, military and micro combined heat and power (micro-CHP). The objectives for this project are to: 1) improve cell power and stability; 2) reduce the cost of cell manufacturing; 3) increase stack and system efficiency; 4) prototype test meeting system efficiency and stability goals; and 5) integrate to a micro-CHP platform.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project is aligned well with DOE objectives for stationary power and CHP applications. The objectives are to: improve cell power and stability, to reduce cost for cell manufacturing, increase stack and system efficiency, and integrate to remote power and CHP platforms to allow short, medium, and longer term market penetrations.
- The project has good relevance to stationary technical targets.
- This system is definitely relevant to the DOE objectives, as it pursues high efficiency, durability, and economics of use for a potentially renewable fuel (biofuels). Currently, the system is focusing on feed stocks that are non-renewable, but is also leveraging niche and mass markets to develop the technology further.
- The work is consistent with a recent (Dec 2009) Request for Information and proposed goals for CHP and auxiliary power units (APUs).
- A solid oxide fuel cell (SOFC) is most useful as a fossil fuel power generator, although its advantages in reversible operation make it an excellent choice for energy storage. By using ultra-pure hydrogen as a fuel, the SOFC will, in general, not perform as well as a low temperature FC. However, an SOFC that meets goals, and an SOFC can certainly meet the EERE goals, is better than no FC at all and still should outperform reciprocating engines, and will certainly outperform a micro-turbine, particularly for distributed applications. A high performance state-of-the-art SOFC will also outperform the competition in large generation. In that sense, the relevance is good. The objectives of the project do address achieving the DOE goals.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The project works to improve performance by improving the individual system components.
- The PI discussed cell manufacturing to improve processing yield and productivity, but these are not being improved by cell manufacturing in this project.
- The project could benefit from some better analytical methods, cost breakdown and/or efficiency estimates.
- This is one of the most complete system development companies I have seen. I was very impressed with the work done.
- To the extent that technical details were disclosed, the approach appears sound. There is appropriate focus on power density, stability and manufacturing process development.
- The project identified "barriers" well. Rather than barriers, this is a list of areas that could be improved, simultaneous improvement in all of them is not required to meet goals.
- The objectives are well stated and the focus is correct.

- The approach is very R&D focused and is focused on the right issues. This organization knows what needs improvement.
- The 1,500-hour durability test looked stable, but there may be a need to run a longer test. It has been demonstrated that there shouldn't be catastrophic failure in a well designed, manufactured, and operated SOFC (one of its advantages), so the short term tests are valid, but at some point, long-term performance needs to be verified. This stable long-term performance has been demonstrated by Siemens and Versa Power for 80,000 (tubular) and 27,000 hours planar—the planar was stable but stopped for analysis after showing stable performance and degradation around 1%/1,000 hours.
- The project's list of barriers include: cell power density, stack power density, cell cost reduction, system cost reduction, system efficiency, and lifetime.
- The approach is attacking barriers by improving cell power and stability, stack and system efficiency, the ability to operate on liquid fuels (funded through the Office of Naval Research (ONR) and the Department of Defense (DOD)), and integration to remote power, military, and micro-CHP platforms to allow short, medium, and longer term market penetrations.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- This project has shown a high degree of cell performance. However, this big percentage improvement is due to the cell performance in prior years being very poor. The polarization curves shown are still not state-of-art for SOFC performance at these temperatures (750°C and 800°C).
- The project states that the operating temperature has been reduced by 100°C and could be reduced by 200°C, which is an excellent achievement; however, this is not shown by the data presented. Other developers show 0.7 V at 1.0 A/cm^2 at 550°C, compared with the 0.6 V at 1.0 A/cm^2 and at 800°C in this project.
- The project exhibited good overall progress, but they need to recognize that the lifetime achieved is very far from where it needs to be.
- The project demonstrated improved system design, life, and efficiency, while having full consideration of system cost implications. This is all one could possibly ask for in driving to meet market needs.
- One percent per thousand hour degradation is a good status. Power density has improved; however, this needs to be presented in the context of operating cell voltage and fuel utilization, along with associated system efficiency. For example, what were the operating conditions associated with the demonstrated ~41% lower heating value (LHV) efficiency?
- The achievement of efficiency is easy at the expense of power density.
- The project showed cost targets based upon the CHP metrics, but the associated status of this technology was not. At the program level, it's by no means clear that residential FC CHP is a viable domestic market.
- Thermal/electricity demand profiles versus CHP system was a relevant accomplishment.
- The project demonstrated good BOP/integration progress.
- Good progress has been made in maintaining cell performance while reducing temperature. This is important to cell/stack life and provides operational flexibility. Power density has increased substantially, this is a major contributor to reducing cost.
- The project's present status reflects progress as follows:
  - Efficiency proven over 40% on stack in a demonstrated system in 2010/2011.
  - CHP efficiency of 85% proven on 1kWe wall hung systems.
  - Demonstrated start-up and load transients as part of ONR liquid fuels testing.
  - The latest generation systems operated with stacks over 5,000 hours and total system tests over 8,000 hours.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Collaborators include EFESO, however, it is unclear if these collaborations are part of this project, as they appear to be a separate project. Other collaborators are listed, but there is no indication about what they are doing.

- The project demonstrated good leverage to other government programs.
- The company has worked with its customer base very closely to provide and supply products that are ultimately accepted by the market. The bottom line is sales.
- The project builds upon Solid State Energy Convergence Alliance (SECA) work, and Acumentrics leverages needed outside expertise appropriately.
- The project has demonstrated good collaboration through multiple government projects. Funds through ONR for fuel processing and small business innovation research (SBIR) for an important heat exchanger development are well coordinated with the work in this project. The activity in Italy is also impressive, although it is not part of this project. Technical collaborations do not appear to exist, although, given the funding level, it would be difficult to support them considering the financial requirements required for a commercial effort.
- The Italian government program granted to Ariston thermal group and 15 partners including Acumentrics is a key partnership.
- Acumentrics is the first foreign company to be issued an Italian government grant for a green energy program.
- Three year, $1.1 million program culminating in a 1-$kW_{electric}$ and 2.5-$kW_{electric}$ CHP prototype.
- New ONR $10 million project on logistics fuel.
- Acumentrics utilizes key technology contributors on inverters, balance of plant (BOP) components, testing laboratories/universities, and certifying bodies

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The project's future work appears straight forward, and is required for commercialization.
- Efficiency enhancement should be a key focus of future work , (as indicated, with cost as a constraint).
- Keep doing what you're doing!
- The project's future work is appropriate.
- There was no direct slide on future work. Clearly there will be future work, and the entire project is focused on the right tasks, but unfortunately this topic is not directly addressed.
- The project's future work, consistent with project objectives, should assure cell stability, correlate stability versus current density, demonstrate stability over thermal cycles, compare results of catalytic partial oxidation (CPOX) and steam reformed systems, resolve thermal issues in stack due to higher power density, test improved thermal management techniques, continue cost reductions on each product platform, continue cell manufacturing automation, and continue "make/buy" decisions on generator and BOP components.

Strengths and weaknesses

Strengths

- Acumentrics shows the ability to manufacture cells and SOFC stacks with developing technology.
- There is good focus on micro-CHP for SOFC.
- The functional demo systems are a strength of the project.
- The project is fully focused on accomplishing fuel cell improvements.

Weaknesses

- There is less focus on analytical methods (simple semi-empirical analytics are fine) than would otherwise be optimal.
- By nature of the open review, reviewers don't have access to material details. For example, what material changes permitted the operating temperature reduction?
- The project could put effort into assessing potential stack lifetime-limiting issues, given the lack of viable accelerated testing protocols. Engineering analysis and microanalysis can pinpoint areas of concern (e.g., materials volatility).
- It's not clear if the project will achieve a high enough power density to get to cost targets.

Specific recommendations and additions or deletions to the work scope

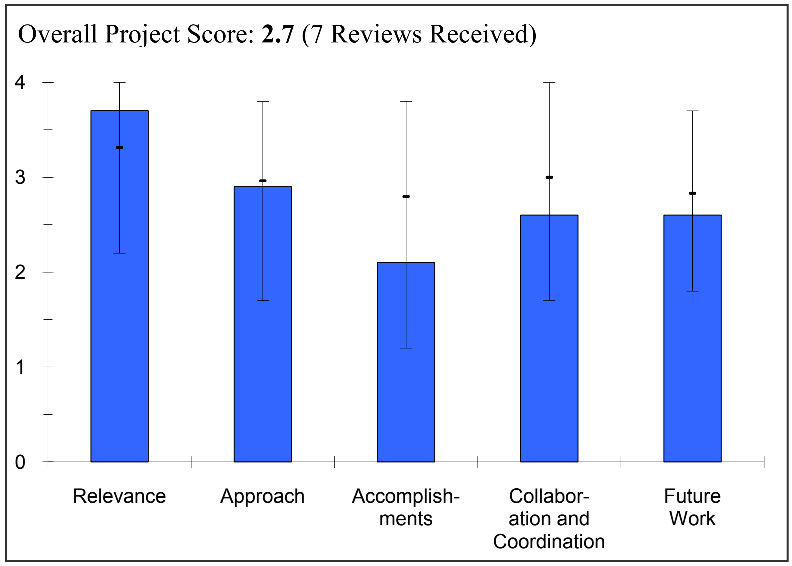
- The reliability of the multi-cell tube bundles should be assessed.

Project # FC-33: New Polyelectrolyte Materials for High Temperature Fuel Cells

John Kerr; Lawrence Berkeley National Laboratory

Brief Summary of Project

The project objectives are to: 1) investigate the feasibility of polyelectrolyte proton conductors that do not require additional water to achieve practical conductivities; 2) determine stability of these materials to oxidation and strong acids; and 3) fabricate and test membrane electrode assemblies (MEAs) to determine gas crossover.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- The project is relevant to the requirements for high temperature membrane development and it meets overall DOE objectives. The goals and objectives of the project are designed satisfactorily. The multi-year plan is in line with DOE RD&D objectives. The study of the model compounds and modeling work pursued by the team is aligned with the development of proton transport knowledge in the low relative humidity (RH) medium. The team should consider *in situ* testing of the material properties at low RH.
- Development of a membrane that is highly conductive and stable at 120°C would enable significant fuel cell system simplification and cost savings.
- The project is geared towards developing membranes and MEAs for hotter and drier operation of fuel cells.
- This certainly is a very relevant project and will help the design of simpler fuel cell systems for practical purposes. Being able to operate at a higher temperature with no or low humidification, while maintaining high performance (requires higher membrane conductivity) and efficiency, are key requirements.
  The project is aligned with DOE goals and targets. Recent change to "feasibility of polyelectrolyte proton conductors that do not require additional water" from "conductors that do not require water" aligns it more with practical systems.
- Improved membranes with greater operating range, higher performance and better durability are critical to the Fuel Cell Technologies Program. Membranes for efficient fuel cell operation without external humidification at operating temperatures between -40°C and 120°C will greatly benefit both automotive and stationary fuel cells.
- Although nearly all membrane projects have conceded the need for humidification to conduct protons, this project is one of two that - in theory - could still fabricate ionomers that do not require water. In that respect, the project is particularly relevant to the stated desire to simplify the balance-of-plant (BOP) system.
- The targets mentioned for the project, such as conductivity, gas crossover, and durability, are aligned well with the objectives of the DOE program.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- The project has been designed appropriately to understand the proton transport barriers under low RH. The approach of the project is good and its outcome is expected to address some of the key technical barriers. The technical feasibility of the synthetic route to an oxidatively stable ionometric polymer is doubtful. The oxidative stability of the new polymer should have been considered. The grafted imidazole polymer possesses benzylic hydrogen, which may affect the durability of the membrane. The team is studying polystyrene-based model compounds to understand the proton transport mechanism, but the instability of polystyrene under low RH fuel cell conditions may not give the team an opportunity to test such materials in real-life fuel cell conditions. From slide 5, it is not clear whether 3M perfluorosulfonic acid (PFSA), which degrades similar to heterocyclic polymer-containing materials, contains peroxide inhibitor or not.

- It is still unclear if this approach is feasible. The base ionic liquids (IL) do not meet the DOE conductivity targets, so why would one expect a less mobile tethered imidazole to be any better. I do not see how the modeling work is helping to make a membrane that meets the DOE targets. The electrode work distracts from required membrane focus.
- There are good strategies to make new functionalized polymers via either modification or new monomers. There is also nice complimentary work going on in electrode development. It all seems a little disorganized though, as there is no clear path to achieving conductivity of 0.1 S/cm.
- It is a difficult project. But the PI was successful in narrowing down the choices for the backbone to polystyrene, polysulfone and PFSA. Now, these materials perhaps can be attached with imidazoles, blended with acid materials and then fabricated as membranes and MEAs. If these membranes achieve conductivity goals, even with some humidification (<25%), it would be a great accomplishment!
- Good idea and sound experimental approach. Very nebulous Go/No-go decision criteria - "How close to 0.1 S/cm at 0 - 25% RH and the full range of temperature (-40-120˚C) is possible without free solvents?" It is unclear what would constitute a No-go when judging the criteria of this approach. There needs to be more specific Go/No-go criteria.
- Discontinue electrode work and focus on membrane work.
- Goal of achieving 0.1 S/cm at 120°C and 25% RH is appropriate. Three backbones, liquid ionomers and blends cover a large area of investigation, commensurate with the funding level.
- The approach of tethering strong acid / weak base groups to a polymer backbone is appropriate. Given high conductivities observed for ionic liquids, a study of whether tethered analogues of these species could provide for a conductive membrane was needed.
- The project approach has addressed the need to understand the conductivity limits of the materials.
- In later stages, the focus upon conductivity limits has driven the project towards understanding the connectivity of conductive routes and has involved a modeling effort. These directions are more appropriate than continued fuel cell testing and electrode fabrication.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.1** based on accomplishments.

- Good progress has been made by the team towards the understanding of proton transport mechanisms in fully humidified and dry membrane conditions. The synthesis of various block co-polymer-based morphologies is commendable. The ionic liquid model work in slide 10 is very interesting. However, in slide 12 it would be nice to compare the conductivity of polymer blends and free solvents with same host PFSA polymer. Comparison of 1100 equivalent weight (EW) Nafion® PFSA (effective EW 2000) with 800 EW 3M PFSA with imidazole alone does not show the true difference between the effects of polymer blend and free solvents.
- The one encouraging result is the improved conductivity with λ of BPSH blended with PAES-COOH-Im, but the conductivities are still below targets and measurements were not done as a function of RH. The other materials tested showed poor conductivities. The other measurements (NMR, TGA, small-angle neutron scattering (SANS), DMA) have not proved helpful to understand conductivity results and trends.
- There were new polymer systems investigated, but conductivity was too low. Small-angle x-ray scattering/ SANS work and modeling is on-going, and the PIs have developed an impressive new electrode system.
- A lot of good work was done for this project. But the fact remains, we are still a long ways away and perhaps have identified the bare essentials to achieve membrane conductivity: the presence of mobile solvents, the high degree of connectivity through membrane (difficult to achieve and maintain), or the presence of water.
- From a system integrator point of view, water management for an automotive power plant with a minimum operating temperature of -40˚C is almost a No-go situation. Even though it is suggested that <25% RH may improve conductivity significantly, I remain concerned due to the practical implications of this approach.
- A good amount of work has been done, but the results do not look promising. Conductivities have been low and well short of targets.
- There has been significant activity with some progress towards the program goals.
- Progress needs to be accelerated for the project to meet all program goals within the remaining time.

- The progress has been slow in getting this project to explore fundamental limits of conductivity and then tethering ionic liquid-derived groups to less exotic backbones before progressing to more exotic chemistries. The 3M 850 mix with imidazole was a step in the right direction, but a tethered version has not yet been reported.
- At the moment, polysulfones appear to be the preferred class of backbones for the polymer (given low conductivity robustness with PSS-PMB), but the rationale is not entirely clear. The project still appears to be searching for a tethered sample that achieves reasonable conductivity, and has been characterized for things such as swelling, crossover, durability, etc.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- The team has very good collaborations with many different research groups with solid understandings of the field. The team also has access to good polymer characterization analytical tools to obtain relevant information in support of the research work.
- Electrode work at LANL is not well aligned to the rest of the project. It's not clear what 3M is doing. Most work seems to be done at LBNL.
- The collaborations with university, National Laboratory and MEA manufacturer need more input from the automotive industry and require input from the catalyst manufacturer.
- Appears to be good collaboration with LBNL and LANL, with topics ranging from various characterizations to polymer synthesis.
- Collaborations within the project are good.
- There is good interaction between partners and great access to analytical facilities.
- The collaboration appears to have improved over the past year, but could be improved more. Modeling calculations with Utah have been a step in the right direction, although this is still a collaboration within the primary contractor organization (LBNL).
- Collaboration could have been improved with 3M to tether imidazole or benzimidazole groups to PFSA materials. This would be a good baseline to serve as a link between the PFSA/IL mixes and the tethered materials from hydrocarbon backbones.
- SANS at Oak Ridge National Laboratory appears to be well used for morphology studies.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- All the proposed work is relevant to the overall progress. However, there is no proposal to carry out high temperature, low RH fuel cell testing of developed materials to demonstrate materials property under DOE targets (0.1 S/cm at operating temperatures of ≤120˚C). The team should consider testing the materials under DOE conditions to show the success of the project.
- The fundamental question of whether the approach is feasible to enable high temperature operation is not being directly addressed. I would recommend focusing on imidazoles attached to PFSA (work with 3M on this) and measure conductivity vs. RH at various temperatures. Fenton's testing is not recommended. The PIs should test hydrolytic stability in boiling water first. I am unclear on the value of the MD calculations.
- It was good to see that the team will pursue the prescribed conductivity goals and it was good to see that more work will be done on the new electrode structures. I am not convinced that the IL work should be continued.
- Proposed future work should help achieve these modified objectives.
- I am not sure what benefit working to tether imidazoles to available materials – polysulfones, polystyrenes, PFSA and perfluorocarboxylic acid – will provide when mixtures with untethered imidazoles did not reach the appropriate conductivity. The untethered materials should provide optimum acid-base interactions, while the tethered materials may not.
- The proposed future work strikes a balance between selecting the most promising approaches and advancing to cell and durability testing.

- The future work has improved in the past year. Electrode studies have been given a much lower priority in favor of looking at conductivity / morphology relationships, which is entirely appropriate for a project that has been struggling to find good conductivity with a tethered, IL-derived material.
- While a fundamental approach may be good for understanding conductivity, it may be preferred to simply use the remaining time to address durability with prescribed accelerated stress tests, as opposed to Fenton chemistry studies and model compound studies.

Strengths and weaknesses

Strengths

- The team has researchers with a thorough understanding of the challenges in meeting DOE low RH objectives and who are capable of providing some meaningful insight into the problem. The team has sound coordination between different research groups. The team has access to good analytical and modeling facilities, which is necessary to carry out such a complex research problem.
- If goals are met, it would be extremely valuable for fuel cell development.
- The PIs displayed excellent electrode work.
- The PI's ability to explore limits of various materials to achieve high conductivity was a strength. The research work was thorough and methodical.
- The basic science work done in the project is good and provides good data to the community to help understand proton conduction.
- The PI investigates a wide range of novel polymer approaches.
- The investigation of the proton conductivity mechanisms was a highlight.
- The initial approach of using imidazole-based ionic liquid-derived materials is one of the strengths of this project. The project does not overlap directly with any of the other projects in the portfolio and it occupies research space that is very much of interest.
- The project has shown willingness to address fundamental concerns of how conductivity is influenced by morphology and the repulsion of water.
- The addition of modeling should enable the project to at least report some addition to the community's knowledge of these materials, even if a finished material is not delivered.

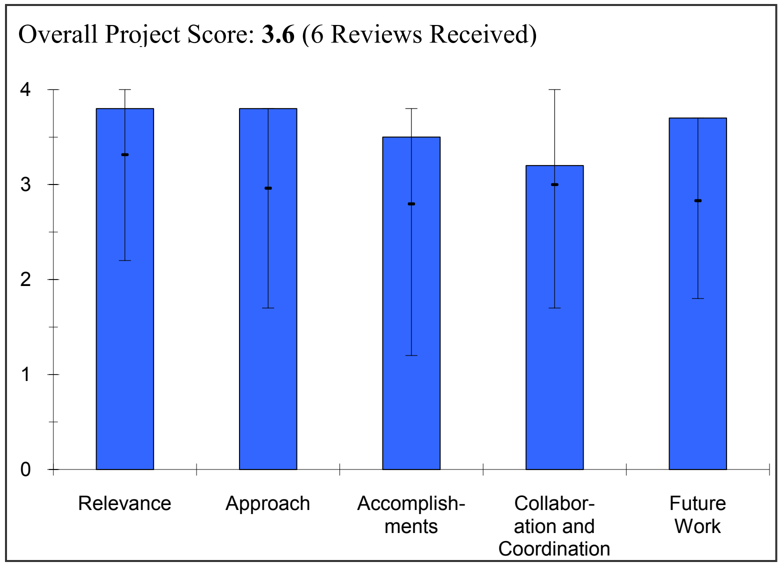
Weaknesses

- This project lacks any plan of testing the materials under realistic fuel cell conditions to demonstrate the progress against DOE targets. The proposed plan of testing MEAs at 30°C to determine whether the membrane has practical conductivity of 0.1 S/cm falls severely short of the DOE target of 120°C. The team should test the materials under DOE's proposed conditions to determine their success.
- The fundamental hypothesis of whether the approach is feasible to enable high temperature operation has still not been proven, after 3.5 years. Poor collaborations were used. Electrode studies and mechanical/morphological testing have not provided significant guidance to improve materials.
- Ionic liquids were used.
- The chosen platform or technology may not have the highest probability to achieve the ultimate goal of a high temperature PEM that requires no humidification.
- The go/no-go criteria are nebulous, which allows work to continue on materials with poor conductivity. They plan to proceed with MEA and stability testing prior to achieving needed conductivity.
- High membrane conductivities and MEA performance have yet to be demonstrated.
- The overall experimental progress of the project is slow. Despite attempts, the project has not really been able to establish a decent baseline for a proton-conducting, ionic liquid-derived, tethered ionomer.
- Some suggestions that have come to the project from outside sources; e.g., the FreedomCAR (Cooperative Automotive Research) and Fuel Partnership's Fuel Cell Technical Team (FCTT)'s recommendation to try tethering to a PFSA, have only recently been implemented and there is not enough progress to report.
- The slow progress has prevented the project from being able to deliver classes of data for which other projects are fairly judged including data for swelling, durability, gas crossover, etc.

Specific recommendations and additions or deletions to the work scope

- A solid Go/No-go gate should be used before any MEA development work is done. The membrane needs to meet some hard conductivity target (I suggest they use the interim target the other set of membrane projects needed to pass, 0.1 S/cm or greater at 120˚C and 50%RH) before any MEA development work is done with it, and I also would suggest before any oxidative stability testing is done.
- Produce a conductivity curve over the entire RH range - this would help in comparing to other materials.
- Focus on imidazoles attached to perfluorosulfonic acid and measure conductivity vs. RH at various temperatures. Eliminate electrode work. Eliminate Fenton's testing and instead test for stability in liquid water. Focus modeling and other characterization work on a systematic series of materials where single variables are controlled independently, so the synthesis guidelines can be developed to improve membrane conductivity.
- Ionic liquids should be included.
- If the project had not added a modeling component to study the limits of conductivity, one would be suggested here. Some focus should remain on how modeling-suggested morphologies could be experimentally realized.
- There should be better rationale provided for why particular backbones or block copolymers are being used. It is too late in the project for approaches that have not been shown to add some morphological / conductivity advantage.
- It would be interesting to see if the water repulsion could be used to a greater advantage. It could possibly lower EW materials.

Project # FC-34: Membranes and MEAs for Dry, Hot Operating Conditions
Steven Hamrock; 3M



Brief Summary of Project

The overall project objective is to develop a new proton exchange membrane (PEM) with higher proton conductivity and improved durability under hotter and dryer conditions compared to current membranes. The approach includes 1) testing of new polymers, new membrane additives, proton transport studies, and new membrane fabrication methods, and 2) performance and durability testing.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Development of robust, high temperature, low relative humidity (RH) membranes is vital for the successful commercialization of fuel cells (FCs), especially for vehicle applications as high temperature and low RH allows for a simplification (cheaper) balance of plant (BOP).
- Membrane performance and durability directly affect DOE's objectives and technical targets.
- Polymer membranes with greater operating range, higher performance, improved durability, and lower cost are critical to the FC Program.
- The project is highly relevant to the DOE objectives. The project tasks are a nice mix of theory, polymer synthesis, and membrane characterization work.
- The project is highly relevant to DOE. Development of membranes with high conductivity under hot and dry conditions is directly applicable to DOE membrane targets. Furthermore, through addressing mechanical and chemical stability, the project retains relevance higher than that of several other membrane projects by increasing the probability that new membrane materials developed will be suitable for FC applications.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- Slide 13 of Hamrock's presentation clearly shows all of the strategies one could implement in order to improve the mechanical robustness of extremely low equivalent weight (EW), short side chain perfluorosulfonic acid (PFSA) materials.
- The introduction of multi-acidic side chains is a very interesting strategy and the ongoing work on heteropoly acids (HPAs) from Colorado School of Mines (CSM) continues to show promise.
- The approach is systematic, based on sound chemistry and parametric consideration of important variables.
- Appropriate *ex situ* analyses are used.
- Experimental data are used to establish necessary changes to membrane electrode assembly (MEA) materials and structures.
- Focusing on materials that are scalable is a good approach.
- The use of additives is questionable for long term operation.
- The approach covers a wide range of candidate materials, experimental and theoretical studies of the proton transport mechanisms, as well as membrane fabrication and evaluation in conductivity cells and FCs.
- The 3M approach of examining polymer blending, crosslinking, reinforcement, and polymer modifications is comprehensive and, in some respects, innovative. For the most part, the members of the research team have

complementary talents that mesh together nicely. 3M seems to have done a good job in overseeing the various project tasks.

- The competitive kinetics and polymer degradation work do not seem to fit with the more important focus of finding new FC membranes with improved performance at high temperatures and low RH.
- The project is well focused on attacking the key technical barrier of achieving high conductivity under hot and dry conditions. The PI has selected several promising routes toward achieving this goal, and has conducted R&D activities in a well-planned, pragmatic manner. Some of the routes investigated; i.e., inorganic HPAs, seem less promising, and should be lower priority, but given that the project continues to make good progress overall, this is not seen as a significant demerit.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- Synthetic progress on perfluoroimide acid (PFIA) materials is commendable and the results thus far are promising. Based on the EW values of the PFIA materials, conductivity is as expected, but FC polarization curves are lower—MEA optimization is still needed.
- HPA conductivity values are off of the chart, and it will be interesting to see if this technology will translate to a water insoluble, mechanically robust FC MEA.
- 3M has demonstrated a stable MEA for 18,000 hours under voltage and humidity cycling conditions.
- Alternative electrode structures and materials have been successfully matched with new membrane materials.
- It is good to see that the conductivity of the membranes has been conducted using the BekkTech apparatus that allows a standard method and setup to evaluate whether a material has reached DOE goals. The positive results in these tests as a function of EW show the significant improvements made in the new materials compared to Nafion®. (The effect of EW on conductivity though is an obvious result well known in the literature, as is the loss of crystallinity with the lowering of EW.)
- Conductivity exceeding the program target was shown with novel perfluoroimide multi-acid side-chain (MASC) polymers.
- Based on the FC durability test data, combining higher conductivity with high durability appears promising.
- There have been a number of significant accomplishments by the research team. The MASC polymers are impressive, as are the results from the PFIA work. The experimental work (whether it involves polymer synthesis, membrane characterization, etc.) appears to have been carried out carefully with attention to detail.
- The progress of this project continues to be very good. The development of the PFIA polymer is a promising step toward developing a membrane that meets DOE resistance targets. The materials appear to currently meet DOE high temperature, areal resistance targets at the high end of the humidity range (120°C, 80 kPa water), but it doesn't appear to be there yet in terms of meeting the target at the drier end of the humidity range. Given that 3M is still planning to investigate lower EW materials of this family, and given that some approaches; i.e., crosslinking, are still available to allow development of stable materials at even lower EW, 3M appears to be making good progress and has a high likelihood of soon meeting all resistance targets.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Collaborations are clearly stated and shown. The program with CSM has been ongoing for quite some time; however, I wonder how integrated the programs of the other collaborators are or if are they more of an afterthought.
- The team includes an automobile original equipment manufacturer (OEM) and several universities.
- Some integration of the collaborators' work is needed to achieve the project goals.
- Some of the collaborative work (the competitive kinetics and polymer degradation work) is not well coordinated with the main thrust of this research project, that being the identification of new membranes that operate at high temperature and low RH.
- Given the large suite of capabilities present at 3M, this project requires a lesser degree of collaboration compared to some of the other membrane projects. Still, it is not clear that 3M is getting much useful help from subcontractors, and I have to wonder if other collaborations could be more fruitful. Collaboration with Case

Western Reserve University (CWRU) and the University of Delaware at Mercy has produced some interesting results, though the degree to which these have guided or influenced polymer development at 3M is unclear. The collaboration with GM is good. The collaboration with CSM is again interesting, but unclear that investigation of this pathway (inorganic HPAs) is necessary or appropriate given how strong progress is toward achievement of targets with the lower risk approaches being pursued at 3M with more conventional organic polymers.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.7** for proposed future work.

- Future work encompasses the further optimization of PFIA and its subsequent incorporation into a MEA for FC testing. At this time, there is not more that can be inputted.
- The project ends in less than a year.
- Continued chemical and mechanical evaluation or characterization and enhancements of MEA materials will continue. More information on how to stabilize low EW materials is needed.
- The downselected membrane(s) will be durability tested under cycling conditions.
- It is not clear from slide 32 what vehicle, cell or stack, will be used in the final testing.
- The results so far are good, but the weaknesses are obvious in that low EW results in betterconductivity but poorer durability. The next steps of "blends, cross-linking, reinforcement, and polymer modifications" are critical and will determine the success of the project.
- A logical plan to continue and complete the tasks is in progress.
- The research team has properly downselected the MASC and PFIA polymers. Future work is correctly focused on both membranes and MEAs.
- The future work presents a reasonable and appropriate pathway to further advancement toward DOE targets and incorporation of novel 3M materials in MEAs.

Strengths and weaknesses

Strengths
- There is very strong project management with the ability of scale up and commercialization. The team has a very strong grasp of polymer synthesis, casting and scale-up. The team is excellent.
- They have a good approach, scalable materials, and good benchmarking tests for conductivity.
- A number of approaches to improving performance and durability have been successfully combined (MASC and additives).
- Building on proven PFSA chemistry has allowed improved conductivity and durability while taking advantage of PFSA's proven track record and manufacturability.
- The team has accomplished impressive research, especially in regards to the MASC and PFIA work.
- A wide range of careful and precise experiments have been carried out.
- The new polymer synthesis work is well thought out.
- 3M has lots of good ideas for attacking membrane resistance targets, using athorough and well-structured approach to investigating the various ideas. The ability of the participants to vigorously attack the conductivity issue while still keeping an eye on mechanical and chemical stability is an important strength.

Weaknesses
- Collaborations with other institutions besides CSM do not seem as strong or vibrant.
- There needs to be some consideration or discussion of potential cost.
- The results so far are good, but the weaknesses are obvious in that low EW results in better conductivity but poorer durability. The next steps of "blends, cross-linking, reinforcement, and polymer modifications" are critical and will determine the success of the project.
- The cost has not yet been discussed.
- It is not clear why the polymer degradation work is required and how the results from this subproject will impact on the MASC and PFIA work. The same can be said for the competitive kinetics work.
- It is not clear why it is necessary to add HPAs to a membrane. The chemical stabilization effect of inorganic oxide/HPA seems to be important. I am not convinced that the additives are needed for enhancing proton

conductivity. With a 50% loss in the additive upon membrane soaking, I am not sure if significant manpower should be devoted to finding a way to improve additive retention.
- Collaborations don't seem to be contributing all that much. Although the number of ideas generated by the project is seen as a plus, narrowing down these ideas and greater degree of downselection of the most promising ones could serve to accelerate progress.

Specific recommendations and additions or deletions to the work scope

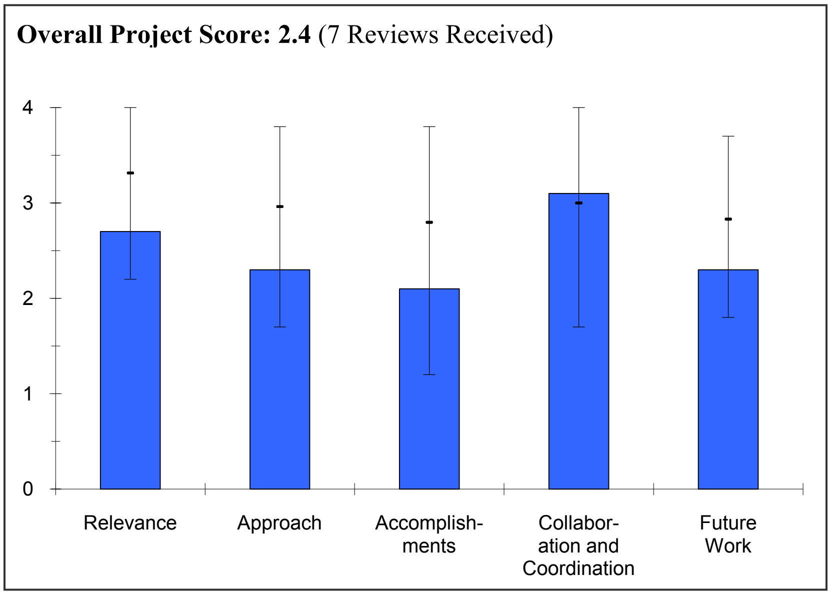
- The project is going as planned with good progress. It is difficult for this reviewer to add to a very well thought out program which is achieving its goals.
- De-emphasize the model compound polymer degradation work and the competitive kinetics work.
- The inorganic HPA work is less significant and less promising, in terms of potential for application in real FC materials, than the MASC/PFIA approaches.

Project # FC-35: Lead Research and Development Activity for DOE's High Temperature, Low Relative Humidity Membrane Program

James Fenton; University of Central Florida

Brief Summary of Project

Project objectives are to: 1) fabricate membrane electrode assemblies (MEAs) from team membranes; 2) test team MEAs for fuel cell performance; 3) standardize methodologies for in-plane and through-plane membrane conductivity measurements; 4) provide High Temperature Membrane Working Group (HTMWG) members with standardized tests and methodologies, and 5) organize HTMWG bi-annual meetings.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- The relevance of this project is clear on principle only for a limited number of DOE research, development and demonstration (RD&D) program objectives. It strives to provide a standardized MEA fabrication and testing service for membrane fabricators as part of the HTMWG, with primary output being membrane properties. Currently automotive fuel cell original equipment manufacturers are shying away from needing 120°C stable membranes, so the original tasking of the HTMWG may not be so relevant any longer.
- Coordination of DOE funded membrane development efforts enables common platforms and methods for materials evaluation and comparison. Focusing on MEA development and performance testing does not provide much value.
- This project is a relevant activity to commercialize fuel cell technology for real world applications.
- The project is aligned with DOE goals and targets. Independent testing of high temperature membranes provides needed data to DOE for evaluation of other membrane projects.
- The development of standardized testing methods for conductivity and fabrication and testing of partner MEAs are needed to compare the different membrane technologies.
- This project is of marginal relevance to the Hydrogen Program. Development of membranes capable of operation under hot and dry conditions is highly relevant, but at this stage, further work on membrane development should be the priority. Optimizing MEAs based on recently developed membranes, most of which still require significant additional R&D effort, is of lesser significance at this stage than R&D on the membranes themselves. The fact that the project did not report MEA testing results, but only sent them back to the teams (who may or may not choose to share them), further diminishes the relevance of this project.
- The DOE membrane projects need to measure conductivity, *in situ* performance, and durability of their samples to understand whether or not progress is being made. This project is assigned to do just that. In this sense, this project is perfectly aligned with DOE objectives.
- The relevance of this project is also helped by the need for consistently measured data between projects. Conductivity data have been shown in the past to be sensitive to the exact protocol and equipment being used. This project helps to make measurements common among developers.

Question 2: Approach to performing the research and development

This project was rated **2.3** on its approach.

- The approach may succeed in providing data that could contribute to overcoming a DOE barrier, but there are significant weaknesses that will challenge the concept. The electrode characteristics, which this project is responsible for making, will have some influence on the resulting membrane performance properties. This is difficult to avoid in any approach, unless all the MEA components are optimized for one another. So, it is entirely possible that their specific electrode and method of application may bias one membrane characteristic over another and, therefore, not allow a level playing field when comparing different membrane suppliers. The electrodes have a strong impact on apparent membrane performance and durability, so conclusions from their approach will be limited to their specific catalyst and electrode fabrication methodology.
- There is too much focus on optimizing MEAs for each project. The focus should be on measuring membrane targets as defined in DOE target table: area specific resistance, chemical and mechanical durability, crossover and shorting resistance. The Pt vs Pt/Co electrode study is not relevant for the membrane project and it has been done already. Chemical degradation studies were not done using DOE recommended accelerated stress tests (ASTs)—alternating current voltage, 90˚C, and 50% relative humidity (RH). The study of the type and amount of ionomer in electrode is also not critical for the membrane program. The reason for adding carbons in electrodes is unclear.
- The scope of the project appeared unnecessarily broad and risky. Assuming the primary objective is to evaluate various membranes in some standard, common MEA design—the approach should be to adopt a low risk, repeatable path to make MEAs. The winning membranes will require specific optimization before commercialization, which will be done by an MEA manufacturer who will optimize around all operational, durability, and cost constraints anyway. An attempt was made to improve MEA performance using a Pt-Co catalyst. There are known issues with this type of a catalyst, which can ultimately cloud the primary objective of evaluating a new membrane performance in an MEA.
- Independent testing and measurements of conductivity as a function of RH (both in plane and thru plane), and fabrication of MEAs using new membranes, provides needed data to DOE on the status of membrane development work.
- The policy to release data only to membrane providers and not show it at this review makes it difficult to determine how much work they've done. Showing the results without attributing them to anyone could be better.
- The flow chart for MEA testing is reasonable and test results showing optimization for 3M membrane vs. Nafion® suggest they are on the right track. The point they showed for optimization of ionomer loading (400 mA/cm^2 at 80°C, and 100% RH) is probably not the point of interest for these materials. The membrane projects are designed for high temperatures and low RH; and they should probably compare ionomer preparations at conditions of high temperatures and low RH, and probably high current density if you are going to pick one point but they really should make comparisons at a variety of conditions.
- The MEA testing is to be done at several conditions.
- The program structure is fairly well defined, feasible, and aligned with the technical barriers.
- Some areas, such as examining fluoride emission rate and the effect of Pt-Co vs. Pt, are outside the scope of the project. These efforts are of little value and distract from what should be the immediate focus: developing optimized MEAs based on membranes supplied by the other teams.
- The project has been able to deliver a common source for membrane conductivity results to the different teams that are funded by DOE.
- The project has focused on optimizing ionomer loadings in electrodes, as well other material and processing parameters that would improve MEA performance. However, the objective here is to obtain membrane information, which can be done with or without optimized electrodes. Gas crossover and high frequency resistance (HFR) measurements can provide valuable information, as well as the time or number of cycles until high gas crossover in accelerated stress tests.
- The use of PtCo is not necessary since it introduces additional failure modes. Furthermore, it may help to mask the failure modes that need to be observed from the membranes, as evidenced by the lower fluorine emission rate with PtCo.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.1** based on accomplishments.

- For one year of work, the number of catalyst coated membranes (CCMs) fabricated and tested is quite small at 29, representing about two per month. This is not a high enough testing rate for the number of different membranes to obtain statistically significant results or even begin to explore the number of independent MEA component parameters and combinations that can significantly affect their membrane performance measurements.
- It is not clear from the presentation what progress was made towards any of the DOE membrane property targets.
- After four years of activity, it would be good to see the overall rate of progress of the project towards the DOE objectives and barriers.
- The University of Central Florida (UCF) has made a lot of MEAs, but the materials and preparation methods vary significantly for the various projects. It would be useful to see a comparison of all the HTMWG team's membranes compared in a common MEA design.
- There is still no through-plane resistance data or defined procedures. ASTs for chemical and mechanical durability are not done.
- Almost every aspect of MEA fabrication was explored.
- It is difficult to determine how much work they've done in MEA testing since data for membrane providers isn't shared.
- The work on the specific area resistance measurement protocol doesn't appear to have progressed. It was not made clear why work on the Pt/Co and 3M ionomers will help the performance of the collaborators membranes, some of which contain novel hydrocarbon-based materials.
- It's hard to evaluate the accomplishments of this project when the PI diverts all MEA testing results back to the other teams. Of course, the data should be shared with the teams first, but the point of this project is (or should be) to optimize MEAs and compare them. In addition to the other work presented, the presentation should have a slide showing results from MEA testing with FuelCell Energy materials, a slide with results from Case Western Reserve University (CWRU) materials, etc., and then a table comparing results from each project.
- Results from the comparison of Pt vs. Pt/Co, and comparison of different types of electrodes on commercially available membranes, are not relevant to the barriers at hand, and, for the purpose of this review, are not considered as progress.
- Although results were not provided in the presentation, the presentation indicates that at least some MEA testing using membranes supplied by the teams has been performed, indicating that some progress may have occurred.
- Given the project approach, the project did manage to demonstrate improved iR-free performance with 7 psig backpressures, NR 211, and < 0.5 $mg_{Pt}/cm^{\wedge 2}$ Pt/C. Although the project should be directed to focus on membrane characteristics, it managed to deliver some results for what it was trying to do.
- The progress on membrane through-plane conductivity was alluded to in some slides, but was not explicitly reported.
- The full cycle of testing has not yet been completed for any given collaborator.
- Attention has been paid to optimizing electrodes to suit different membranes, but apples-to-apples comparisons with common electrodes are valuable as well.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- The nature of this project relies on collaboration with membrane suppliers and they have a good number. It is not clear to this reviewer, however, why, after four years, they are still in the "exchange e-mail state" with some suppliers, or preliminary discussions of what will be tested.
- UCF is working well with all the membrane developers to evaluate their membranes. UCF is also working with Bekktech and Scribner on conductivity measurements, although no results from Scribenr have been presented.
- There appears to be good collaboration among all partners and stakeholders. For apples-to apples-comparisons, all partners should provide membranes, which then should be made into MEAs using the same electrodes and fabrication processes. This reviewer is not sure why Arizona State University will provide MEA in hardware.
- There is good collaboration with groups providing membranes for testing. There is good collaboration with 3M.

- This is a good group of partners and project collaborators. Degree of coordination with most of the collaborators appears adequate.
- The collaboration is reasonable. Clearly, the main collaboration here is that between the Florida Solar Energy Center (FSEC) and the membrane development teams. The fact that negotiations on testing procedures are still ongoing with some teams suggests that the degree of cooperation could be improved.
- The project relies considerably upon collaborative interactions, but the progress reported reveals that these interactions have been difficult. Nearly all the progress reported this year focuses on electrode optimization carried out at UCF-FSEC, and not on testing of collaborator materials. The slides reveal that collaborators appear unwilling to share samples, or that they wish to change electrode parameters before doing so, which defeats the purpose of obtaining consistent *in situ* results.
- Some collaboration was evidenced with the use of a 3M ionomer for electrode optimization.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- The proposed future work appears to be more of the same *modus operandi*.
- No specific plan was presented of how the results of the testing will feed back to the membrane suppliers to make further improvements, followed by subsequent further characterization to make progress towards overcoming the barriers.
- Progress may also be constrained by the lack of clear guidance on the area specific resistance measurements.
- There was too much focus on MEA optimization. This is a membrane project. Pick a common, stable, MEA platform to use for all membranes and do the testing. Focus on through-plane resistance and chemical and mechanical durability.
- The proposed future work should include endurance testing. Other proposed tasks appear consistent with the goal of the project.
- MEA testing is to be done at several conditions, including high temperature (120°C) and low RH.
- Area-specific resistance (ASR) should be measured for all contributors' membranes.
- The project is supposed to end in ~one year.
- Recommend that the collaboration with 3M on the electrode development and characterization should be de-emphasized. Focus should be to develop standardized test methods and to work with the university to test their membranes using standard, rather than state-of-the-art (SOA), materials.
- The proposed future work on membrane characterization and MEA development and testing is good. The discussion of through-plane vs. in-plane characterization is irrelevant. Through-plane is the only thing that matters for fuel cells.
- Continued electrode optimization does not appear to lend itself to the membrane comparisons that this project should deliver.
- Identifying ASR as a "controversy" reveals that the project intends to argue with revised DOE targets. The use of ASR is intended to encapsulate both proton conductivity (a material property) and the ability of the individual projects to fabricate thinner membranes. If either conductivity is low or if the membrane cannot be made thin, then ASR will be high, which is a much more relevant result for research customers than just proton conductivity alone.

Strengths and weaknesses

Strengths
- This has the potential to compare multiple membrane types within one electrode platform and testing protocol. It can provide testing capabilities to some suppliers that may be new to the area without access to their own testing.
- Strong collaboration with most membrane projects.
- Building consensus among all partners.
- Collaborations with membrane providers are strong.
- There is a good group of partners and collaborators on this project.

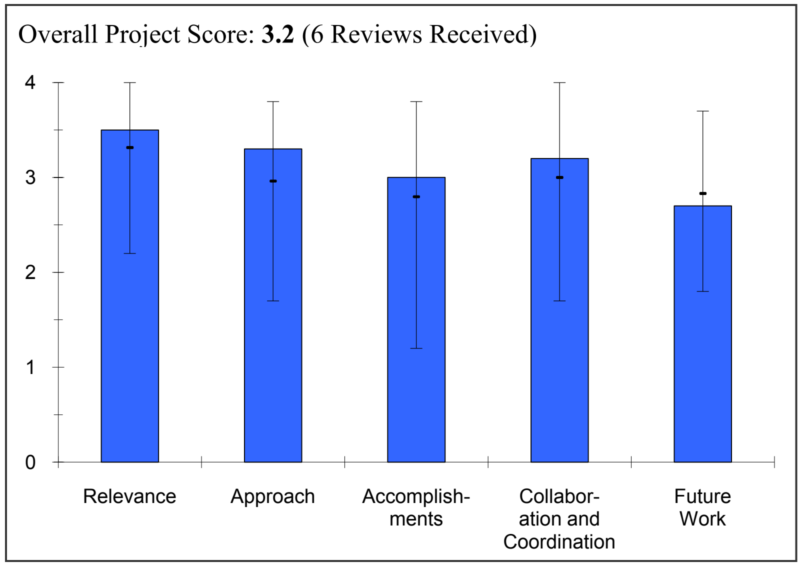
- A number of exciting materials have been developed by the membrane teams, so this project has some good (albeit, mostly still unoptimized) materials to work with.
- The project benefits from its approach to produce apples-to-apples comparative data for membrane samples.
- The project has a proven history of providing in-plane proton conductivity data.
- Although the need is questionable, the project has proven itself capable of optimizing Pt/C electrodes.

Weaknesses
- It is complicated to work with many different suppliers on a confidential basis.
- The rate of progress appears slow due to the lack of charting progress towards the barrier targets. It would be useful if the various team members who report results in their own projects would allow their reported progress to be compiled to make overall charts of progress towards a specific target.
- Trying to develop and apply their own electrode is limiting their ability to provide a SOA MEA component that is critical to the membrane operation.
- It has lost focus on measuring the membrane properties and comparing to the targets, as defined by DOE. Much of the electrode work has already been done by others.
- They need to keep the project scope simple and focused.
- The goals of the PI mostly do not seem to align with the needs of DOE. The project has been wasting time and resources on studying things like Pt vs. Pt/Co, when it should be focusing on developing electrode structures optimized for each different membrane. Let the catalyst projects look at Pt vs. Pt/Co.
- The project has allowed itself to lose focus on providing membrane characteristics and has detoured into electrode development using Pt alloys, varied ionomer loadings, use of collaborator ionomers, and other activities that take away from learning about membrane performance and durability.
- The project needs to find a way to make delivery of membranes for testing more facile and to make established protocols for MEA assembly and testing more acceptable to collaborators.

Specific recommendations and additions or deletions to the work scope

- They should discontinue trying to optimize electrodes on their own. Consider going outside to a well-established SOA fuel cell laboratory in industry for their best electrode knowledge, and adopt as much of that art as possible. If this is not possible, then focus more on the *ex situ* membrane fundamental properties that can be measured without complications of how the whole MEA was constructed.
- Stop optimizing MEAs for each program.
- Prioritize through-plane resistance and chemical and mechanical durability testing.
- During the questions and answers part of the presentation, certain technical team members indicated that electrode optimization should be eliminated from the scope of the project, and that the same type of electrode should be used on all membranes. This reviewer strongly disagrees with that recommendation. Membrane-electrode interactions are a critical factor in determining MEA performance, and there is no "one-size-fits-all" ionomer-containing electrode. The only reason this project still exists is so that it can work to optimize performance of novel membranes in fuel cells. Use of the same standard electrode would negate this project's *raison d'être*.
- Studies of different catalyst materials should be deleted from the project scope. While the optimal catalyst may be different for different types of MEAs, this effect is expected to be smaller than the effect of ionomer type, ionomer loading, processing conditions, and fabrication methods. Furthermore, the project should not be investigating optimized electrodes on commercial membranes, except to the very limited extent that these are needed for base lining. Keep the focus tightly on optimizing electrodes for each novel membrane.
- The following should be removed: the use of PtCo and further optimization of electrodes.
- While certain collaborators may have a preferred electrode composition, a common electrode should be used for at least one series of tests. This electrode may be a gas diffusion electrode. Tests with preferred electrode compositions may be done in comparison with an apples-to-apples baseline, if requested.
- *In situ* tests need to be focused on those diagnostics that provide membrane information: HFR, hydrogen crossover, and electrical insulation measurements. These three diagnostics cover the ideal function of the membrane.

Project # FC-36: Dimensionally Stable Membranes
Cortney Mittelsteadt; Giner Electrochemical Systems, LLC



Brief Summary of Project

The ultimate goal of this project is to meet DOE performance targets with a membrane film that can be generated in a roll at DOE cost targets. Project objectives are to: 1) determine the effect of pore size and substrate thickness and demonstrate polymerization of the perfluorosulfonic acid (PFSA), 2) achieve 0.07 S/cm at 80% relative humidity (RH) at room temperature, 3) demonstrate membrane conductivity greater than 0.1 S/cm at 25% RH at 120°C using non-Nafion® materials, 4) demonstrate ability to generate these materials in quantities suitable for automotive stack, 5) build short stack with optimized materials and demonstrate durability, and 6) demonstrate how these materials can be produced to meet DOE cost targets.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Development of robust, high temperature, low RH membranes is vital for the successful commercialization of fuel cells (FCs), especially for vehicle applications as high temperature / low RH allows a simplified (cheaper) balance of plant (BOP).
- The project is directly relevant and applicable to DOE's objectives.
- The project is relevant to the objectives of DOE's multi-year R&D plan. Initial activities were very much aligned to DOE's goal. Improvement of low RH membrane conductivity is critical to the success of DOE's hydrogen research initiatives.
- This program is relevant to DOE R&D objectives for a more durable conductive membrane.
- Membrane survivability is key to FC program success.
- The project seeks to find new membranes that operate in a high temperature and low RH FC environment. Such membrane materials are important to the DOE Hydrogen Program.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The approach of this project is to develop two key aspects of the membrane: 1) extremely low equivalent weight (EW), chemically stabilized PFSA materials, and 2) a mechanically robust reinforcement layer.
- Giner is addressing the issue of low EW (high conductivity), leading to poor mechanical stability by supporting the ionomer in a strong mechanical structure and by modifying the polymer.
- Difficulty in impregnating the structure with ionomer is an issue.
- The porous structure results in a conductivity penalty.
- The approach of making dimensionally stable membranes with two-dimensional laser-drilled hole supports (2DSM) and dimensionally stable membranes with three-dimensional porous supports (3DSM) is good for meeting the program objectives. Determining the conductivity of triflic acid and benzene sulfonic acid (BSA) is a good approach to understand the feasibility of achieving the DOE low RH membrane target using presently available sulfonic acid based ionomeric membranes. The data in slides 9 and 10 show that it's very convincing

that it's an uphill battle to achieve the DOE target in a practical polymeric membrane, while neat BSA just meets the target. Conversion of BSA in any polymeric form will result in a conductivity penalty.
- The low EW ionomers do meet conductivity and resistance targets. The key to this program is to find a way of making stable membranes. It is not clear if the supports will make these membranes stable enough, but this is the right approach.
- The work keeps cost, performance, and life targets in sight and in balance.
- The PI has correctly noted that FC membranes with next-generation properties (performance) will be composed of low EW polymers, where the membrane polymer is chemically stable and where the membrane is very thin. The PI's approach of embedding an ionomer in a two-dimensional or three-dimensional matrix is not particularly innovative, but it does represent a rational and reasonable approach to making better FC membranes. The key here is to find the right ionomer, i.e., an ionomer with a very high conductivity that is not water soluble, since the inert matrix will effectively dilute the ionomer's ion exchange capacity (IEC) and lower the conductivity.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Technical progress in the two key areas of this project, 1) the ionomer, and 2) the reinforcement layer, is ongoing.
- Unfortunately, Giner's core intellectual property (IP) and unique sales point (USP) is it's 2-DSM material, but they were unable to reduce the cost sufficiently. With that in mind, it still remains unclear what Giner is bringing to this program.
- With the 3-DSM, it is not readily apparent how uniformly the polyelectrolyte is penetrating the reinforcement layer. Furthermore, the inconsistency between the proton conductivity (PC) and polarization (I-V) curves indicate perhaps a thin film of polyelectrolyte on the reinforcement surface, which in turn would affect the proton conductivity results in a positive way (through plane vs. in plane).
- The electrode-membrane interface has been improved, resulting in lower MEA resistivity and higher cell performance.
- Giner has downselected to the 3-DSM approach and stabilized the low EW material.
- Partner Millipore estimates the cost of the porous structure is well within DOE targets.
- The team has made good progress in the development of two- and three-dimensional, stable membranes. The team has used 700 EW membranes and PFSA homo-polymers to develop dimensionally stable membranes. It seems the team's strategy is to lower the EW of the ionomer to achieve low RH and high temperature conductivity. The team should also consider the dissolution behavior of low EW ionomers during humidity cycling. Under fully humidified condition, these low EW ionomers and homo-polymers tend to dissolve and leach out of the reinforcement matrix. So, the team should also include testing of these membranes under automotive humidity cycling conditions. From the data in slides 23 and 24, it seems that the team has hit the conductivity limit that can be achieved by low EW PFSA type material. The team needs to think of an alternative strategy to achieve low RH conductivity, while maintaining the stability of the ionomer under humidity cycling condition. The durability study of 3DSM under electrolyzer conditions would demonstrate the viability of 3DSM membranes for electrolyzer applications.
- Conductivity values are high enough to meet the targets that have been demonstrated. Showing that three-dimensional stabilization can allow membranes to meet the durability targets is critical at this point. A method of determining if the 20,000 humidity cycle target can be met is needed.
- Improvements in cycle life were demonstrated. Significant improvements in combined performance, cost, and life are still needed.
- Technical accomplishments this past year have been good. The PI has realized that his two-dimensional matrix was too expensive for commercialization. The move to examine three-dimensional supports was correct. Unfortunately, the conductivity results for the three-dimensional matrix are not as good as those collected with a two-dimensional support (I believe that the two-dimensional support conductivity results may have been skewed by the presence of an ionomer over layer). The project is now moving toward identifying (synthesizing) new high conductivity polymers to impregnate into the three-dimensional support. The problem here is the water solubility of very low EW polymers.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The project is very well managed, with clear objectives from the various partners—State University New York Environmental Science Forestry (SUNY-ESF) provides low EW PFSA materials, Florida does PC and FC testing, and General Motors (GM) is the voice of the original equipment manufacturer (OEM).
- However, as previously stated, Giner is no longer using their 2-DSM material; therefore, their reliance on collaborators and suppliers increased significantly. Perhaps Giner should be a little more active with their membrane scale-up and potential catalyst-coated membrane (CCM) fabrication.
- The team includes an automobile OEM, universities, and an industrial manufacturer of porous structures.
- The team consists of good, but very small, academic and industrial collaborations. Collaboration with national laboratories may benefit the team. The team should also try to implement some of the testing protocols developed by their industrial partner, GM, to test newly developed 2DSM and 3DSM membranes.
- It is not clear what GM is doing on this project.
- The project team has been collaborating with OEMs and responding to cost and manufacturability concerns raised in prior years.
- Most of the work was carried out at Giner. The low EW polymer synthesis work was performed at SUNY-ESF. Coordination between the partners seemed to be adequate.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Proposed future work covers the key needs of the program; however, it is unclear if enough time or money remains in this project to reach a conclusion. The membrane at this point still requires a lot of optimization, as does the membrane electrode assembly (MEA).
- The project will end within a year.
- Effort on stabilizing the system (with RH cycling) and optimizing the catalyst layer/membrane interface will continue.
- A stack test may not be the best use of remaining scarce resources.
- The proposed plan is sound and fits with the development work. RH cycling and short stack testing is necessary. However, given the difficulty in impregnating the reinforcement matrix with ionomer, a performance reproducibility study using membranes made during different batches should be considered.
- More work should be done on the insoluble 3-DSM. This material looks most promising to meet the targets.
- A better definition of future work plans is needed.
- The success of this project hinges on finding a low EW ionomer to impregnate into the three-dimensional support, where the ionomer is not water soluble and is of sufficiently high conductivity, so that the "effective" conductivity of the composite membrane meets DOE targets. The project PI realizes this point. I am somewhat skeptical that such a polymer will be found (for many years researchers have been searching in vain for such a material).

Strengths and weaknesses

Strengths

- The project management of Giner is a strength.
- The team has access to automobile test protocols and real-world drive protocols at their partner's (GM) test laboratory. The team also has individuals with solid understandings of the field and related challenges in such development work.
- The low EW ionomers do meet conductivity and resistance targets.
- The project builds understanding of low EW material performance and durability, when combined with support structure materials.
- The project PI has a good grasp of the technical challenges that must be overcome next year.
- The PI has performed both physical property characterization experiments and FC tests on his new membrane materials.

Weaknesses

- Unfortunately, Giner's core IP and USP is its 2-DSM material, but they were unable to reduce the cost sufficiently. With that in mind, it remains unclear what Giner is bringing to this program. Perhaps Giner should be a little more active with their membrane scale-up and potential CCM fabrication.
- The team is exploring the avenue of low EW ionomers, which is a common strategy most of the researchers are pursuing. The team should think of some alternative strategy to circumvent the traditional approach to low RH membrane conductivity.
- It is not clear if the supports will make these membranes stable enough.
- A method of determining if the 20,000 humidity cycle target can be met is needed.
- There needs to be a clearer definition of future work.
- The use of a three-dimensional inert polymer matrix with ionomer impregnation is not new and innovative (the PI's composite films are very much like GoreSelect membranes).
- It is doubtful that a water-insoluble ionomer for impregnation will be found with a very low EW. The use of higher EW polymers will result in a composite membrane conductivity that does not meet the DOE target. In moving from a two-dimensional to a three-dimensional matrix, the PI is confronting the same problem that many FC membrane researchers have faced in the past: anything that one does to a charged polymer to prevent water solubility or excessive swelling produces an unwanted drop in proton conductivity.

Specific recommendations and additions or deletions to the work scope

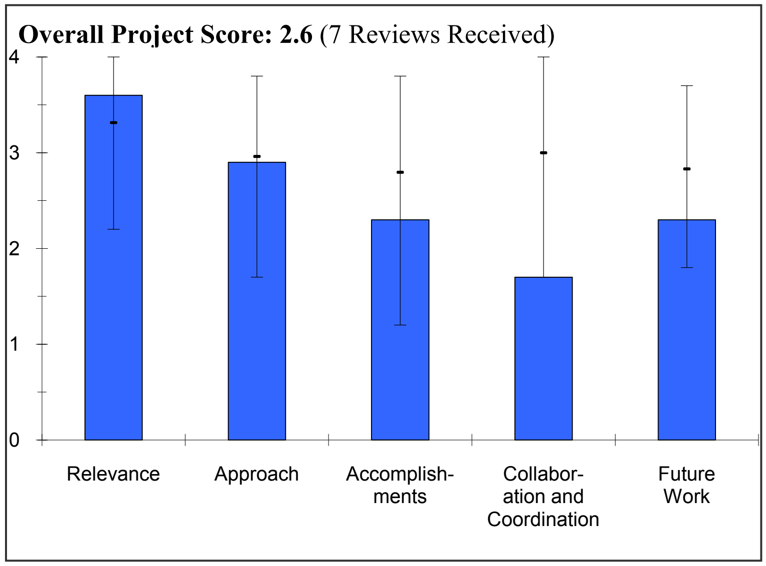
- The project plans cover what is needed for successful completion. However, this reviewer is concerned about the time and money left in this program. Furthermore, the inconsistency between the PC and I-V curves for 3-DSM composite membranes indicate perhaps a thin film of polyelectrolyte on the reinforcement surface, which in turn would affect the proton conductivity results in a positive way (through plane vs. in plane). Further characterization and optimization is needed.

Project # FC-37: Rigid Rod Polyelectrolytes: Effect on Physical Properties Frozen-in Free Volume: High Conductivity at Low RH

Morton Litt; Case Western Reserve University

Brief Summary of Project

Project objectives are to: 1) synthesize polyelectrolytes that reach or exceed DOE low humidity conductivity requirements, 2) use materials and synthetic methods that could lead to cheap proton exchange membranes (PEM), 3) understand structure/property relationships in order to improve properties, and 4) develop methods to make these materials water-insoluble and dimensionally stable with good mechanical properties.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Development of robust, high temperature, low relative humidity (RH) membranes is vital for the successful commercialization of fuel cells (FCs), especially for vehicle applications as high temperature and low RH, allows a simplified (cheaper) balance of plant (BOP).
- Membranes for hot and dry service are relevant to DOE's objectives.
- Membranes with high conductivity at low RH would provide significant performance benefits and enable PEMFC system simplification and cost reduction.
- This program is very relevant to DOE R&D objectives for a more durable conductive membrane.
- This certainly is a relevant project and will help design simpler fuel cell systems for practical purposes. Being able to operate at a higher temperature with no or low humidification, while maintaining high performance (which requires higher membrane conductivity) and efficiency, are key requirements.
- The project is very relevant and shows promise for new materials to meet DOE targets.
- Interesting and relevant to the program. Novel polymer chemistry is being developed to produce materials that are thermally stable and have enhanced conductivity at low RH. It is not clear if this is going to be a cost-effective solution, considering the chemistry and reagents being used.

Question 2: Approach to performing the research and development

This project was rated **2.9** on its approach.

- While the PI is clearly a world leader in polymer synthesis, this approach clearly lacks a strategy or pathway of getting a high potential or high proton conducting polyelectrolyte into a commercialized mechanical robust membrane. Instead of focusing in on a promising polyelectrolyte and trying to make it more viable for FC applications, the PI moves onto another polymer system.
- The approach of a rigid rod structure "locking in" volume to hold water is valid.
- Apparently, other parts of the system; e.g., mechanical stability, have not been easy to achieve.
- Base polyparaphylene polymers have proven to have very high conductivity. The focus has been to apply a variety of techniques to make mechanically stable membranes via high molecular weight (MW), grafting, and co-polymerization.
- They have taken a very novel approach to high conductivity at low RH.

- This is a very creative, out-of-the-box approach, but it is also a risky one! The PI is approaching this methodically to address issues with mechanical properties and also to address the issue of poor co-polymerization with non-polar co-monomers.
- The approach is good for the preparation of new materials, but it needs work on stabilizing them and making them water insoluble.
- The project approach is generally reasonable. The work to crosslink the polyelectrolyte is understood, but there is a need to do some characterization of membrane properties, *ex situ* polyelectrolyte durability, and membrane electrode assembly (MEA) testing to establish proof of concept that this technology will work in a FC.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- On numerous slides, incredibly high proton conductivity as a function of RH is demonstrated, but no work on stabilizing these materials has taken place. As previously mentioned, instead of focusing in on a promising polyelectrolyte and trying to make it more viable for FC applications, the PI moves onto another polymer system.
- Materials stability is still an issue. By now, cell testing should be underway.
- So far, attempts to make mechanically stable polymers have not worked.
- The conductivity of these materials is very high at low RH percentages and exceeds targets. Demonstrating progress on making membranes with suitable mechanical properties and chemical stability will be important for the final year of the program.
- It is a difficult task and will be a challenging one to find an optimum solution. Good progress was made toward developing water insoluble PEMs with high ion exchange capacity (IEC) and high conductivity by grafting alkyl benzenes on sulfonic acids. Some of them even demonstrated dimensional stability. Issues remain with mechanical properties and other areas.
- The accomplishments are excellent except in the area of working with a collaborator to form stable materials that can be further tested and characterized.
- Some progress has been made toward crosslinking the polyelectrolytes, but more is required.
- No progress has been made on determining the *ex situ* durability of these materials and little progress has been made on membrane properties, outside of solubility and conductivity.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.7** for technology transfer and collaboration.

- There have been no collaborations outside. "Conversations" with outside people is the bare minimum.
- There seems to be active involvement only with other universities.
- The project could benefit from additional polymer synthesis expertise. Approaches from General Motors (GM) were rebuffed because of intellectual property (IP) concerns.
- This program would benefit from greater interaction with a stack or MEA manufacturer for more FC evaluation.
- They appeared to have good collaboration. A slide recognizing various partners is always a good idea!
- Seriously needs some collaborative support and acceleration of developing a stable testable material.
- Other than the Florida Solar Energy Center (FSEC) and Dr. Pintauro's involvement, there is no evidence of solid collaboration with other institutions or industry.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- In my opinion, until the PI starts to take proactive steps to stabilize promising polyelectrolytes and works with outside institutions, this project will be nothing more than an academic exercise.
- The project has less than a year to go.
- The path forward is unsure, nebulous, and somewhat Edisonion.

- Working with Pintauro at Vanderbilt to electrospin composite membranes may enable preparation of stable membranes. Otherwise, it looks like more of the same ideas will be tried.
- Developing a water-stable membrane will allow more extensive testing of this important new material. This should be a high priority.
- A significant amount of high risk work is ahead of the PI to successfully complete this project. Completing this task within the remaining budget and on schedule will be challenging.
- Electrospinning and grafting are possible pathways to stabilize the materials and should be pursued with urgency.

Strengths and weaknesses

Strengths
- They have exhibited world-class polymer synthesis.
- Inherently highly conductive materials developed that exceed DOE targets.
- Very novel approach to high conductivity at low RH.
- Out of the box, creative approach!
- Excellent materials development.
- Creative approach and the conductivity results of unoptimized membranes look promising.

Weaknesses
- There is no membrane stabilization in the works. There are no collaborations with other institutions.
- The project seems to be manpower limited.
- They have poor collaborations. Very little progress has been made in the past year.
- This program would benefit from greater interaction with a stack or MEA manufacturer for more FC evaluation.
- Inherently poor mechanical properties of the rigid rod material.
- They lack urgency in preparing a stable, water-insoluble material and collaboration with industry.
- Mechanical properties of the materials may be a problem if there is any degree of swelling of the membrane in the MEA. The approach may not adequately address this and it is not clear that putting it in an inert matrix will solve the problem. Water solubility of the materials also hasn't been fully addressed.

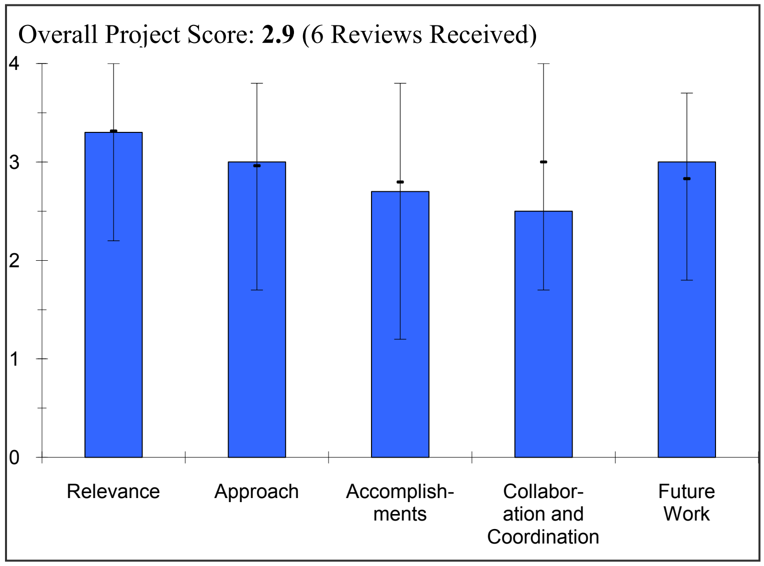
Specific recommendations and additions or deletions to the work scope

- Pursue membrane stabilization and increase collaborations.
- Professor Litt claimed that copolymerization with non-functionalized monomers and blocking cannot be done, but I would still like to see block polymerization attempts added to the program. Include quantifiable measurements of mechanical properties (tensile testing and swelling).
- This program would benefit from greater interaction with a stack or MEA manufacturer for more FC evaluation.
- Narrow down the scope of the proposed future work.

Project # FC-38: NanoCapillary Network Proton Conducting Membranes for High Temperature Hydrogen/Air Fuel Cells
Peter Pintauro; Vanderbilt University

Brief Summary of Project

The project objective is to fabricate and characterize a new class of nano capillary network (NCN) proton conducting membranes for hydrogen/air fuel cells (FC) that operate under high temperature, low humidity conditions, with: 1) high proton conductivity, 2) low gas crossover, and 3) good mechanical properties. The 2009-2010 project goal is to prepare NCN proton exchange membranes (PEM) where high ion exchange capacity (IEC) sulfonated polyphenylene (from Morton Litt's group at Case Western Reserve University (CWRU)) replaces the sulfonated polyhedral oligometric silsesquioxane (POSS) and a commercially available uncharged polyphenylsulfone (PPSU) replaces Norland Optical Adhesive (NOA) 63.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Membranes which operate under high temperature and low humidity conditions and maintain high proton conductivity, low gas crossover, and good mechanical properties are important to achieving DOE's FC performance and durability goals. The objective should also contain a membrane cost target that is consistent with DOE's cost targets.
- The project is relevant to the objectives of DOE's multi-year R&D plan. The activities are very much aligned to DOE's goal. Improvement of low relative humidity (RH) membrane conductivity is critical to the success of DOE's hydrogen research initiatives. The NCN method is a unique way to generate nanomorphology mediated block-copolymer morphology.
- The project is aligned with DOE goals and targets. High temperature membranes are an enabling technology that allows system simplification and cost reductions.
- There is good support of DOE program objectives. It would help to outline the benefits of this approach versus using an expanded poly-tetrafluoroethylene (PTFE) scaffold, especially since perfluorosulfonic acid (PFSA) ionomers appear to be the material of choice for the work now.
- The project is relevant to DOE objectives. Development of stable, low resistance membranes is important to the success of the DOE Hydrogen Program, and this project is making a contribution to that effort. The relevance is diminished to some extent by the dependence of the PI on materials supplied by other groups, which are not necessarily being developed with funding from this project. As it stands, the relevance of the project is limited to membrane architectures. The project does not include a strong proton-conductor development component, which is arguably a bigger challenge in developing membranes for hot and dry operation.
- This project represents a concept for creating durable membranes by allocating mechanical stability to a blended component. Because durable membranes are highly relevant to the DOE effort's goals, this project is highly relevant.
- The project has been using both PFSA-based and hydrocarbon ionomers, which imply that the project has not limited itself to proton conduction in one temperature regime, which further extends project relevance.
- Other targets (low gas crossover) are also taken into consideration.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The research shows promise, but it has not yet focused on a single process methodology, and appears to still be in "scouting" mode. A more in-depth study of the best process should be completed within the next year.
- This is a "out-of-the-box" approach to the solution to low RH membrane conductivity. Changes in the electrospinning material composition and corresponding inert materials is a good approach in exploring the feasibility of obtaining a functional nanocomposite membrane. Mixing inorganic additives to the ionomers to make inorganic additive impregnated ionomeric nanofibers is also a good approach to add different materials to nanofibers. However, the team should carry out studies on the leachability of such additives from ionomeric nanofibers. The approach to make "Hollow Bore" Nafion® nanofibers is an interesting approach.
- This reviewer is not sure about using the materials mentioned.
- This is an interesting and flexible approach that may be applied to a variety of polymer chemistries to make composite materials.
- The approach of the project is to develop membrane architectures that combine a proton-conducting component with an engineering polymer capable of supplying the necessary mechanical properties. This is a sound and reasonable approach. Collaboration with 3M to incorporate low equivalent weight (EW) ionomers is a critical part of this approach. If successful, the approach would succeed in producing membrane materials that meet DOE resistance targets while maintaining good mechanical and chemical stability. The focus, however, seems diffused, with too much emphasis on experimenting with various combinations of proton-conducting and support polymers, and insufficient emphasis on using the knowledge gained to develop high performance membranes.
- Many membranes prepared in the DOE projects are attempting to decouple mechanical stability from the conductive ionomeric phase in order to create greater flexibility in ionomer chemistry, while also providing mechanical stability. This project is consistent with those approaches, while adding the novelty of reversing the usual roles by preparing the ionomer in a "strand-like" phase, while the non-conductive component fills the remaining volume (or is also electrospun).
- The identified approach still leaves some questions unanswered, particularly those related to the cost of scaling up the electrospinning process.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Significant progress has been made in this project, but the results presented do not demonstrate a significant advantage over current commercially available materials.
- Most of the results presented show that the performance of nanofiber-based membranes depends on the volume fraction of ionomer present in the composite membrane. With Nafion and 3M PFSA, the composite membrane performance depends on the total amount of ionomer present in the composite. Hence, the advantage of having an ionomeric nanofiber matrix has not yet been realized. So the question is whether there is any promise of going for such a route. If the overall performance depends on the total ionomer quantity and the nanofiber entangled matrix doesn't offer any advantage, then pursuing this membrane fabrication route is questionable.. The team should think along this line.
- Replacing NOA with polyphenylsulfone and developing a dual fiber mat and consolidation technology demonstrates the practicality of this approach.
- Swelling data is useful, especially the comparison of Nafion 212 with a 3M825 mat.
- Durability comparison should be done using the same cycles for both Nafion and nanofiber composite.
- No conductivity data at high temperature and low RH for this past year—still need to work toward high temperature, low RH targets with stable materials, or show nanofiber geometry stabilizes materials (CWRU materials or lower equivalent weight PFSAs from 3M).
- A good amount of progress has been made, especially with eliminating the optical adhesive as the matrix.
- Accomplishments this year have been good, but not as good as the previous year. Interesting work has been performed, but it is not clear in all cases that this work represents significant progress toward meeting DOE targets. Replacement of the NOA 63 resin with PPSU is a good step. However, the value of the studies

comparing Nafion fibers in PPSU matrix vs. the inverted PPSU fiber in Nafion matrix is unclear. One would have hoped that the PI would have continued to make progress in decreasing area resistance by optimizing structure, thickness, nature of ionomer, and use of additives, but there is no data or other indication that resistances of current membranes are lower than those from a year ago. Use of a higher EW ionomer like commercial Nafion is acceptable in this project to develop fundamental understanding of the nanoporous network, but it would have been nice to have seen a low resistance membrane incorporating a low EW ionomer to demonstrate that progress has been made over the past year in developing materials that will be useful in FCs. Without such a demonstration, it is not clear that the work done over the past year is actually leading toward the development of viable materials for FCs. The preparation of electrodes based on ionomer nanofibers, in contrast, represents a potentially significant development for FCs. This electrode work should be emphasized going forward.
- Although the concept has been shown to deliver high conductivity results, efforts with more conductive PFSA, and with use of PPSUs for mechanical stability have not yet shown exceptional conductivity. However, what is being compared is essentially Nafion versus less Nafion, so not much can be expected yet.
- Nice progress has been shown for open circuit voltage (OCV) testing with electrospun Nafion (at least 200 hours near 2 mA/cm^2 hydrogen crossover), and for low in-plane swelling using the 3M ionomer (lower than PFSA baselines).
- Electrospun electrodes are interesting, but have not demonstrated a tangible advantage yet.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- 3M and Nissan are both capable of providing practical industry feedback.
- The team consists of good partners and good collaborative work is expected. So far, no technology transfer scenario is reported by the team. Except obtaining a PFSA ionomer from 3M, not much collaborative work with the partners has been reported.
- Extending collaboration with Nissan would be helpful.
- There is good collaboration with 3M.
- The project aims to develop membrane architectures that are conducive to stable, low resistance operation, but does not address the chemistry of the low EW or otherwise highly conductive ionomers required as the proton-conducting component of these architectures. Effective collaboration is required to supply this component. The collaborations with 3M and with CWRU are, therefore, vital to the success of the project. A lack of data with CWRU materials, and a lack of data with more advanced 3M materials, suggests that the collaborations are not yielding as much fruit as one would have hoped.
- As indicated by the low x-y swelling data for the nanofibers generated from 3M 825 EW ionomer, the 3M collaboration is contributing to the progress of this project.
- In the question and answer part of the presentation, it was mentioned that cost analyses are being performed with a company in Tennessee. This collaborator should be identified.
- Some test protocol sharing exists with Nissan, but this does not appear to be an extensive effort.
- This could be an opportunity for greater collaboration as the project attempts to deliver final membrane samples and evaluations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The future work plan is ambitious, considering past progress and the project end date, but the work is relevant and could lead to better membrane and/or electrode manufacturing processes.
- Most of the emphasis of the future work is on further optimization of the nanofiber matrix and nanofiber membrane formation. Very little emphasis is given to MEA fabrication and FC testing. The team should first determine the limit of this technology and see whether the nanofiber route is capable of delivering membranes with low RH conductivity better than corresponding dense membranes made with the same ionomer. Otherwise, the research work will remain far away from DOE's low RH membrane goal. So far, all the data suggests that

the nanofiber membranes perform to the level of available ionomer volume fraction and doesn't show any performance advantage over corresponding dense polymeric membrane.
- Stability of the sulfonated polyphenylenes from CWRU is questionable. A back-up plan is needed for increasing conductivity at high temperatures with low RH.
- Collaborations with others in the high temperature membrane group could be beneficial. The team could look into spinning CSM polyPOMs or using them as additives.
- The future work proposed is logical and is constructed to move the project in the correct direction.
- For the most part, the future work is reasonable, but some parts should be deemphasized. In particular, the work on hollow bore Nafion fibers should not be continued unless the PI has a reasonable path toward producing narrower fibers; from the presentation, I gather that he does not have such a path. However, the PI verbally indicated during the presentation that work on hollow-bore fibers is ending, so for the purpose of scoring the review I will give precedence to this comment and ignore the fact that hollow-bore nanofibers are listed on the future work slide.
- The proposed future work on inert or uncharged polymers has not been sufficiently justified. The benefits of this work are unclear. The inadequacies of polyvinylidene fluoride (PVDF) and polyphenylsulphone (PPSU) were not pointed out, and Vanderbilt should explain if there is reason to believe that other materials will be better.
- The electrode work should be emphasized, as this is a promising application for electrospun nanofibers. The initial work looks very promising and should be pursued aggressively to eliminate potential poisoning components and to maximize performance.
- The proposed new membrane samples are as expected from the context of the presentation. The continued use of 3M PFSAs is the right direction.
- The examination of alternative non-conductive components will likely prove to be useful, although the deficiencies of the incumbent PPSU have not been clearly reported (the x-y swelling appeared to be low).
- The hollow bore materials are worth studying for the possibilities that exist in promoting proton conduction, but the size must be reduced.
- It is not yet quantified what the motivation might be for electrospun electrodes. An oxygen diffusivity measurement might be useful. Some questions may be asked as to whether this electrode structure would tend to be overloaded with ionomer, and, therefore, promote platinum dissolution.

Strengths and weaknesses

Strengths
- The project pursues an alternative route to low gas crossover membrane fabrication. The team is solid, with a good knowledge base of the challenges in this project. The project is exploring an alternate route for making block-copolymer type morphology in membranes. The electrospin process is versatile and can accommodate many different types of ionomers and ionomer mixtures. This gives the team the flexibility in fabricating and evaluating many different types of composites.
- Vanderbilt employed a novel materials engineering approach to improving membranes.
- Vanderbilt used a novel approach toward creating composite membranes, one that is completely different from most of the other approaches out there.
- The electrospinning technique is a good technique to make interesting and potentially useful membrane and electrode structures. The project is not dependent on the development of any one ionomer chemistry, but rather could be applied to just about any ionomer capable of being electrospun. If combined with the best available proton-conducting materials, the project has the potential to produce high performance membranes. Electrodes produced with electrospun fibers would have a promising architecture for good mass transfer, as well as good protonic and ionic conductivity.
- The project manages to present a novel concept that is still consistent with the strategy of many of the other projects.
- The project has shown the capability of creating materials that are highly conductive at 120°C, 50% RH.
- The project has responded well to past feedback and has incorporated PFSAs as candidate ionomers.
- The project has been able to adapt the primary technique of electrospinning quite capably for a variety of materials, and has been able to find ways to work with materials that have been historically tough to work with, through the addition of polyethylene oxide (PEO) or other species.

Weaknesses

- The project should focus more on the feasibility study of nanocapillary methods for obtaining a functional, low RH membrane. So far, none of the performance data suggests that the nanocapillary route to membrane fabrication offers better conductivity than the dense membrane made from a parent ionomer. The team should work more to determine whether the mix of ionomer nanofibers offers any potential of having nano composite membrane with higher low RH conductivity than its parent ionomers.
- The ionomer synthesis is a weak aspect of the project.
- The process is complicated, especially for a dual fiber mat with a polysulfone matrix.
- The lack of a significant ionomer-development component makes the PI dependent on partners, or non-partnering institutions, for ionomer materials. So far, no results have been presented for the highest performance state-of-the-art or experimental ionomer materials. The project has not demonstrated a high performance membrane in the last year.
- The project still needs to be able to deliver final membrane samples using components that are expected to be durable and highly conductive.
- There are some areas where project decisions should probably be explained more clearly. For example, the reasons for trying different non-conductive materials as mechanical supports were not explained. . This reviewer would also like to know if there is any data that supports the use of electrospun electrodes.
- A cost estimate is still needed.
- There is still some opportunity for greater collaboration.
- *In situ* diagnostics for cell tests should be reported; e.g. high frequency resistance (HFR), hydrogen crossover, electrochemical area (ECA), etc.).

Specific recommendations and additions or deletions to the work scope

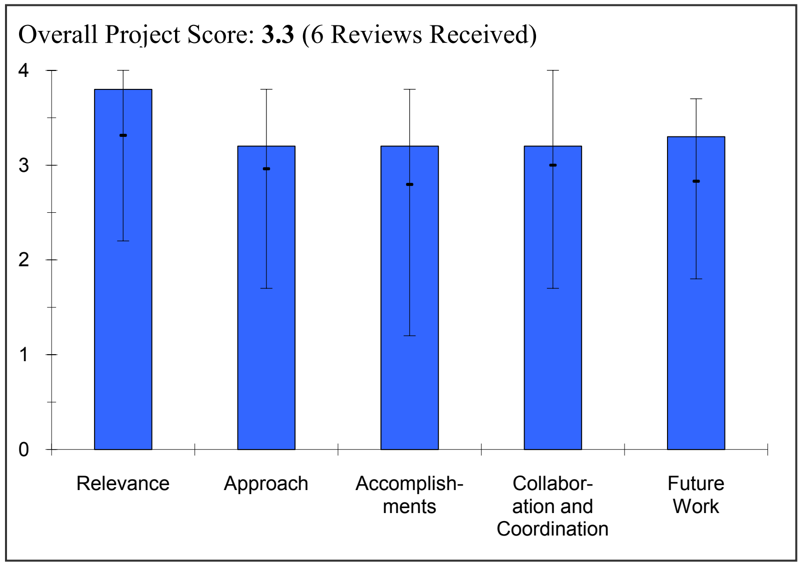
- Include a cost study to prove that an electrospun membrane can be consistent with DOE's cost targets.
- Compare electrospun membrane performance, durability, and cost against current competitive options (W.L. Gore & Associates (Gore), or 3M membranes, not Nafion).
- Add a cost study and a comparison of projected cost for electrospun fibers vs. three-dimensional mats; i.e. the Giner approach.
- Eliminate hollow-bore nanofiber work.
- Without compelling data for the electrospun electrode effort, this part of the project could be truncated.
- A cost estimate should be added and the collaborating party should be listed.
- RH cycling should be performed soon with a 3M-based PFSA for either one of the two methods (ionomer surrounded or uncharged polymer surrounded), just to have some baseline.
- More rationale could be provided for the down selection of uncharged polymers.

Project # FC-39: Novel Approaches to Immobilized Heteropoly Acid (HPA) Systems for High Temperature, Low Relative Humidity Polymer-Type Membranes

Andrew Herring; Colorado School of Mines

Brief Summary of Project

The overall objective of this project is to fabricate a hybrid heteropoly acid (HPA) polymer (poly-polyoxometallates (poly-POMs) from HPA functionalized monomers with conductivities (σ) >0.1 S/cm at 120°C and <50% relative humidity (RH). The objective for 2010 is to optimize hybrid polymers in practical systems for proton conductivity and mechanical properties. The objective for 2011 is to optimize hybrid polymers for proton conductivity, mechanical properties, and oxidative stability or durability.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- The development of robust, high temperature, low RH membranes is vital for the successful commercialization of fuel cells (FCs), especially for vehicle applications, as high temperature with low RH allows a simplified (cheaper) balance of plant (BOP).
- The project directly addresses membrane development, one of DOE's key technical issues.
- The development of a membrane that is highly conductive and stable at 120°C would enable significant fuel cell system (FCS) simplification and cost savings.
- The work addresses the very significant challenge of high conductivity, low RH, high temperature membranes.
- This certainly is a very relevant project and will help design simpler FCSs for practical purposes. Being able to operate at a higher temperature with no or low humidification, while maintaining high performance (requires higher membrane conductivity) and efficiency, are key requirements.
- This project is very relevant. The need to provide conductivity in the absence of water is directly addressed. The project has materials that are inherently conductive and hence its full upside potential is very large.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The concept of immobilizing HPA materials into a polyelectrolyte is a very interesting approach which has fairly high potential.
- PI states that a series of experiments, which were known beforehand not to be viable, were necessary (model compounds which certainly would have dissolved), but this reviewer still does not believe their validity.
- The project has overcome most of the issues expected with HPA-based membranes.
- The Colorado School of Mines (CSM) has employed a unique approach of incorporating "super acids" in a polymer backbone to enable low RH conductivity. Several types of HPA polymer systems have been studied, including those with and without methylene groups. There's been some work on making chemically and mechanically stable films, but more emphasis is needed there.
- The work seeks a novel approach, but with a clear connection to achieving a manufacturable solution.
- The very focused and methodical approach has been yielding promising results.
- The staged approach is good and demonstrates the utility of the HFA additives.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Conductivity as a function of RH is certainly excellent and far exceeds the DOE milestones. Immobilization techniques with 3M appear to be progressing well, and preliminary results look promising, provided the baseline material (pink line on slide 17) does not conduct protons as the PI stated.
- Conductivity has been established and good mechanical properties are claimed, although IP issues prevent full discussion.
- CSM seems to have settled on the $HSiW_{11}(vinyl)_2$ HPA to develop membrane concepts. I couldn't understand the morphological studies results presented for Poly-POM 85v. There is some encouraging conductivity data on the initial type II copolymers, but these materials are not mechanically stable. The type III copolymers developed with 3M should provide better mechanical stability, but the conductivities are lower than perfluorosulfonic acid (PFSA) ionomers.
- There has been good progress in high temperature conductivity, but there aresolubility challenges.
- They were consistently demonstrating high proton conductivity in robust films. Several film chemistries were developed that demonstrate high oxidative stability and promising mechanical properties.
- The progress is good, although one might think 3M could provide more assistance with the polymer synthesis and modification portion of the work.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The collaboration with 3M is very strong and clearly is shown throughout the project. In fact, this collaboration is exactly why this program is so strong and should be used as a benchmark for future DOE collaborations. My suggestion is to have the stack developer involved at one point for outside testing and validation.
- The industrial partner provides valuable technical and material support.
- There is a strong collaboration with 3M to develop the copolymers.
- There is a good connection to manufacturing through 3M. There is also a good use of external test house through the Florida Solar Energy Center (FSEC).
- They have exhibited a satisfactory level of collaboration with development partners.
- The addition of General Motors (GM) is good, but they could use more academic collaborations to extend understanding. 3M might not share results.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The proposed future work is outstanding and is in line with previous accomplished work. CSM could perhaps add a third party for cell testing and validation.
- The project ends in less than a year.
- The remaining work focuses on morphology, durability, and FC testing.
- Cost needs to be addressed.
- The plan is to develop materials that have both good conductivity and are mechanically robust. It is unclear if any more HPA monomer work is going on or if CSM has settled on the $HSiW_{11}(vinyl)_2$ monomers for all membrane concepts. Explicit mechanical testing should be included. Some effect should be focused on improving lower temperature conductivity. The project could also benefit from some modeling effort to understand the conduction mechanism in these systems to guide in materials selection.
- These are all reasonable plans. One would like more detail on plans for addressing solubility.
- CSM has well thought out future works. The combined efforts at CSM and 3M should yield an optimum solution.
- It seems like an awful lot to do in the last year. It would be good to break out which organization is doing what. The plans are challenging.

Strengths and weaknesses

Strengths
- The immobilization of HPA into polymer backbone has the potential in achieving key DOE milestones. The collaboration with 3M is excellent.
- They employed a unique approach to meeting low RH conductivity targets. They have a strong collaboration with a polymer and membrane supplier in 3M.
- They used a novel approach that adds to knowledge base.
- Material selection and zooming in with the right platform are both strengths of this project.
- The HPA materials are unique and provide a novel mechanism.

Weaknesses
- They took perhaps too long to get this point. The model compound studies perhaps were not necessary.
- Low temperature conductivity is an issue.
- The materials for type III copolymers were not disclosed, so the value to the FC community is limited. There is a lack of mechanical characterization, understandable swelling data, stability measurements, and modeling effort.
- Solubility needs to be addressed.
- The reviewer didn't get a good feel for projected durability in a real-world environment.
- CSM needs more synthetic help.

Specific recommendations and additions or deletions to the work scope

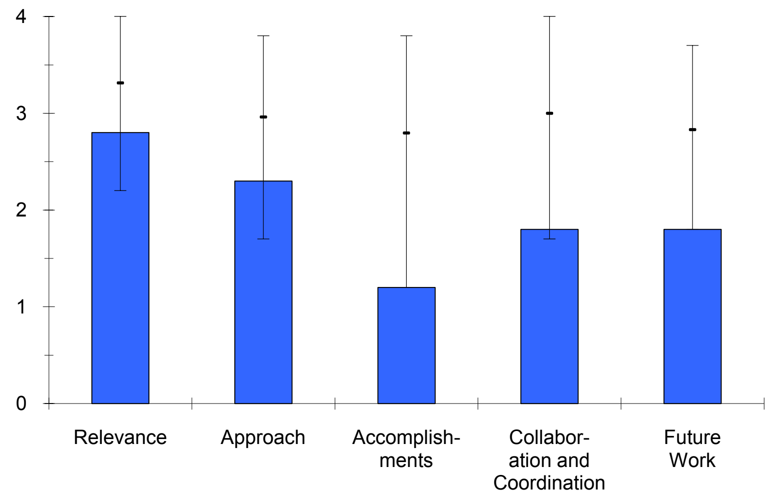
- Add a stack developer for outside testing and for validation when a stable membrane is made.
- I think that adding a functional group, such as sulfonic acid, to the copolymers in type III may warrant investigation. Add mechanical/swelling testing and hydrolytic stability testing to scope.
- 3M needs to do more in this project.

Project # FC-40: High Temperature Membrane with Humidification-Independent Cluster Structure
Ludwig Lipp; FuelCell Energy, Inc.

Brief Summary of Project

The objectives of this program are to: 1) develop polymer membranes with improved conductivity at up to 120°C, 2) develop membrane additives with high water retention and proton conductivity, 3) fabricate composite membranes, 4) characterize polymer and composite membranes, and 5) fabricate membrane electrode assemblies (MEAs) using promising membranes.

Overall Project Score: 1.9 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Polymer membranes with improved conductivity at up to 120°C are relevant to DOE targets, but the objective makes no reference to cost or durability targets.
- The project is relevant to the requirements for high temperature membrane development and meets overall DOE objectives. The goals and objectives of the project are designed satisfactorily. The multi-year plan is in line with DOE research, development & demonstration (RD&D) objectives.
- The project goals are relevant. The presentation spent too much time on this, and not enough time on how they would actually do this.
- The project is aligned with DOE targets and goals. High temperature membranes are an enabling technology that will allow system simplification and cost reductions.
- The development of composite membranes with high conductivity under hot and dry conditions is relevant to the goals and objectives of the Hydrogen Program. The relevance of the project is limited by the refusal of FuelCell Energy, Inc. (FCE) to share information with the public, despite the fact that the work is funded by the public.
- This project is marked "good" for relevance, since it seeks to fabricate high temperature membranes that will meet targets for conductivity, durability, and gas crossover.
- It is well understood within the program that membranes are needed that can perform at temperatures up to 120°C.
- Although there are no compositional details provided as to the materials being used, there is no reason to conclude that this work is not attempting to achieve targets relevant to automotive fuel cell (FC) development.
- Because chemistry is not identified, this project can only be rated "Good" for relevance. For all that the reviewers know, there may be unstable chemistries or high cost processing involved.

Question 2: Approach to performing the research and development

This project was rated **2.3** on its approach.

- The approach seems well planned. They need to approach references, cost, and durability, which were neglected in the project objective statement.
- The approach of making composite membranes possessing materials with adequate properties is a good idea. However, interaction between these multiple components in the composite under low relative humidity (RH) FC operational condition is the unknown.
- It is very difficult to assess the work. The approach seems okay, but experiencing a lot of different additive or instability issues, as this project is, is asking for trouble.

- It is difficult to judge the approach as the description of the approach is limited. Very little information on the approach was provided.
- The approach of using a low equivalent weight (EW) perfluorosulfonic acid (PFSA), with reinforcement to provide mechanical stability, and additives to provide increased water uptake and increased protonic conductivity, is a good one. The project effectively addresses technical barriers.
- Details regarding the chemistry of the ionomer, reinforcement, and additives have not been provided. Details regarding processing have not been provided. Therefore, without knowledge of the chemistry involved, the approach cannot be judged.
- In the past, distinctions have been made between membrane projects based on approach. Some projects have used ionomers with unstable functional groups, or have used reinforcements that proved to be too costly. This project, however, has attempted to avoid any judgment by not revealing composition or processing. In doing so, it violates the intention of publicly funded research.
- Partners in the project have not been identified, which is completely unprecedented in a DOE effort!

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.2** based on accomplishments.

- Qualitative descriptions of challenges and solutions did not provide an adequate explanation of technical accomplishments. More details and data need to be shared in order to prove that the project has made any measurable progress.
- No data have been provided by the team to conduct fair analysis and review of their technical accomplishments. Increasing the synthetic capability of the conductivity enhancer from a one gram (g) batch to a 25 g batch and water retaining additive from two g to eight g is not a huge increase. A 25-g batch size is too small to claim any commercial feasibility of the material. It's understandable that the structure of these additive materials is company confidential; however, the team should at least tell the nature of these materials. From the presentation, it's not known whether these materials are compatible with each other or not. The claims of technical accomplishments in slides 11-17 don't include any data. It's very hard to judge these accomplishments without seeing any data. The cost figures claimed by the team don't have any baseline membrane material against which they have claimed improvements. The team needs to provide more explicit data for the reviewers to understand their progress.
- One really cannot assess this. We are given isolated data points with no graphical presentations, which would give us some ability to assess the quality of the data. The presenter is hiding his data. There has to be more data that can be provided that will not give away intellectual property (IP).
- No data was provided, so it is not possible to determine what progress has been made. More information needs to be provided.
- FCE already had a membrane capable of meeting conductivity targets at last year's AMR. This year, they should have reported MEA testing results. Although indicating that membrane samples have been supplied to the University of Central Florida (UCF) with "promising" initial results, they provided no data and no quantitative information. Shockingly, this is the only 2010 milestone marked as completed. The other three milestones are in progress or are planned. Assessing progress toward milestones is difficult when specific dates are not provided for each milestone, but still, the fact that only one out of four has been achieved by this point in the year (and the assertion that the one that is complete is questionable) is disappointing. FCE does provide various assertions about progress in improving additive retention, lowering additive cost, etc., but evaluation of these assertions is complicated by the fact that FCE has chosen to withhold all data and nearly all technical information from their review presentation.
- Except for the "Project Summary Table" slide, no data were provided.
- The fact that a membrane was fabricated that achieved lower temperature targets was already known from prior years.
- The achievement of a range of 86-148 S/cm at 120°C with 50% RH is not impressive for the following reasons: 1) the target should actually identify 25% RH as the humidity reference in order to avoid water partial pressures over 50 kPa (100 kPa of water is not feasible and pinches oxygen partial pressure), and 2) the composition of the membrane, as well as prospects for expected durability, including consideration of swelling or leaching, is completely unknown. This is unacceptable for a membrane project in this late stage.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- FCE seems to be collaborating with a number of other companies and institutions.
- The partners in this project seem to be nameless. It's hard to assess the capability of the partnering entities without their names. The team needs to provide the names of their partners.
- This is very difficult to assess. It seems they may be keeping some collaborators secret.
- Their collaborators are not identified, and it is unclear how many collaborators there are and what roles they play. More information needs to be provided.
- The fact that FCE is collaborating with a polymer partner and with additive partners, and the fact that changes in the ionomer and in the additive synthesis have been made, suggests that some reasonable level of collaboration is occurring.
- Collaboration can usually be measured based upon the reputation of the collaborators, the work plan to involve them (delineated by tasks), and the results that speak to the collaboration. Here, the collaborators are not named, and their efforts are addressed in abstractions that reference unidentified baselines; e.g., "15x lower material cost" or "6x shorter processing time").
- The collaborators are unknown, and, save for a few hints, their efforts are unknown. There is no way to objectively rate collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- More quantitative data and technical detail is required to evaluate the quality of the proposed future work.
- The future work, as explained by the team, seems to be aligned with their present status of their work. However, due to the lack of data, it's hard to know their present status and correlate it to their future work. The only option that we have, as reviewers, is to believe the claims of the team and reply on their judgment. The team should provide more data to help the reviewers.
- This entire section was too vague.
- The proposed future work is reasonable, although inclusion of more specifics and a clear timeline would have been nice. The existence of alternative development pathways is unclear.
- Any evaluation of future work is meaningless without data pertaining to material composition, swelling, leaching, chemical durability (such as open circuit voltage (OCV) testing), mechanical durability (such as RH cycling), or gas crossover.
- No cost estimates or cost study has been shown for the materials studied here, which means that the worthiness of any milestone associated with selecting a "low cost" membrane design is impossible to judge.
- No data were provided on the 1,000-hour stability test. The test is not identified to show whether it is relevant to targets. DOE-prescribed tests for membranes do not call for a "1,000 hour" metric (OCV test has specified 200 hours or, more recently, 500 hours; the RH cycling test has specified 20,000 cycles).

Strengths and weaknesses

Strengths
- They have a good concept for a multicomponent membrane. Unlike other researchers, who try to develop one material with multiple problem solving features, this team has made a good choice of using different components to address different issues and challenges in the membrane.
- The overall approach is reasonable and FCE seems to be getting good materials from their suppliers. The project has succeeded in producing materials with good conductivity under hot and dry conditions, although most of these accomplishments were realized prior to the current year, and significant progress in the current year has not been conclusively demonstrated.
- In general, the project follows a similar strategy to what would be expected—the development of a high conductivity, water-insoluble ionomer with reinforcement to ensure mechanical durability, and with additives, to enhance proton conduction.
- Conductivity measurements from the project are high relative to other projects.

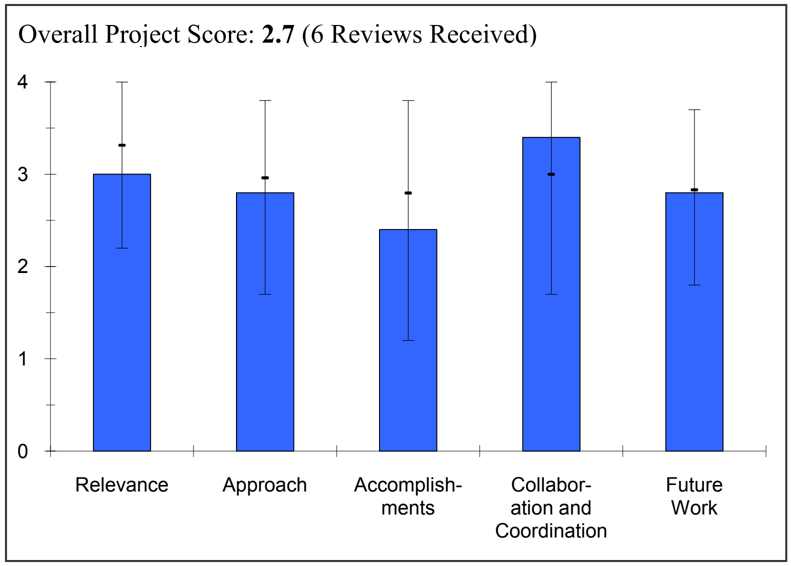
Weaknesses

- The qualitative descriptions of challenges and solutions did not provide an adequate explanation of technical accomplishments. More technical details and quantitative data need to be shared in order to prove that the project has made any measurable progress.
- Except for claims of accomplishments, no data has been provided by the team for review. It's absolutely impossible to conduct an outside review without seeing the data. The team needs to disclose more data to help reviewers understand the synergy between these multiple components and to understand the feasibility of maintaining such synergy under low RH FC operational conditions. From the information provided in this presentation, an independent review cannot be done for this project.
- The lack of information on the materials and processes, and the inadequacy of the data, raises large suspicions about the quality of this project.
- The lack of data and any information on the systems being investigated make it impossible to judge the work.
- The value of this project to taxpayers is questionable. FCE has, to date, provided virtually no data and no technical information to the public. Without existence of any public information from this project, should FCE choose not to pursue commercialization of materials developed under this contract, the public will have gained absolutely nothing from the investment of R&D dollars into FCE.
- The complete lack of compositional or processing information leaves the project impossible to judge.
- The cost analysis is missing.
- The lack of compositional information completely removes any possibility for speculation on durability, gas crossover, swelling, and other characteristics that are crucial to understand at this late stage of the project.
- The collaborators have not been identified.
- Durability has not yet been quantified, nor does the future work contain a reasonable plan for doing so.

Specific recommendations and additions or deletions to the work scope

- More technical details and quantitative data need to be shared in order to prove that the project has made any measurable progress.
- You should be able to share more of your results while maintaining your trade secrets or IP. This project needs to at least reveal some better experimental data to provide some credibility.
- The entire project should be deleted unless FCE shows a willingness to make knowledge gained from this taxpayer-funded project available to the taxpayers.
- The time has come for this project to disclose the chemistry and processing involved with the membrane samples.
- The project needs to show data immediately. Without data, it is impossible to know which efforts need to be pursued more vigorously. Instead, we are forced to take the project at its word when claims are made that molecular weight is being increased to enhance mechanical properties, or that the removal of a hot acid step lowers cost. If only qualitative statements such as these are made, then the statements become meaningless.

Project # FC-41: Novel Approach to Advanced Direct Methanol Fuel Cell Anode Catalysts
Huyen Dinh; National Renewable Energy Laboratory



Brief Summary of Project

The overall objective of this project is to develop and demonstrate direct methanol fuel cell (DMFC) anode catalyst systems that meet or exceed DOE's 2010 targets for consumer electronics application. The project goal is to improve the catalytic activity and durability of the platinum ruthenium for the methanol oxidation reaction via optimized catalyst support interactions. A similar approach for oxygen reduction reaction catalysis is advantageous for both DMFC and hydrogen fuel cells (FCs).

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The project demonstrates relevance to the need for moved anode catalysts for DMFC systems. The project objectives are a fit with the need for improved performance and durability supporting the overall goals. While the project is still in the early stages, and it has not moved to more practical catalyst supports, there was no connection drawn to the expected cost of the catalyst compared to the potential benefits over the catalyst's expected life.
- Direct methanol anode catalysis is very important for methanol oxidation. This project perfectly fits the research need. The oriented pyrolytic graphite substrate could increase the catalyst stability under DMFC conditions.
- The project objective is to develop and demonstrate DMFC anode catalyst systems that meet or exceed DOE's 2010 targets for consumer electronics application by improving the catalytic activity and durability of the platinum ruthenium alloy for the methanol oxidation reaction (MOR) via optimized catalyst or support interactions.
- Methanol represents a liquid fuel that represents stored hydrogen. If methanol could be stably oxidized at an anode at high currents, it would be a great advance in FC technology for power sources, and, to date, this feat has not been possible. The failure mechanism of methanol platinum/ruthenium oxidation catalysis is not understood or even known. This project addresses stabilizing the dispersion and chemical composition of the platinum/ruthenium catalyst on a support to attempt to stabilize catalysis of methanol oxidation, and therefore this project is of good merit.
- The assumption is that platinum ruthenium is a suitable and chemically stable catalyst and just needs to be stabilized. This assumption may or may not be true.
- This work should make an effort to analyze stability of the dispersion by microscopy and should analyze catalyst surface composition; e.g., by X-ray photon spectroscopy (XPS) or atomic emissions spectroscopy (AES). In this way the contribution to MOR catalysis of these two effects: 1) dispersion of platinum/ruthenium, and 2) chemical surface composition of platinum/ruthenium catalysts, should be individually trackable. The relevance of this project lies in determining whether catalyst support interactions will improve 1 or 2 or both, and whether MOR activity is stabilized and/or improved.
- The intended relevance is the improvement of durability and the lowering of cost for DMFC for small portable power applications, such as consumer electronics. Development of FCs for consumer electronics is one of the DOE-EERE Fuel Cell Technologies Program (FCT) objectives, as stated in the Multi-Year RD&D plan. However, as described below, it is not clear how the project's approach will address the most prevalent DMFC catalyst degradation mechanism.

- The development of methanol oxidation catalysts with improved activity represents the greatest challenge of DMFC technology. Consequently, this project is relevant to DOE objectives. The advantage of the approach taken is still to be demonstrated, though.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The initial approach of examining a model catalyst support to examine the benefits of the catalyst fabrication process is good, but the program needs to move to high surface area catalyst supports and membrane electrode assembly (MEA) measurements to find the real benefit, as well as to determine the catalyst performance under real operating conditions. The durability of the catalyst needs to be determined in an MEA to examine if there are any benefits to this approach, particularly with ruthenium dissolution.
- The ion implantation method for the platinum ruthenium catalyst deposition is a good method for fundamental research to investigate the platinum ruthenium catalyst arrangement on the substrate. Nitrogen doped highly ordered pyrolytic graphite (HOPG) shows good performance as a support for platinum ruthenium. I believe the nitrogen replaced the oxygen on the graphite surface.
- The graphite surface contains dangling carbon-oxygen bonds. Nitrogen treatment could be effective even without ion implantation.
- The main problem for graphite support is that the graphite surface is very smooth (thinking as a good lubricate). How the platinum ruthenium catalyst stays on the surface for long term tests is a good question. Graphite surface area is quite low (even if roughing the surface), and the orientation is fixed after the catalyst deposited on electrolyte membrane or gas diffusion layer (GDL). Therefore, in the real FC, the catalyst utilization could be reduced.
- The project is to use ion implantation to stabilize catalyst dispersion (and possibly composition) -- a good first step in a rational attempt to stabilize methanol oxidation catalysis.
- It is a good idea to work on implantation to stabilize a catalyst on a well-defined surface like HOPG, because it will help understand the effects of implantation of the support on catalyst stability. Other low surface area carbon supports should be considered, like glassy carbon and edge plane of HOPG. Possibly molybdenum nitride might be considered, since it spontaneously attracts platinum, so platinum/ruthenium might benefit too. Ultimately translating the low surface area supports to high surface area supports (like nanotubes) is needed for making a practical catalyst, but this translation should be delayed until the effects of implantation on catalyst stability and activity can be understood on a low surface area support.
- The approach to lowering the cost of DMFC anodes is to improve the dispersion of platinum ruthenium on the carbon support and to maintain that dispersion over the life of the device by enhancing the interaction of the catalyst particle with the support through doping of the carbon support with other elements. However, the major degradation mechanism of DMFC anode catalysts is the loss of ruthenium from the alloy. The presentation did not acknowledge this degradation mechanism, nor did the presenter give a clear scientific rationale that doping the support would mitigate this loss mechanism.
- While interesting from the catalyst design point of view (anchoring of catalyst particles to carbon support via nitrogen), the proposed approach does not offer a clear path toward improvement in MOR electrocatalysis.
- To date there has been no evidence of improvement to catalysis of methanol oxidation with this approach. Voltammetric data attest to some possible mass-transport benefits only.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- The team has made good progress with the fundamental catalyst studies and in preparing to make catalysts on high surface area supports for the MEA studies.
- Graphite particles have different shapes and structures. So far, almost a hundred graphite structures have been reported, for example, spherical graphite, planar graphite, horn graphite, worm graphite, etc. Apparently, the research group may not know enough about graphite. Naturally boron graphite and nitrogen graphite are available. I suggest the team consult or work with other researchers to find the best graphite for ion implantation (may not be necessary to use ion implantation).

- The work plan began July 2009 (not much time to report a lot of results), and is well on its way. Preliminary results on HOPG (with a 52x gain in activity) are impressive.
- The material and equipment have been set up to learn if the approach will work. It remains to be seen if they will execute the plan methodically.
- The optimal implantation process has been determined to be a 45-second exposure, and implanted surfaces have been spectroscopically analyzed to show implanted moieties.
- Nitrogen doping was found to be the best doping to date in regards to promoting MOR catalysis.
- Three catalyst deposition methods have been found.
- High surface area catalyst making was shown (but I feel no more work should be done on this until results with low surface area supports find the best treatments, assuming this low surface area work will translate into high surface area catalysts).
- An array cell has been set up.
- Excellent progress toward the stated objectives of the project have been made over the short duration of the project. However, as mentioned in the approach section, it is not clear that the existing scope and approach of the project will overcome the most significant electrocatalytic barriers toward implementation of DMFCs in portable electronics. It is also not clear if the improvements in MOR activity seen with the electrode deposited platinum ruthenium on doped HOPG versus HOPG are due to the effect of the support or to different platinum ruthenium catalyst composition; i.e., X-ray diffraction (XRD) and/or elemental analysis was not presented. The stated purpose of the HOPG studies is for a "better understanding" of the catalyst-support interactions; however, understanding obtained from the HOPG results to date was not clearly stated.
- Nearly halfway through its duration, this project offers little performance data and no proof of the validity of the approach taken. Progress should be accelerated involving, among others, fundamental kinetic analysis under rigorous conditions.
- Data preceding the project inception should not be presented as an accomplishment (Raman spectra in slide 9 were already published in J. Mater. Chem, 2009, 19, 7830, before this project had even begun.).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- There are good collaborations on the program for the testing of the catalyst. The program is quite short and the team demonstrates good coordination.
- The partners include Mechanical Technology, Inc. (MTI) and BASF Fuel Cell, which are good for the catalyst scale up.
- The collaborators (outlined below) have great capabilities and that should greatly support the execution and validation and implementation of the program:
    - Develop novel catalyst-doped supports (the National Renewable Energy Laboratory (NREL) and Colorado School of Mines (CSM)).
    - Combinatorial electrode studies (the Jet Propulsion Laboratory (JPL) and NREL).
    - Generate downselected novel catalysts for DMFC MEA (NREL, CSM, and BASF).
    - MEA evaluation (NREL,CSM, and MTI).
- This teaming is impressive.
- The PI has assembled a very good team covering all aspects of the project's approach. The project is new, but has already demonstrated significant collaboration between CSM, JPL, and NREL.
- The team represents a good mix of two national laboratories, a university, and two FC developers.
- The role of MTI is unclear.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The continued combinatorial testing of the catalyst to optimize the performance is a good approach. It is important to move to a high surface area carbon support and to test in an MEA to determine to performance in an MEA and the durability under practical DMFC conditions, including both on time, start up and shutdown, as well as in the off state.

- As said above, the graphite has too many structures. Without selecting the best graphite, the project may go nowhere.
- There is a plan to extend the work to high surface area supports. Carbon nanotubes seem a logical choice as a well-defined surface for modification by implanting stabilized catalysts for making a high power methanol oxidation anode. The following three tasks seem to be most important:
    - 1) Initiate implantation of other dopants into HOPG (boron, sulfur, iodine).
    - 2) Continue combinatorial electrochemical investigation of various implanted HOPG substrates (type and extent of dopant) (JPL).
    - 3) Investigate different methods to dope carbon (in situ and ex situ) ion implantation, chemical vapor deposition, and pyrolysis.
- I think work on low surface area supports will give the most information at this stage. Alternative carbon supports are important and trying to stabilize platinum/ruthenium on inorganic supports (like molybdenum nitride supports) may be a good idea.
- The immediate proposed future work of characterizing the MOR of platinum ruthenium on doped high surface area carbon is good. The majority of the future work should focus on the high surface area carbon support, rather than HOPG, and on a scalable and cost effective catalyst deposition method.
- Proposed future research lacks a kinetic component that would help validate the approach used.
- By this time, HOPG should have been eliminated and replaced by viable carbons.

Strengths and weaknesses

Strengths

- NREL has a good sound approach to examining the approach for attaching the catalyst and determining the inherent performance of the catalyst. The project has moved quickly toward preparing to make high surface area catalysts, which are important to establishing the real benefits of the catalysis approach in terms of performance, potential costs, and durability.
- Graphite has better corrosion resistance, which is needed to improve catalyst lifetime. However, the researchers need to find a good graphite.
- Team skills and resources are the great strengths.
- NREL assembled a good team with a good approach to anchoring platinum ruthenium on support, providing better dispersions and preventing catalyst particle agglomeration.

Weaknesses

- The project needs to move toward determining the ability to produce high surface area catalysts, to demonstrate the indicated performance benefits that are realized in the MEA at acceptable loadings. The NREL team should determine the durability of the catalyst compared to the commercially available catalyst under the same MEA conditions, including a range of operating conditions including start up, shutdown and off state time (thermal cycling, dry out, etc.) .
- Graphite has low surface area and is well orientated, which could reduce the catalyst utilization in a real FC. Also, the binding force between platinum ruthenium with graphite is lower than platinum ruthenium on high surface carbon. Therefore, the platinum ruthenium may not stay on graphite long enough under FC operation.
- Methanol electro-oxidation catalysis failure mechanism is not known, which jeopardizes the project unless the team can clearly learn this mechanism.
- There is a lack of convincing rationale that doping the carbon support will address one of the major degradation mechanisms for platinum ruthenium anode catalysts, which is ruthenium leaching from the alloy.
- The electrochemistry part is weak; and the kinetic analysis is non-existent.
- Electrocatalytic benefits of the approach have not been shown yet.

Specific recommendations and additions or deletions to the work scope

- I recommend increased testing in MEA and if possible testing short stacks to determine the durability under a range of real operating conditions experienced in the predicted devices.
- Select the best graphite to do the later work.

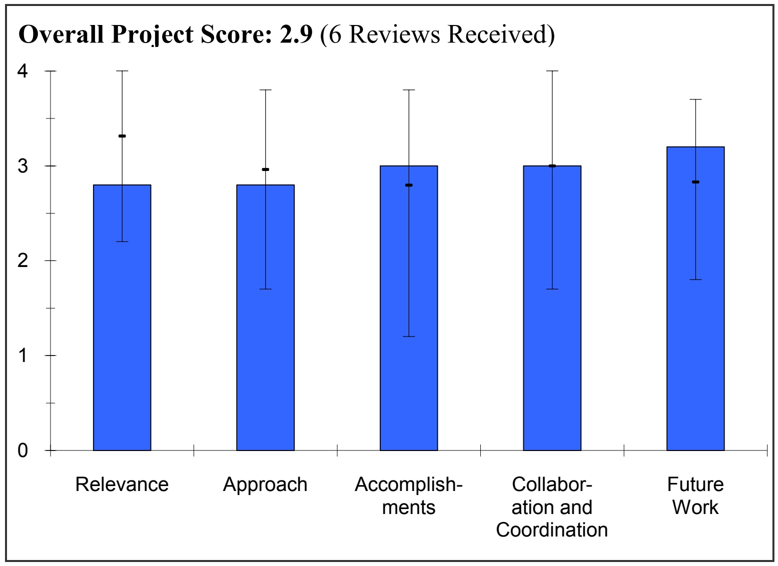
- I recommend work on a catalyst stabilized by ion implantation on low surface area supports, with careful characterization of the catalysis of methanol oxidation using the materials made in this project. The great benefit of this project would be to find the first stable and possibly high activity catalyst for methanol electro-oxidation.
- Add a project task aimed at demonstrating that the alloy composition of platinum ruthenium anchored to doped carbon supports is more stable than the same platinum ruthenium alloy with the same particle size anchored to a support that has not been doped. Down selection for one catalyst deposition process should be made halfway through the project duration, with criteria including scalability and cost of production of the catalyst.
- The validity of the approach has to be demonstrated in an FC as soon as possible using state-of-the-art techniques in MOR electrocatalysis as a reference.
- Catalyst performance should be optimized using practical carbon supports, not HOPG.
- Kinetic analysis of MOR on different catalysts is required. Cyclic voltammetry can be used for general screening of electrocatalytic properties, but steady state techniques should be used for kinetic analysis.

Project # FC-42: Advanced Materials for RSOFC Dual Operation with Low Degradation

Randy Petri; Versa Power

Brief Summary of Project

The project objectives are to: 1) advance reversible solid oxide fuel cell (RSOFC) technology in the areas of endurance and performance through RSOFC materials development and reversible stack design; and 2) meet the following performance targets in a kW-class RSOFC stack demonstration, a) RSOFC dual mode operation of 1,500 hours with more than 10 SOFC/solid oxide electrolyzer cell (SOEC) transitions, b) operating current density of more than 300 mA/cm^2 in both SOFC and SOEC modes, and c) an overall decay rate of less than 4% per 1,000 hours of operation.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- This project exactly fits DOE's objectives for residential hydrogen production and power generation.
- Reversible fuel cells may be part of DOE's new portfolio, but the performance targets proposed for this project are so low that its relevance is questionable.
- This is the most relevant project to DOE as it addresses renewable energy storage with mindfulness of high capital utilization.
- I don't see where this fits under the Multi-Year Program Plan (MYPP) currently on the EERE website (updated April 2009).
- RSOFC can integrate renewable production of electricity and hydrogen when power generation and steam electrolysis are coupled in a system, which can turn intermittent solar and wind energy into "firm power."

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- Reversible proton exchange membrane fuel cells (PEMFCs) have been well developed. Reversible SOFC has been demonstrated in single cells.
- This project focuses on performance and lifetime, which are the main hurdles for SOFC commercialization.
- Leveraging Solid State Energy Conversion Alliance (SECA) technology is a good start.
- The technical targets are too low. A reversible SOFC should have a lifetime target of five years minimum and ten years preferred, with an end-of-life performance of, at worst, 80% of the original. Therefore, decay should be approximately 0.5% per 1,000 hours.
- They propose 1,500 hours of operation with ten SOFC/SOEC transitions and to use this in energy storage. For energy storage they should be transitioning between SOFC/SOEC at least every 24 hours.
- They did not look at turndown performance. For integration with renewable sources, turn down will be important.
- Energy storage systems using hydrogen show that one would have an electrolyzer to make hydrogen when electricity is cheap and have a fuel cell (FC) or turbine to make electricity when it's expensive. In this paradigm, an energy storage facility would have to purchase two pieces of capital, and use each of them at a fraction of the time. However, in Versa Power Systems' (VPS's) technology, one would purchase a single piece

of hardware and perform both functions with it, thus utilizing capital to a much larger extent. Capital utilization is one of the most critical aspects to favorable economic performance.
- The project is appropriately focused on degradation mechanisms, with quantitative targets. The experimental approach is sound, building on SOFC-mode progress to date. The team utilizes intra-cell voltage measurements and transmission electron microscopy (TEM) to good effect. They have solid testing protocols.
- Developing high performance and low degradation RSOFC cell and stack technology is critical for the reversible SOFC/SOEC system. This is the focus of the work.
- The approach is excellent and includes building on VPS's strong SOFC cell and stack baseline, leveraging cell and stack advancements from the DOE-SECA-SOFC project, addressing RSOFC degradation mechanisms in SOEC mode with innovative cell and stack repeat unit configurations, conducting parallel materials development activities and integrating them with cell production technology development, and completing RSOFC stack and process designs to address durability, performance, and cost in both SOFC and SOEC operating modes.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Technical progress as shown is the SOFC test under electrolysis mode. All of those tests showed the degradation, even under steady state conditions. No thermo-cycles were reported. Thermo-cycle is the main challenge for the durability. Without solving the fundamental thermo-cycle durability problem, the scale-up may not be useful.
- Their performance is an improvement over Idaho National Laboratory's (INL's) high temperature electrolysis work.
- The long-term testing showed a degradation rate of 3.8% over 1,000 hours, meeting their milestones, but that degradation rate is still >7 times too high for a commercial system.
- There was no discussion on degradation mechanisms. From the discussion they have some studies in this. It would be nice if they presented the details.
- There was no discussion on materials development.
- The number of cycles is too low.
- They need to report their round-trip efficiency.
- They need to look at turndown operation for both the SOFC and SOEC.
- Outstanding development of better materials to provide higher performance and lower degradation rates.
- The project is solidly ahead of schedule, in part due to the initial lack of SOEC mode data upon which targets and schedule would be established. Nevertheless, combined degradation, SOEC-SOFC transitions, and operating time demonstrate significant progress.
- Two types of RSOFC cells developed have met the electrochemical performance target, and RSOFC; four met both performance and degradation criteria.
- A steady-state single cell test has run in electrolysis for one year with a degradation rate of less than 3% over 1,000 hours.
- A baseline 28-cell stack (kW-class) test has run in electrolysis for over 2,500 hours at 3.8% over a 1, 000-hour degradation rate.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Their partners include Boeing, SECA, and INL. They did not show what Boeing did or what has been done by SECA.
- They have a large number of "collaborators," but it seems that most of the collaboration is VPS applying technology developed on other projects, such as SECA's and the Defense Advanced Research Projects Agency's (DARPA's) work to this one.
- The company has communicated well with its customer base to drive toward a product set that would land them in a very useful market space.

- This project builds upon established SECA technology and concurrent Boeing RSOFC work. VPS has excellent in-house personnel and equipment for the proposed work.
- Great leveraging: Boeing: collaborated on and funded the initial RSOFC development work through both Boeing and DARPA funded efforts, and anticipated follow-on a DARPA award this calendar year. SECA: as subcontractor to FuelCell Energy in SECA, VPS has advanced SOFC cell and stack technology which has been applied in this program. INL will integrate SOEC technology for hydrogen production with Next Generation High Temperature Nuclear Reactor, and demonstrate suitability of VPS SOEC technology for this application at the kW-class stack level.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The proposed work only shows the electrochemical degradation tests without thermo-cycle tests. If the RSOFC failed on several thermo-cycles, the electrochemical degradation tests might not need to occur.
- The future work does address the core issues of degradation.
- Good as proposed—see comments under scope additions below.
- For fiscal year (FY) 2010: This is an excellent plan building on progress and accelerating RSOFC stack development: Complete degradation mechanism study. Conduct single cell tests at various operating conditions (temperature, current, steam utilization). Conduct post-test analysis with detailed microscopic analysis; i.e., TEM, scanning electron microscopy (SEM), and energy dispersive X-ray spectroscopy (EDX). Complete test facility improvements. A potential additional scope could be conducting additional stack testing early in the project.
- For FY 2011: Complete go/no-go decision point test. Complete cell and interconnect materials development. Down-select material systems for RSOFC stack development. Complete the final project metric test with kW-class RSOFC stack.

Strengths and weaknesses

Strengths
- The high-temperature electrolyzer may have better electrochemical efficiency.
- A reversible SOFC for energy storage has the potential for high capital utilization.
- Solid technology that demonstrated progress.

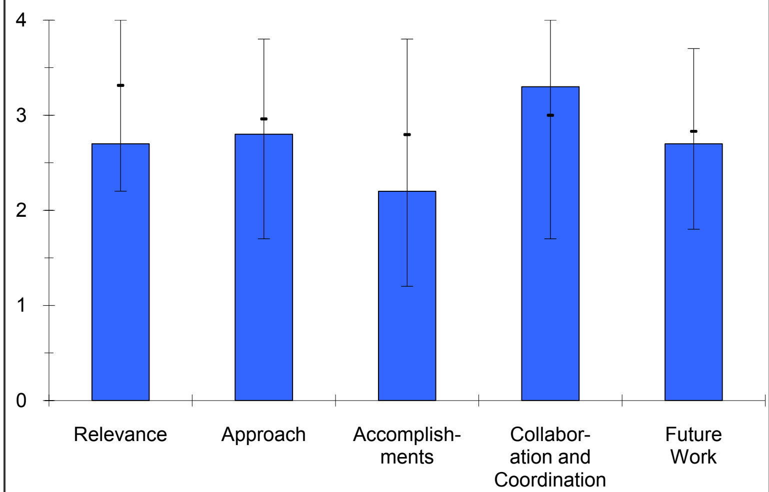
Weaknesses
- Thermo-cycle needs to be addressed.
- The targets are too low and need to be increased as discussed above.
- INL has a great deal of understanding on degradation mechanisms for SOEC. VPS is not using that knowledge.
- Work such as this should always reference a systems study or analysis to show the reason for pursuing the work. Yes, one can certainly envision an energy storage system based upon RSOFC; however, a cost entitlement analysis relative to existing and advanced concepts would establish a basis for the work. Cost is always a consideration.

Specific recommendations and additions or deletions to the work scope

- Test the thermo-cycle before the electrochemical cycle.
- They need to make the degradation target at most 0.5% per 1,000 hours.
- They need to do real cycling experiments.
- They should define what the roles of the participants are and clearly state what each participant is doing.
- They need to discuss materials development.
- They need to include turndown experiments.

Project # FC-43: Resonance-Stabilized Anion Exchange Polymer Electrolytes
Yu Seung Kim; Los Alamos National Laboratory

Overall Project Score: 2.6 (6 Reviews Received)



Brief Summary of Project

The objectives of this project are to: 1) develop anion exchange polymer electrolytes that have high hydroxyl conductivity and stability in high pH conditions, and 2) demonstrate an improved single cell performance of solid-state alkaline fuel cells using the polymer electrolytes and non-precious metal catalyst.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- It is not clear how this project fits with the overall DOE objectives for the Hydrogen Program. The PIs are targeting the alkaline membrane system, but the performance targets look too low.
- It was unclear what application the project would be relevant to. If the alkaline membrane fuel cell is for consumer electronics, then the DOE technical targets for this application may apply. If it is for transportation application, then the fuel cell performance from this approach would not be relevant since anion exchange polymer electrolytes cannot achieve the technical targets. I think that it would be difficult for this approach to achieve even the consumer electronics technical targets.
- The alkaline membrane conductivity, stability, and electrode processing issues are worth studying.
- The conductivity technical target set for the project does not seem significant since it is set at 80°C and the recently reported conductivity values are for 20° and 50°C. It is expected that higher conductivity would be achieved at a higher temperature.
- Alkaline-membrane fuel cells have several advantages, but also several challenges. They could reduce fuel cell cost and enhance performance.
- Finding electrolytes that allow the use of active inexpensive materials for hydrogen and air fuel cells is important to the practical use of fuel cells.
- PIs will develop anion exchange polymer electrolytes that have high hydroxyl conductivity and stability in high pH conditions.
- PIs will demonstrate improved single cell performance of solid-state alkaline fuel cells using polymer electrolytes and non-precious metal catalysts.
- This project is an innovative way to find if stable alkaline membranes can be obtained to achieve these goals, namely, > 50 mS/cm at 80°C, and > 500 h at 80°C in 1 M NaOH solution at > 200 mW/cm^2 H_2/air at 80°C.
- I think characterizing membranes in 1 M aqueous KOH is a good first start, but the effects of water activity should be addressed. It may not be important to do address water activity at first, but when a good electrolyte system is found in 1 M aqueous potassium hydroxide, it is relevant to practical systems to scrutinize the effects of decreasing water content and fuel cell-gas humidification.
- This project is for an alkaline fuel cell, which is a peripheral part of the Fuel Cell Technologies Program.
- Alkaline membrane approach may allow for the use of non-precious metal catalysts.
- Supports targets proposed to DOE are: conductivity at > 50 mS/cm at 80°C, stability at >500 hours at 80°C in 1 M NaOH solution, and electrode processing at > 200 mW/cm^2 H_2/air at 80°C.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The in-cell performance targets seem too low. It is good that the team is working on the key ionomer for the electrode to provide sufficient oxygen access and stability.
- The project is interesting and addresses several issues that are relevant for alkaline membrane fuel cells.
- It seems that many new tasks need to be completed within a limited time and with limited funding. The team is trying to develop an anion exchange membrane with higher conductivity and stability, non-precious catalysts for the electrode, optimize the electrode with new catalyst, membrane and ionomer, and fabricate membrane electrode assemblies (MEAs) for fuel cell testing. These are not trivial tasks by themselves. If successful, it could have some impact and have demonstrated some novel concepts. The resources may be spread too thin to achieve significant progress in all areas.
- It is also unclear what percentage of the effort and in what time line will be dedicated to membrane development, non-precious catalyst development, electrode optimization and MEA fabrication. This information would be helpful to determine the degree of difficulty of each task and whether appropriate effort is dedicated to the task.
- The project targets stability, one of the main challenges for the alkaline membrane fuel cell.
- A major degradation route of anion exchange polymer electrolytes is nucleophilic substitution (SN2).
- The project's approach is to make resonance stabilized guanidine functional group in a polymer. This approach should reveal if SN2 is a major degradation route for anion exchange polymer electrolytes and if the resonance structure of guanidine base enhances the cation stability and hydroxyl conductivity or not.
- The approach is very interesting. It is focusing on low cost materials that would enable solid state alkalines. However, most materials in existing alkaline fuel cells are already cheap. The big problem is CO_2.
- Next to reacting with the alkali, alkaline fuel cells historically are lower in power than polymer electrolyte membrane fuel cells (PEMFCs). Their approach may improve the power.
- One of the objectives is to develop a non-precious metal catalyst.
- Approach is very sound and has a contingency plan.
- The two approaches, stability and electrode performance, are correctly identified.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- The project does not appear to be making much progress on obtaining a viable ionomer for the electrode that provides sufficient stability. The performance targets for the stated vehicle application look too low even when taking into account the potential lower catalyst costs that can be realized from the use of non-precious metal catalysts.
- It is a new project, so limited but good progress has been achieved with membrane development.
- Apparently the membrane is not stable even under hot water. Did not see the data from leachants. The proposed degradation mechanism is valid; however, if the electron withdrawing decreases (hydrocarbon guanidine base), it might change the pH of the membrane or reduce the OH^- conductivity.
- This is a relatively new project (September 2009 start).
- Amine and guanidine polymer materials have been made.
- Methods set up to track membrane composition and performance to validate if S_N2 is a major degradation route for anion exchange polymer electrolytes and if resonance structure of guanidine base enhances the cation stability and hydroxyl conductivity or not.
- Finding the effects of electron density on the stability is impressive and should be followed up in guiding new materials.
- The improvement in conductivity is a good step.
- The new membranes show promise, but it would be good to see more conductivity and operation data.
- The stability was disappointing, but the PI has ideas on how to improve it.
- One of the objectives is the development of non-precious metal catalysts, but this topic was not discussed in the presentation.
- Significant progress has been made.
- The project started in September 2009. There have been few accomplishments. Conductivity of the perfluorinated guanidine base in boiling water was disappointing.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- There are limited significant ongoing collaborations. There are a large number of stated occasional collaborations. It was unclear if these were well coordinated to help to address the issues. Increased collaboration is needed to develop the electrodes, improve performance, and ensure that the goals are targeted at the DOE Hydrogen Program objectives for performance and cost.
- The collaboration and partners seem appropriate.
- A very strong team.
- The team is skilled in all aspects of the project and should have all bases covered to do the work.
- There are a lot of partners that are contributing to the work.
- Excellent collaborations.
- Outstanding.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- It is important to address the durability and performance of the ionomer if this system is to be practical. More effort may be needed to develop the ionomer and the catalyst to optimize the performance and potentially address the cost and performance objectives for the intended hydrogen / air application.
- It was unclear what non-precious metal catalyst will be studied. What if no good performing non-precious metal is found via high throughput electrochemical screening? Will a Pt catalyst be used instead to achieve the targeted performance?
- The backup plan to use nanostructured thin film (NSTF) materials in electrodes if an appropriate ionomer is not found may or may not be appropriate. The idea is that NSTF materials do not need an ionomer in the electrode. However, the non-precious metal catalysts that will be sputtered onto NSTF materials have to be good ion conductors like Pt.
- There was no time line or decision points specified.
- Before solving the stability issues, MEA fabrication may not be necessary. Also, membrane stability should be tested under hydrogen and oxygen saturated water at elevated temperatures.
- The work is based on prior knowledge of past alkaline membranes and has a hypothesis to improve stability (that is, cation stability based on electron donating spacer), which has long eluded workers trying to make stable anion exchange membranes.
- The future work focuses on the appropriate areas.
- Since so much development needs to occur on increasing the membrane stability, testing in an FC would be premature at this time. But it needs to be done eventually.
- Very good future work plan.
- Focus is polymer chemistry. Suitable plan.

Strengths and weaknesses

Strengths

- Good polymer synthesis expertise. Targets are for a good, potential technology that may address catalyst cost issues.
- The issues being pursued are worth addressing and the general approach is fine.
- The membrane shows CO_2 tolerance with promising cation conductivity. The alkaline fuel cell will drastically reduce the precious metal usage in the fuel cells.
- A great strength is that the team is quite knowledgeable on the subjects of alkaline membranes and technology and methods to characterize materials and material performance.
- Strong team.
- Innovative idea.

- A lot of work can be done on improving alkaline fuel cells. Compared to PEMFC R&D, there is not a lot of work being done in this area.
- Has the potential for alkaline fuel cells that do not use precious metal catalysts.
- Interesting project in polymer chemistry. Excellent technical work just beginning.

Weaknesses

- Projected performance goals at the MEA-level seem too low for the intended vehicle application. Given the lower performance targets, the project does not provide any information about the potential cost improvements that the membrane will provide in terms of lower catalyst costs for the fuel cell system.
- The project tries to tackle too many difficult issues with limited resources to achieve them.
- Relevance is not clear.
- The stability is not very good.
- Alkaline catalysis is well known to give fairly stable high power to fuel cells with a hydrogen anode and an oxygen cathode. However, alkaline fuel cells are not well understood with cathodes running on air with carbon dioxide and other impurities.
- It needs to be appreciated that catalysis work to date is really short term, and electrolyte and catalyst stability in air must be questioned.
- The alkaline fuel cell is CO_2-sensitive. The PIs may develop the best membrane and great non-precious metal catalysts, but unless they address the CO_2 issue, the commercial viability of this work will be limited.
- There is a significant probability that anionic conductivities and stabilities in alkaline solutions of the membranes will not be suitable for applications.
- Should use non-precious metals from the beginning.

Specific recommendations and additions or deletions to the work scope

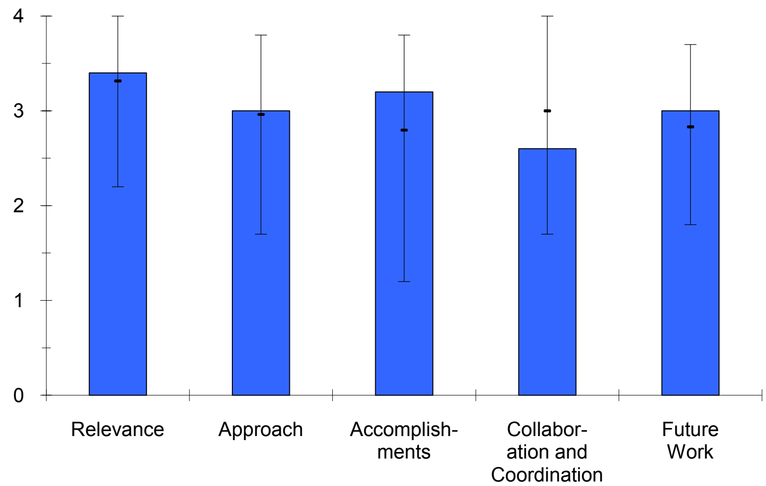
- Increase MEA testing and the involvement of collaborators to ensure the cost and performance targets are aligned with the intended application.
- Focus on 1-2 tasks or issues rather than all of the issues or tasks presented.
- Improve the membrane stability before putting it into a fuel cell.
- The project is just underway. Membranes based on the team's design criteria should be made as soon as possible for testing.
- It may help to try to model studies by making small molecules of a stabilized guanidine and mixing with 1M aqueous KOH base and to characterize performance in a 3-electrode cell as a function of water activity.
- Recommend testing in a fuel cell earlier than what they currently plan. Many materials that look good in characterization tests perform poorly in a fuel cell.
- The PIs need to test with air (including CO_2) in addition to the O_2 testing.

Project # FC-44: Engineered Nano-scale Ceramic Supports for PEM Fuel Cells
Eric L. Brosha; Los Alamos National Laboratory

Brief Summary of Project

The overall objective of this project is to develop a ceramic alternative to carbon material supports for a polymer electrolyte membrane fuel cell (PEMFC) cathode. Ceramic supports must: 1) have enhanced resistance to corrosion and Pt coalescence; 2) preserve positive attributes of carbon such as cost, surface area, and conductivity; and 3) be compatible with present membrane electrode assembly (MEA) architecture and preparation methods. Materials properties goals include: 1) high surface area, 2) high Pt utilization, 3) enhanced Pt-support interaction, 4) adequate electronic conductivity, 5) resistance to corrosion, 6) synthesis method/procedure amenable to scale-up, and 7) reasonable synthesis costs.

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project evaluates alternates to the carbon-supported catalyst types currently in used in PEM electrodes. The evaluations are worthwhile in that corrosion of the cathode supports in a low fuel scenario is a key cell failure mode. A catalyst support that is more corrosion-resistant than the currently used supports, with the same degree of hydrophobicity, should result in a more durable cell.
- This review has adequately covered and summarized the topic.
- Cost and durability are the most critical gaps for fuel cell commercialization, and catalyst durability is one of the most important factors for these critical attributes. Alternative support materials may solve carbon-related materials stability problems.
- Development of high surface area supports alternative to carbon materials is indeed important, as demonstrated clearly by the development of 3M NSTF catalysts. In this project, the PIs have proposed to develop a low cost durable ceramic support that will enhance resistance to corrosion and attenuate Pt agglomeration. Although the development of ceramic supports with such characteristics appear to be feasible, this project needs more convincing data that this type of support will have any benefits to the existing carbon- or 3M-type supports.
- This project is aimed at developing alternative supports for Pt-based catalysts to improve stability of cathode catalysts. While this approach addresses cathode degradation due to support corrosion at high potentials and may also address Pt particle coalescence, it does not address Pt dissolution as was stated in the presentation.
- Supports DOE technical targets.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach appears novel.
- Modeling approach part is not clear. Is this a molecular dynamics to develop a Pourbaix diagram?
- The project is relatively well designed and is based on the required stability and conductivity of supports in PEMFCs. The methods for characterization are also reasonably well chosen, so it appears that it will be possible to establish the important stability and activity properties of such systems. The PIs need to be more specific

about how they, in fact, plan to determine the adsorption sites for Pt particles as well as the methods to follow oxide-induced dissolution of Pt.
- Why do modeling on stability without the presence of catalyst? Catalysts are known to accelerate the processes contributing to support degradation.
- The approach is correct—focus on select support candidate materials using various synthesis methods.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The accomplishments appear to correspond with expectations.
- Current data seem to be promising.
- The project is in its early stages, so it is very difficult to judge real technical accomplishments. Nevertheless, some of the materials synthesized based on Mo-N compounds show promising characteristics, such as exceptional surface area electronic conductivity and stability. In my opinion, this is the better part of the project. However, electrochemical measurements, in particular for the oxygen reduction reaction (ORR), are clearly questionable and need more to be done to make any meaningful conclusions about catalytic activity. Based on polarization curves for the ORR on Pt, it is not clear why the activities are well below the benchmark activities observed for the ORR on Pt catalysts supported on carbon. In addition, for samples prepared at 700°C, a clearly visible peak in the cyclic voltammogram (CV) at ~ 0.4 V may indicate that Mo is still “active,” and that this temperature is not the best choice for the sample preparation. The PIs are claiming that they have the evidence for enhanced Pt interaction with Mo_2N support. The 300°C temperature may not be high enough to have any effect on Pt agglomeration; therefore, more careful experiments need to be performed to come to such a conclusion.
- The bulk of data shown was for the Mo_2N system. Good progress has been made in getting this material to a high surface area form. Initial and preliminary data were shown for ORR activities of Pt deposited on high surface area Mo_2N. Though the presentation states that the ORR activity is comparable to commercial (E-Tek) 20 wt.% Pt/C, the raw rotating disk electrode data showed problems with achieving a flat limiting current region over the potential studied. Without a steady-state limiting current, it is impossible to evaluate the ORR kinetic current and thus impossible to make the claim of comparable activity. It is also premature to say that there's evidence for enhanced Pt/support interaction for Mo_2N versus carbon. Also, a better method for determining electronic conductivity of support in powder form should be developed—the two point probe resistance method of the foam is not reliable or quantifiable—and may be skewed by presence of carbon in foam.
- On-schedule to meet all milestones:
  - Synthesis of alternative ceramic supports pushed ahead of schedule.
  - BET analysis of Mo_2N cubic phase indicates exceptional surface area.
  - Pt/Mo_2N CV characterization with 0.5 M solution of H_2SO_4 shows Pt/C-like activity, which is good.
  - Evidence for enhanced platinum interaction with molybdenum nitride support.
- In short, the Mo_2N ceramic support is a strong candidate with high surface area, electronic conductivity, and stability. Evidence for enhanced Pt-support interaction with Pt/Mo_2N, electrochemically active surface area, and demonstrated ORR activities.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- Collaboration appears to be limited to members on the contract. There does not appear to be any commercial involvement to act as a sounding board and provide guidance toward specific applications.
- For the scope of this project, three partner organizations may not be sufficient. The systems chosen to be studied are rather complex and addition of carefully selected theoretical methods may help in the quest to design a new generation of non-carbon supports.
- This project relies mainly on expertise at LANL. The project has a subcontractor with the University of New Mexico, but it was not evident from the presentation that they had done anything yet (problems in establishing subcontract). Once the subcontract is in place, the project would benefit greatly from the XPS characterization capabilities at the University of New Mexico (UNM), which was not included in the description of the tasks.

- Good collaboration.
- Good interaction among UNM, LANL and ORNL.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The future work appears to be more toward lab techniques for deposition. Is there any thought to scale-up techniques?
- In addition to the current project scope, it would be good to investigate catalyst performance with respect to support materials (interactions) and to understand mechanism of performance enhancement with support materials.
- In general, the proposed future work appears to be reasonable, at least for the synthesize part. I think the project needs much more aggressive electrochemical characterization.
- The proposed future work should include developing a reliable method for quickly screening the electronic conductivities of the newly-developed support materials and for determining their corrosion resistance in the presence of catalyst. The proposed future work should also include an evaluation of the cost effectiveness of the proposed materials and processes for making them. Fast-tracking the go/no-go decision and downselecting classes of materials early in the project is a good idea.
- Future work is logical extension.

Strengths and weaknesses

Strengths
- The strengths are the technical expertise of the researchers.
- The major strength of the project is the unique ability of PIs to synthesize and characterize different ceramic type supports, which might be of potential interest for the Hydrogen Program. The PIs are qualified for this work, but they have to demonstrate that the kind of support they are proposing may eventually compete with the existing carbon supports and especially with 3M NSTF systems.
- The strength of the project is the ability of the LANL team to make high surface area ceramic supports, as one of the major limitations to using the conductive ceramics as supports in catalysis is their low surface area.
- This is important work. Some substrate support development should go in this direction.
- Very promising.

Weaknesses
- The weakness appears to be the lack of commercial guidance on applications.
- Electrochemical characterization is an apparent weak point of this project. The electrochemical results must be more convincing and in line with the state-of-the-art PGM catalysts supported on carbon and/or attached to the 3M supports. Given that this is just a first year of the project, there is a plenty of time to overcome these problems.
- Weaknesses of the project are the lack of experimental evidence behind the claims of improved properties of the ceramic supports versus traditional carbon supports, though it is early in the project. The ultimate proof of the concept is the performance and performance durability in a fuel cell and MEA fabrication and testing is part of the planned near-future activities. However, as LANL knows quite well, it is not straightforward to fabricate high-performing MEAs with a new material set. It would, therefore, speed progress in the downselection of promising materials if quick, reliable, quantifiable, *ex situ* methods were identified for determining important materials properties such as electronic conductivity.
- PI needs to optimize structures at some point.

Specific recommendations and additions or deletions to the work scope

- In addition to the current project scope, it would be good to investigate catalyst performance with respect to support materials (interactions) and understand mechanism of performance enhancement with support materials.
- Need better method for determining electronic conductivity of support materials. Is carbonaceous material responsible for two-point conductivity observed for nitride support? Add TGA in the presence of catalysts to

determine mass loss and degradation of catalysts in a humidified air atmosphere. Add an evaluation of cost effectiveness and scalability of materials and materials synthesis techniques. Refine RDE technique / catalyst deposition on disk to obtain data that can be used to evaluate and compare kinetic currents.

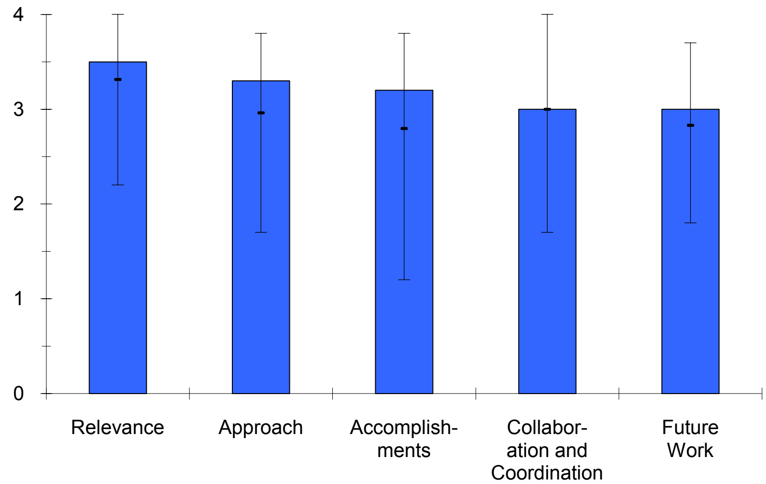
Project # FC-45: Effects of Fuel and Air Impurities on PEM Fuel Cell Performance

Fernando Garzon; Los Alamos National Laboratory

Brief Summary of Project

The overall objective of this project is to lower the cost of fuel cell operation by improving performance and increasing lifetime. The objectives are to: 1) understand the effects of fuel cell operation with less than pure fuel and air and simulate "real world" operation, 2) understand how impurities affect DOE fuel cell cost and performance targets, 3) contribute to the scientific understanding of impurity fuel cell component interactions and performance inhibition mechanisms, 4) develop science-based models of impurity effects upon fuel cell performance, 5) perform experimental validation of models, and 6) develop mitigation strategies and methods.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The generation of data on reactive compounds that could be in air is a valid area of research. It is rational to be examining compounds that might be found in the fuel stream and could diffuse across the membrane electrode assembly (MEA) to the cathode.
- The project's objectives of understanding durability and the performance effects of key impurities are well aligned with the DOE Program objectives. The stated objective of forming mitigation strategies for the impurities is also well aligned with improving fuel cell performance, durability and performance.
- Understanding the impact of impurities on the fuel cell operation is critical for the FCT program.
- Understanding impurity effects on the rates of fuel cell reactions is of paramount importance; therefore, this project is (potentially) critical to the Hydrogen Program.
- This project identifies and addresses critical path barriers of cost, durability, and performance.
- In general the group did an outstanding job. They focused on highly relevant materials and investigated useful operating conditions. The project was very informative.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The approach is valid. For example, according to the EPA (2002), SO_x concentration in a number of U.S. cities is in the range of 10 to 15 ppb. In Europe, this number can be 4 to 5 times greater.
- The approach to understand the effect of the key impurities is both sound and thorough. The testing seems to be well designed and targeted at improved understanding of the behavior of the key impurities.
- The single component impurity testing is well thought out.
- The PIs need to include mixtures of impurities to understand their impact. Perhaps a design of experiment statistical analysis approach would be useful to be able to determine and model the impact of mixtures without having to test every mixture composition.
- The project is relatively well designed and is based on utilization of variety of analytical methods, surface sensitive techniques, and modeling. In my opinion, the addition of a classical rotating disk electrode (RDE) method will be very useful in unraveling the true effects of sulfur poisoning of active catalyst sites supported on

high surface area carbons. In addition, careful measurements with bulk-like polycrystalline electrodes will be very useful to resolve many puzzling issues associated with Pt-S interactions and poisoning effects on the oxygen reduction reaction (ORR).

- The proposed objective is in line with overall DOE program objectives (NH_3, sulfur compounds), and some impurities studied seem to be of minor relevance (NO_x). More input or guidance from industry may identify impurities of more importance.
- This was a really nice project with an excellent approach.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The accomplishments are as expected for this point in the investigation.
- The team has made good progress on the understanding of the effects of the impurities individually and in interaction with the fuel cell operating parameters, in particular the effects of ammonia and its interaction with water. There is little discussion or progress against the stated objective of developing mitigation strategies.
- LANL has made good progress in understanding the impact of impurities.
- The results are a good beginning. The team needs to continue with this work and expand it to understand mixtures.
- Since this is the end of a well-funded project, more results were expected. LANL should present a little of the model results and validation.
- The project is in its final stage, so the accomplishments must be rather obvious, which appears they are. The project clearly points to various degrees of contamination of platinum group metal (PGM) catalysts and membranes, which in turn have a strong effect on the polymer electrolyte membrane fuel cell performance. Although it was clearly demonstrated that both SO_2 and NO_2 have a negative effect on the activity, it is not clear how the adsorbed species are affecting the performance. The surface chemistry of NO_2 is rather rich and the nature of N-adsorbed species may depend strongly on the applied potential. Much more fundamental work is needed to resolve these complexities. The same applies for both the effects of NH_4^+ and other cations present in the membrane. In my opinion, more fundamental work is required to fully understand electrochemistry at three phase interfaces.
- Ammonia and sulfur compound results are important.
- Outstanding technical accomplishments.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The noted collaboration looks to be closer to a list of subcontractors or vendors. It is suggested that this data be discussed with John Van Zee of South Carolina and Scott Greenway of Savannah River National Lab. Discussion of this data plus the data they generated as part of the hydrogen purity research might help pull together a clearer picture of NH_3 poisoning.
- The project has good collaborators for the examination of the key impurities and their effects on the performance and durability. The collaborations appear to be well focused on the detailed fundamental understanding of the poisoning effects of the impurities and the interaction with the fuel cell operating parameters.
- They have good collaborations and spread the work around appropriately.
- One would expect more publications from this work from the results and the funding level than what they reported.
- This project is multifaceted, and as such requires a close collaboration between the PIs. Eight partner organizations with highly complementary skills are put together in catalysts development, electrode-structure design, materials characterization, and catalyst fabrication. The degree of collaboration is indeed outstanding and certainly a big part of success of the entire project.
- More collaboration with industry is needed for focused direction.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The future work appears to be appropriate. Using EPA air quality data may help bound the expected exposures. Similar data from major cities worldwide would be helpful also.
- The future work looks good for addressing further understanding of the effects of the impurities. More focus on mitigation strategies and understanding them would be helpful.
- The future work seems appropriate, but should be expanded to include mixtures of impurities.
- They need to continue model refinement and validation.
- In general, the proposed future work appear to be reasonable, at least for the synthesize part. I think the project needs much more aggressive electrochemical characterization.

Strengths and weaknesses

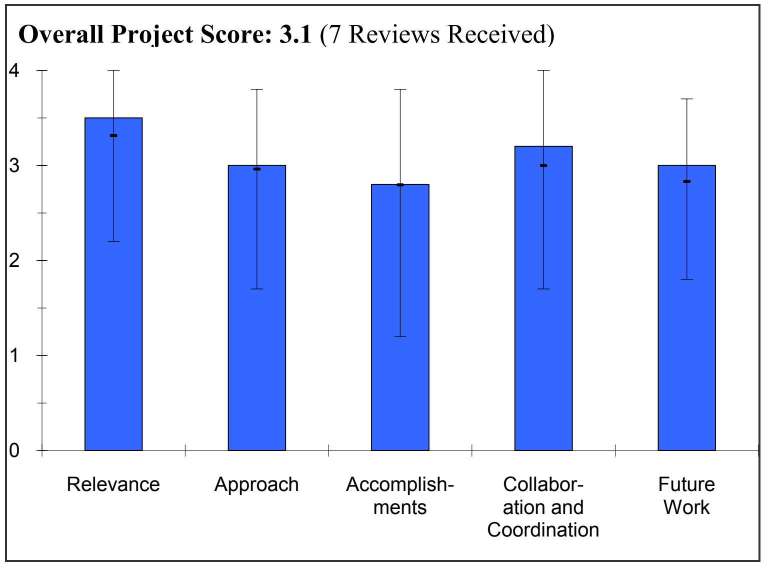
Strengths
- The strengths of this effort appear to be the researchers' knowledge and techniques.
- Good approach to understanding the fundamental effects of impurities on the performance and durability and their interaction with water in the fuel cell.
- Strong teams. They have the right tools to perform the analysis.
- The major strength of the project is unique ability of PIs to synthesize and characterize different ceramic type supports, which might be of potential interest for the Hydrogen Program. The PIs are qualified for this work, but they have to demonstrate that the kind of support they are proposing may eventually compete with the existing carbon supports and especially with 3M NSTF systems.
- Overall, this is an excellent project and has produced very useful data.

Weaknesses
- An apparent weakness is the lack of the team availing themselves to the synergy of the hydrogen quality researchers.
- No reported work on the development of mitigation strategies for these impurities to improve the durability.
- Electrochemical characterization is an apparent weak point of this project. The electrochemical results must be more convincing and in line with the state-of-the-art PGM catalysts supported on carbon and/or attached to the 3M supports.
- Short list of collaborators.

Specific recommendations and additions or deletions to the work scope

- The PIs need to look at chlorides.
- More work on understanding mitigation strategies for the impurities to improve the fuel cell durability.
- They need to look at mixtures.
- The researchers should publish more of their work (they have some publications, but with the amount of research performed more publications were expected).
- DOE should consider expanding the impurities to include ones that people do not often think about such as the off gases from fresh blacktop and chlorides.

Project # FC-46: Effects of Impurities on Fuel Cell Performance and Durability
James Goodwin; Clemson University



Brief Summary of Project

The overall objectives of this project are to 1) investigate in detail the effects of impurities in the hydrogen fuel and oxygen streams on the operation and durability of fuel cells, 2) determine mechanisms of impurity effects, and 3) suggest ways to overcome impurity effects.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project addresses two principally different contamination processes that are both key for fuel cell performance: the impact of contaminants on catalyst performance, and the impact of contaminants on membrane conductivity
- The effect of impurities on membrane performance is of great importance if fuel cell technology is to become practical.
- The study of impurities is an essential topic in moving fuel cell technologies successfully to the marketplace.
- This project to investigate in detail the effects of impurities in the hydrogen fuel and oxygen streams on the operation and durability of fuel cells is relevant no matter what membrane is ultimately used in PEM fuel cells.
- Comprehensive analysis on impurity effectiveness is important for standardization and development of risk mitigation strategy.
- Addresses long term durability due to impurities, both on the air side and fuel borne.
- Topic area is extremely important to fuel cell performance and durability in real-world environments; however, the project could be even more relevant if it were to focus on the study of contaminants not already covered by past and existing projects.
- Understanding the effects of impurities and the development of mitigation strategies are critical to the advancement of fuel cell technology. However, it is unclear that new observations and insights have occurred through this project.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The PI intends to investigate a large number of contaminants. Each contaminant as well as contaminant mixtures are studied in a very short time frame of 3 months only. These studies are likely to be incomplete and may leave many questions unanswered. Contaminant effects are typically strongly affected by operating conditions and the investigation requires a significant amount of time at realistic contaminant concentrations. Furthermore, no information was given on how the correlation between *ex situ* phenomenological studies and fuel cell studies will be established.
- The introduction of known impurities and careful characterization of their short and long-term effects in fuel cell performance and membrane conductivity is a good approach to learning the effects of impurities.
- Empirical correlation of impurity concentration to performance is important. Formulating mechanisms of impurity effects is important and there have been some trends developed from empirical correlations to develop mechanisms of impurity effects (cations, ammonia and water content on conductivity, surface poisoning to fuel

cell performance, etc.). The project started in 2007, so I would have expected more explanation of mechanisms of impurity effects. Maybe this is forthcoming.

- It is necessary to identify how to distinguish performance degradation by impurity effects from materials degradation by fuel cell operation.
- PI is focused on both near and long term. Correlation experiments between SRNL and Clemson are relevant for comparison of data sets. The approach, as outlined, covers both component level and system level effects and feeds forward to mitigation of detrimental conditions.
- The majority of the contaminants studied in this project have been studied in previous projects. Due to the importance of this topic area to the ultimate success of fuel cells in real-world applications, the effect of understudied contaminants should have been the focus of this project. The effect of Na^{+}, Ca^{2+}, and Fe^{3+} have already been studied. The effect of CO on MEA performance has been extensively studied in the past. The effect of furan, hydrocarbons, and perchloroethylene are good selections as, to my knowledge, they have not been studied at all or have not been adequately studied in past work.
- The overall project objectives are too broadly stated as "investigate effects of impurities," determine mechanisms, and overcome impurity effects." The objectives for FY 09-10 are more specific, but the choice of some of the study subjects seems repetitive with past work and other ongoing projects. Also, the work seems disjointed compared to the very broadly stated goals.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **2.8** based on accomplishments.

Ex situ studies on CO only confirmed existing knowledge. The benefit of these studies and the meaning of the received numbers are unclear. Some explanation and/or clarification of whether these numbers are useful for a modeling effort would be helpful.

- Understanding how an increase of the ionic conductivity of the catalyst layer affects the overall resistance of the fuel cell is very important, specifically if it allows one to detect such an effect within the fuel cell. It could also be beneficial to see if *in situ* diagnostics such as electrochemical impedance spectroscopy could detect such an effect.
- Cleaning agents have been identified as potentially important contaminants that may find their way into a fuel cell system. In this study, however, the investigation of cleaning agents seems to be somewhat disconnected from the other studies.
- The presented mitigation studies are not thoroughly discussed. In the case of RH, the PIs create conflict since they suggest a higher RH for one contaminant and a lower RH for another. Obviously you cannot run both these conditions at the same time, although it is likely both contaminants are in the same feed stream.
- For the limited impurities and feed streams studied to date (CO, water, cations, organics like THF, perchloroethylene, ammonia) the team has well characterized impurity effects and shown good understanding of these effects.
- It is good to identify implication of fuel cell operating conditions (humidity).
- Impressive accomplishments in both basic studies of poisoning mechanisms and in the bottom line effect of poisons on measured fuel cell performance and performance recovery. The scope of the number of potential poisons, the importance of this issue, and the limited time to achieve the DOE technical targets indicate that a more prudent use of project resources would be to focus on contaminants that have not been studied and focus on combinations of contaminants, especially airborne contaminants.
- Listed below are comments on each major area of work separately:
  - (1) Effect of Nafion® and %RH on hydrogen activation.
  - It is common knowledge that hydrogen adsorption and oxidation are not severely impacted by these variables.
  - Given that CO has been studied extensively in fuel cells, the impact of water on CO removal in a qualitative sense is known. With only three humidity conditions shown, it is unclear that more is known after these experiments.
  - (2) The esterification approach is a useful tool for assessing cationic contamination that has been developed through this project and validated experimentally.
  - (3) The cationic contamination studies appear repetitive with previous work in terms of choice of cations and specific observations; e.g., the effect of monovalent versus divalent ions is known by now.

- (4) Some "new" contaminants such as THF and PCE have been screened, but no chemical insight has been gained. We have learned from this work that these species poison the fuel cell, and that some extreme operating conditions can remove them, but not much else.
- (5) Expansion of the contaminant "database": Although such data must be tabulated, ultimately this information should be synthesized into useful models or groupings that allow for prediction, which has not occurred.
- (6) Mitigation strategies: two of the suggested strategies, increasing RH to increase conductivity during NH3 poisoning or using more Pt to prevent CO poisoning (the latter is impractical), are already known. As for the third strategy of lowering humidity for CO removal, one would expect that the effect of humidity on CO poisoning has also been determined based on the extremely long history of the study of this molecule in fuel cells. No strategy was experimentally validated to be pragmatic.
- Overall, the work did not generate many new, predictive insights or any validated mitigation strategies.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Collaboration between university, National Laboratory, and industry partners provides a very good mixture.
- The team is a great strength of the project.
- The team is well situated to have one segment do the impurity effect experiments with good control in the laboratory and good prospects for having another segment of the team validating these results in practical environments.
- How are these results are connected to the codes and standards team efforts?
- Good triad of collaborators: laboratories, academia, and industry.
- Collaboration and coordination of the project team was evident with defined roles for each partner in the team.
- Although university, national laboratories, and industry are represented here, there seems to be duplication of work performed in other impurities projects and, importantly, when work on well-studied topics has been performed (such as CO, Nafion®, cationic contamination, etc.), the degree of understanding has not been significantly advanced over previous reports.
- The exact roles of John Deere and Argonne National Laboratory remain somewhat unclear.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Logical continuation of the project is suggested for most tasks. However, the future work should address the gaps in the research to make this project more valuable to the fuel cell community. This includes developing and confirming applicable mitigation strategies, and correlating the phenomenological studies with fuel cell experiments in a useful applicable way.
- The plan is being well implemented to learn the effects of impurities on fuel cell performance and stability.
- At this stage, I expect the team should emphasize the proposed future work to "correlate phenomenological results with actual fuel cell runs."
- Builds nicely on past accomplishments.
- The proposed future work on PCE, CO_2, ethane, ethylene, and acetaldehyde is well planned and very relevant. The proposed future work on ammonia and CO has minimal value in providing new information on these contaminants and in addressing fuel cell performance barriers.
- The project is nearing completion, and, although studies on "poisoning mechanisms and rates" have been promised, this type of work has not been well represented in the project to date. Degradation rates on a variety of contaminants will be obtained to "provide Argonne National Laboratory Modeling Group with ... data on NH_3 poisoning of Nafion® and CO and PCE." Such information is needed but does not constitute a comprehensive, insightful contribution to the subject of impurity studies.

Strengths and weaknesses

Strengths
- Sound approach using some novel ideas.
- The methodology, resources, and team skills are the greatest strength of the project.
- Comprehensive set of characterization techniques with complementary fuel cell and components studies. Study of hydrocarbon contaminants.
- Communication with DOE seems to be strong.
- The analytical test developed to assess conductivity in electrodes may prove useful to the community.

Weaknesses
- Correlation of *ex situ* results to *in situ* fuel cell results is largely missing. The benefit of the *ex situ* experiments is unclear.
- Mitigation strategy section is non-existent and needs more attention.
- The modeling component of the project is very limited and for a lot of the data, it is unclear how the results will be used and if they will be used at all.
- Approach is not followed through far enough to exploit the full potential of beneficial information that the ideas in this project had to offer.
- A limited set of impurities has been selected. The overall effect of studied impurity on fuel cell response is being well characterized and this is useful information. However, how the mechanisms of the effects of the impurities (in particular cooperative interactions of mixtures of impurities) are being determined was not clear to me. As a result, the team is clearly finding how a selected impurity affects fuel cell performance, but may be getting limited use out of its work. The effects of a particular impurity may be discovered, but this knowledge may not be extendable to other similar materials unless the mechanism is determined (e.g., is an activated hydrocarbon like PCE an organic or chloride impurity?). This approach may change as mechanistic trends emerge. The team should try to learn trends.
- Duplication of past contaminant work, dwelling on ammonia and carbon monoxide impurities.
- Many older topics are addressed such as effect of Nafion® and water on CO without any notable advance in understanding or mitigation.
- When working on heavily examined topics, the burden is on the researcher to demonstrate clearly what the advances in knowledge are.

Specific recommendations and additions or deletions to the work scope

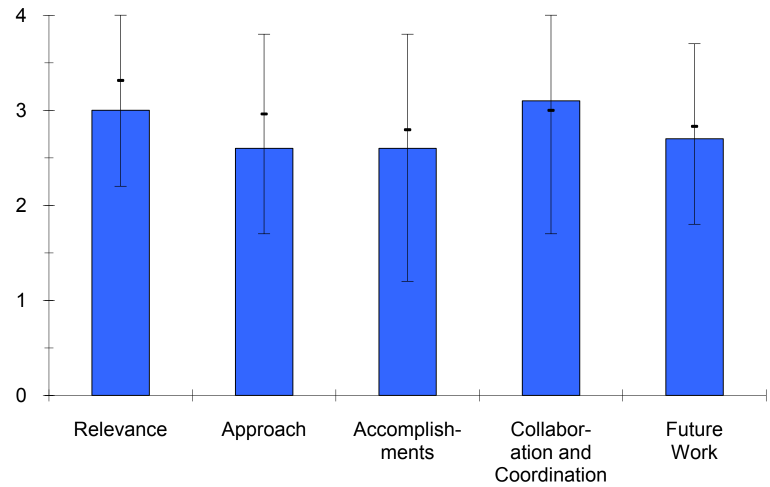
- It would be wise to try to isolate the moiety that is causing trouble in a complex impurity. For example, in the case of perchloroethylene, it would be wise to see if an equimolar amount of chloride gives the same impurity effect.
- Delete further work on ammonia, carbon monoxide, and other contaminants already studied in past projects. Add airborne impurities to project scope.
- Perhaps any work using CO should discontinue. It is unclear what is new here.
- New chemical insight should be generated. Which contaminants act as simply site blockers? Which can be removed by potential pulses? Which have irreversible effects? Are there any non-additive effects when more than one contaminant is present? How can one deal with cation contamination once it is present? Such questions that generate new information should guide the project.

Project # FC-47: The Effects of Impurities on Fuel Cell Performance and Durability
Trent Molter; University of Connecticut

Brief Summary of Project

The overall objective of this project is to develop an understanding of the effects of various H_2 impurities on fuel cell performance and durability–critically important for automotive fuel quality. Specific task objectives are to: 1) identify contaminants in both fuel and oxidant streams; 2) develop analytical methods to study contaminants; 3) develop contaminant analytical models that explain contaminant effects; 4) construct that quantify that material degradation; 5) validate contaminant models through single cell experimentation; 6) develop and validate novel technologies for mitigating the effects of contamination; and 7) conduct outreach activities.

Overall Project Score: 2.7 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The study of impurities is an essential topic in moving fuel cell technologies successfully to the marketplace.
- Impurities are specifically important for the performance and durability of fuel cell vehicles.
- This activity is highly relevant for a hydrogen economy. The testing is in support of a hydrogen fuel standard, the lynch pin for a hydrogen infrastructure. This research validates JARI data while continuing the screening of related compounds of interest to industry.
- The investigation of the effects of the key impurities on the fuel cell performance and durability is well aligned with the DOE objectives. The proposed objectives of developing mitigation strategies and providing the information to the industry through outreach are well aligned with the DOE objectives.
- Although understanding impurity effects on the rates of fuel cell reactions is of paramount importance in the quest for full utilization of PEMFC systems, in my opinion, this work is not critical to the Hydrogen Program.
- Good focus on the automotive component of fuel quality.
- Though contamination is a very relevant area of study, and the authors had input from the fuel quality team for potential impurities to study, the conditions were not very relevant. Nearly all experiments were performed at 100% RH, which would likely flush the contaminant species from the system, and 800 mA/cm^2 is too high a current density for the contaminants being studied. It is not surprising that the authors observed little effect from many of their experiments, the impurities not flushed off were likely oxidized at high potentials. It is recommended that the authors probe different operating conditions.
- The stated work scope of the project is certainly relevant to Hydrogen Program goals. However, it is not clear based on this presentation that the objectives related to model development, mechanistic understanding, and development of impurity mitigation strategies have been achieved. Perhaps some of these significant remaining tasks can be addressed in the final half year if external data sets are also used.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- The overall project approach is sound.

The project focuses on the effect of contaminants in the anode feed stream at contaminant concentrations identified by DOE, industry, and working groups. This is a good approach for creating valuable data of interest to the community.
- The project's intent is to use the data for model development. Modeling is one essential part of increasing understanding of the effect of contaminants.
- The approach is tried and true and is compatible with the research done by the other labs involved in this type of testing and funded by DOE, NEDO, and the European Union.
- The use of industry input and standardized testing provides a good approach to the understanding of the effects of the impurities. The initial examination of high levels of impurities to screen for significant effects is good.
- The project is not well designed and needs major improvement to be able to add substantially to this DOE Hydrogen Program. Although testing the impacts of different hydrocarbons and halogenated compounds in systems of importance for fuel cell developers is important, the true effects of these impurities should have been tested in a simple electrochemical cell. With this approach, the data collection (and understanding) would be much more improved. For example, this methodology would allow utilization of infrared spectroscopy, which is indispensable for such systems. I do not think that NMR is as useful as IR. To use solely cyclic voltammetry for analyzing the coverage and the nature of contaminants is insufficient, as seen with the great uncertainty about CO adsorption even for as simple system as formic acid oxidation. Recovery, i.e., oxidation of CO produced in a course of HCOOH oxidation, is well understood and the PIs should consult the existing literature about this "problem." The same applies for NH_3 contamination. Simple laboratory testing is required to unravel the effect of NH_3 on modifying catalytic properties.
- The project builds on and leverages past work and it sticks to standard protocols where possible. I recommend focus on specific impurities identified by DOE, industry, and working groups. The project plan and participant roles are well defined.
- It does not make sense for the authors are running for 100 hours to establish a baseline and then starting to introduce contaminant species. It seems like a waste of test stand time. Perhaps they should develop a better break-in protocol and then run a baseline for only a couple hours.
- At the end of the third year of a project, it is fair to look at what has been accomplished rather than what is promised to evaluate the approach. If one looks at what has been finished, the core of the approach seems to have been to run 100-h experiments on a variety of contaminants. Without any supporting experiments, little insight can be gained from this type of work.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- This project addresses a significant amount of necessary labor and time-intensive footwork for understanding hydrogen fuel contaminant effects. The results are important, but scientifically not very interesting.
- Measurements of a variety of hydrocarbons and halogenates have been performed. While most impurities that were tested have little to no effect, a more detailed study was conducted on formic acid which showed an effect at high but not at low concentrations. Such studies would further increase the value of the project when combined with predictive models.
- The study of contaminant mixtures was missing.
- The amount of directly applicable data generated over the duration of this project is impressive.
- The project has made good progress in understanding the effects of the impurities. The PI did not report any significant progress against the goal of developing migration strategies for the key impurities.
- I am not convinced that accomplishments are significant. Most of the data presented so far are unconvincing and even without this project, the fuel cell community has already been aware of contamination problems induced by CO adsorption, ammonia decomposition, and negative cation effects on the membrane properties.
- Critical issues defined beyond standard program rhetoric. Need of additional coordination between labs is critical to eliminate duplication of effort. Standardization of protocols and hardware is critical for comparison of results.
- Impurity investigation is methodical and well documented.
- The rate of progress seems really slow. The experiments are very long and testing conditions do not seem that well thought out, especially for a three-year-old project.

- Most of the experiments presented were 100-h fuel cell tests at constant current density followed by introduction of a contaminant and continuation of the test for another 100 h. While such experiments contribute the building of a database of contaminant behavior under consistent conditions, which is one type of valuable information, they are not a sufficient basis for model development and or mitigation strategy development. For example, perturbations such as humidity changes and competition between contaminants would aid in generalizing the results. Comparisons between contaminants should also be made in the context of some governing model that leads to predictions that can be validated.
- It is unclear what new information has been gained from the formic acid study, especially the cyclic voltammetry section. If the loss in ECSA had been correlated to the voltage loss in some way, then that would have added more value.
- Aside from the above details, the stated goals of "develop(ing) contaminant analytical models, construct(ing) material state of change models, and develop(ing) and validat(ing) novel technologies for mitigating the effect[s] of contamination" have not been addressed to any meaningful degree by the end of year three of a four-year project.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- Collaboration between two university institutes and two industry partners provides a good mixture. It remains a little bit unclear how much the individual parties contributed to the project.
- I recommend collaborating with / participating in a work group addressing hydrogen fuel contaminants and
- with a national laboratory.
- The collaboration with JARI, CEA, HNEI, LANL, the University of South Carolina, Clemson/SRNL, NREL and ANL is refreshing. The outreach to industry on the selection and prioritization of impurities was a pleasant development. The effort by UConn is the gold standard on collaboration.
- The PI has built a good team of participants for the project and appears to be working on outreach and industry interaction to ensure understanding of the key impurities.
- Collaboration between partner organizations is good.
- The collaborative group is far and away the most diverse of the fuel impurity investigations.
- Since the use of partner data for the creation of models and mitigation strategies is implicit, and these goals have not been achieved, one should assume that the collaboration level is also insufficient.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The base idea that future work should concentrate on completing the contaminant database, model development, and the development of mitigation strategies was presented.
- Detailed plans were not presented and left open questions such as which contaminants will be focused on, which will be used for modeling, for which will mitigation strategies be developed?
- Mixture effects were not included.
- The long term effects of contaminants are also important although they may be out of the scope of this work.
- Real-world operating scenarios during exposure to contaminant mixtures would be another important future work item.
- The future work is appropriate to the stage of the project. More data on halogenated compounds and diols (heat transfer fluids) would be useful.
- The proposed approach of moving towards testing mitigation strategies is an important objective in meeting the stated project objectives in the next year.
- In my opinion, the proposed future work may lead to improvements, but needs much better focus on overcoming barriers.
- Plans are methodical, well documented and not the typical scatter of proposed future work.
- I would recommend the group discuss testing conditions and proposed mitigation experiments with the Tech Team to get input on the experiments. I would also recommend asking the Tech Team to suggest a better break-in protocol for future experiments.

- Since most of the major goals (including modeling and mitigation) have not been addressed, the project is in a state of catching up. It is not very likely that three years of work can be finished in only one.

Strengths and weaknesses

Strengths
- Not much scientific new ground is explored. Mostly labor intensive though important footwork is performed.
- This project focuses on filling the gaps that were present to create a comprehensive overview of the hydrogen fuel contaminant impacts.
- The strengths of effort is the collaboration with other labs, leveraging of industry expertise, and the researchers' techniques.
- Good fundamental understanding of the effects of the impurities and their effects on the performance. Good industry interaction on understanding the important impurities.
- The strength of the project is systematic collection of data related to poisoning effects of hydrocarbons and halogenated compounds. Also, a positive side of the project is the potential technology transfer of data and a strong collaboration with industrial partners.
- Well thought out. Well organized. Well documented.

Weaknesses
- Although this project is focused on testing impurity effects in real fuel cell systems, much more fundamental work is needed to be able to understand the formation of spectator species and how to remove them to recover activity and stability of fuel cell interfaces.
- There is no higher level organization of the data sets into predictive models. If this is to be a contaminant screening exercise, then that should be clearly stated from the beginning.

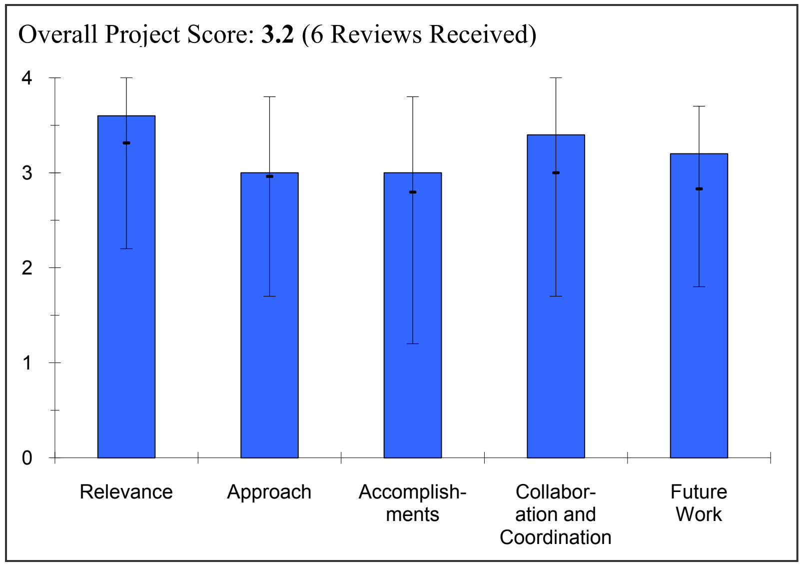
Specific recommendations and additions or deletions to the work scope

- Need to move forward on the development of mitigation strategies for the impurities examined.
- Increase the scientific output of this project by concentrating on the mechanism of contaminants that showed a performance effect, modeling of these mechanisms with the goal to provide a predictive model, and identifying reactions that may occur in the cell even for contaminants that do not show any performance impact. This last activity may particularly be important for understanding contaminant mixtures.
- Continuation of activities in this area would be money well spent.

Project # FC-48: Effect of System and Air Contaminants on PEMFC Performance and Durability
Huyen Dinh; National Renewable Energy Laboratory

Brief Summary of Project

The overall objective of this project is to decrease the cost associated with system components without compromising function, fuel cell (FC) performance, or durability. Objectives are to: 1) identify and quantify system derived contaminants, 2) develop *ex situ* and *in situ* test methods to study system components, 3) identify severity of system contaminants and their impact on operating conditions, 4) identify poisoning mechanisms and investigate mitigation strategies, 5) develop models/predictive capability, 6) develop material/component catalogues based on system contaminant potential to guide system developers on future material selection, and 7) disseminate knowledge gained to the community.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The project is very relevant to this program. The off gassing of many compounds used in the manufacturing of a vehicle has the potential to become an airborne contaminant that would adversely affect the performance of the FC.
- To identify contaminants which have an effect on FC performance and durability is important. It is necessary to obtain general knowledge for possible contaminants rather than specific materials which they are using in the current system. This should not be an engineering project; it should be a research project to obtain fundamental knowledge about possible contaminants and their mechanisms.
- FC system contaminants can have a significant effect on FC durability, but there has been little quantitative work performed to date. This project fills that gap.
- Relative to other DOE-funded impurity projects, which are often overlapping and redundant, focusing on system impurities is new and relevant.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach is valid. It is suggested that NREL go through the same round robin time testing that the Japan Automobile Research Institute (JARI), the Hawaii Natural Energy Institute (HNEI), the Los Alamos National Laboratory (LANL), and the Universities of South Carolina (USC), Clemson, and Connecticut did. This testing would verify repeatability and reproducibility, as was done on the fuel quality effort.
- The project seems to focus on materials that are used in the current system and to see whether or not they would be contaminants. It is necessary to dig up one more detail to obtain fundamental knowledge about possible contaminants and their mechanisms.
- The addressing of system derived contamination has been overlooked in the past. As commercialization proceeds, this possible avenue of contamination must be investigated more closely with the direct input of industry collaborators.
- The focus has been prioritized by employing in-house knowledge and experience from General Motors (GM).

- The approach is sound and well-considered.
- Additional details of the testing protocols used for different system contaminants would help to even better evaluate the approach taken.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The accomplishments are appropriate for this point in the program.
- The project has just begun, but significant progress is already being made.
- This project has shown good progress at the development of test methods.
- The lack of any electrochemistry and FC data is a drawback.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- The list of collaborators is impressive.
- They have taken an excellent team approach.
- The team is strong with well-defined roles of participants. The level of involvement to date of several partner-organizations in the project; e.g., that of LANL, USC, and the University of Hawaii (UH), has not been clearly highlighted in the PI's presentation.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The proposed work flow is rational.
- The proposed future work plan appears well-focused.
- The proposed future work plan does not seem to have been very well thought out. Most materials should have been selected and acquired by now. The same applies to the review of prior work.

Strengths and weaknesses

Strengths
- The strengths of this project are the collaborations.
- NREL assembled an excellent team with excellent capabilities and a well-focused effort.
- The focus on system materials and good teamwork are definite strengths of this project.

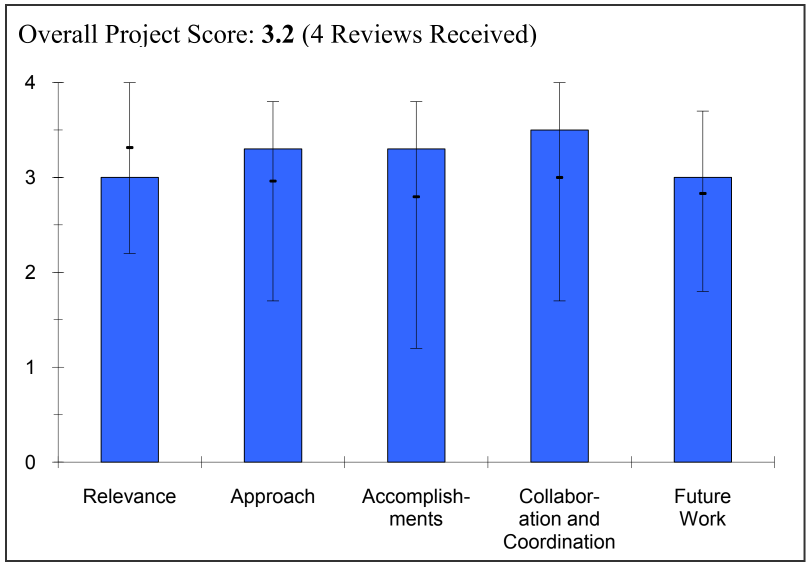
Weaknesses
- Weaknesses do not appear to be evident.
- Impact of contaminants on FC performance has not started yet.

Specific recommendations and additions or deletions to the work scope

- The project seems to be focusing on materials which are used in the current system and to see whether or not these materials could be contaminants. It is necessary to dig up one more detail to obtain fundamental knowledge about possible contaminants and their mechanisms.
- Electrochemical ("half-cell") and FC testing should start as soon as possible.
- The development of a preliminary model for impurities with different impacts on the key FC components (catalyst, membrane) should be initiated and subjected to experimental verification, in parallel to the stability testing of system materials.

Project # FC-50: Economic Analysis of Stationary PEM Fuel Cell Systems
Kathya Mahadevan; Battelle

Brief Summary of Project



The overall project objective is to assist DOE in developing fuel cell (FC) systems by analyzing the technical, economic, and market drivers of polymer electrolyte membrane fuel cell (PEMFC) adoption for stationary applications. Support in 2009 included two tasks: 1) developing technical targets for a 5-kW direct hydrogen PEMFC for backup power by developing a manufacturing cost analysis at varying levels of production, and 2) developing an economic and market opportunity analysis for micro-combined heat and power (CHP) PEMFCs to identify key target markets and value proposition for PEMFCs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project is an ongoing study of the cost of stationary FC systems targeted at the 5-kW output power level. It is relevant to the new responsibility of the Fuel Cell Technologies Program to focus on near-term, early market FC opportunities.
- This project addresses all barriers in the multi-year plan through economic analysis (task 9) of small stationary PEMFCs. Both manufacturing cost and micro-CHP analysis are considered.
- This project is relevant to the DOE program objectives, particularly since DOE has increased itsfocus on stationary and early-market applications.
- The project addresses the DOE goals for boron and fluorine.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The approach of developing a "generic" baseline system design that is not dependent on a specific FC supplier design is good. Likewise, doing costing for 2,000, 10,000, and 100,000 units per year is important and provides costing information with demand flexibility and economy of scale effects to be analyzed.
- Input was received from the extensive stakeholder and user community. Battelle used a very logical approach, starting from system design to base case analysis (2,000 units, 5 kW), with future case cost and analysis refinement. Some of this was done based on their own experience, which could have some error bars associated with it, although that is mostly from external input. Battelle used well-accepted Boothroyd-Dewhurst estimating software for calculating the manufacturing cost.
- Previous studies by Battelle under this contract identifiedpotential markets and opportunities for FCs in non-automotive applications. In addition to the market analysis, this year's effort includes a cost analysis for micro-CHP systems. The Battelle approach is similar to the approach taken by Directed Technologies, Inc. (DTI) for estimating the manufacturing cost for automotive FC systems—the design for manufacturing and assembly (DFMA®) analysis. Their assumptions regarding system configuration and manufacturing techniques have been vetted with FC developers.

- The approach is solid. Using leading cost estimation software makes sense. The weakness lies in the current limitation to one design. Incipient sensitivity analyses were presented. This should be extended in the future work.
- All data delivered are concrete data without consideration of the statistical variations. The Monte Carlo simulation allows handling of statistically distributed cost data easily. It might be considered for implementation, since some cost contributions are well known and others are highly speculative. This method allows for a better feeling for the variation of the cost prediction.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- This has been a long-term project (~6 years) and has shown steady progress toward meeting its goals and objectives.
- There is good technical progress, but it is a very long project (started 2003). Battelle presented believable numbers for the base case scenario. The scrap or reject rate assumptions (up to 30% for some parts) seem quite high. They should go down when going from 2,000 to 100,000 units. The balance of plant (BOP) breakdown cost summary was quite good. The stack component manufacturing cost breakdown (including tooling, processing, set-up, and raw materials) was revealing. There was a good capital cost analysis for a manufacturing plant, which was useful for Columbus, Ohio, but may not be useful in high cost areas.
- As of the date of slide submission to the AMR for fiscal year (FY) 2010, funding had not been received. Accomplishments include completion of the manufacturing cost analysis for 2000 5-kW systems, and the definition of market requirements for these systems.
- The achievement is very good. The analyses dig deep into the manufacturing process and consider the setup of a manufacturing plant.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- There are extensive collaborations securing input from FC users—more than 400. Many publications and reports are making the findings of this study available to the general FC community.
- They conducted extensive analysis using input from more than 60 FC industry stakeholders and more than 400 current or candidate users.
- Collaboration primarily consisted of vetting the project assumptions with numerous component and system developers in this market space.
- Sixty partners is the utmost number for a project to handle.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- It was not clear how long this study would continue, but the proposed focus on early adopters and market opportunities seems appropriate.
- The project is wrapping up. PI and coworkers are in the final stages of future case cost analysis modeling and refining technical targets.
- The project is ending in October 2010. The most important deliverable to be completed is a cost-benefit analysis of FC systems vs. the competing alternatives.
- Since the cost calculations are based on one selected case that can be referred to as a "base case", it would make sense for the future work or perhaps for a future project to add variations to this base case since cost is very sensitive to the systems design and not only to the stack.

Strengths and weaknesses

Strengths
- Battelle has extensive experience base in stationary system design and costing.
- Battelle exhibits good use of DFMA tool in a ground up analysis to evaluate manufacturing cost. There is good effort to obtain input from a variety of external sources.
- Battelle's market analyses have correctly identified market pull opportunities for back-up power and material handling vehicles.
- Battelle employed industry standard methods in developing a manufacturing cost estimate of micro-CHP systems.
- There is only a generic estimation of the cost, including production cost. Many industrial partners are involved.

Weaknesses
- None apparent.
- The sensitivity analysis could have large error factors.
- Micro-CHP systems, for the most part, will operate on natural gas or other common fuels, not bottled hydrogen, as depicted in the system schematic. A reformer should be included in the system cost.
- The estimation was based on one base case and no statistical data was incorporated.

Specific recommendations and additions or deletions to the work scope

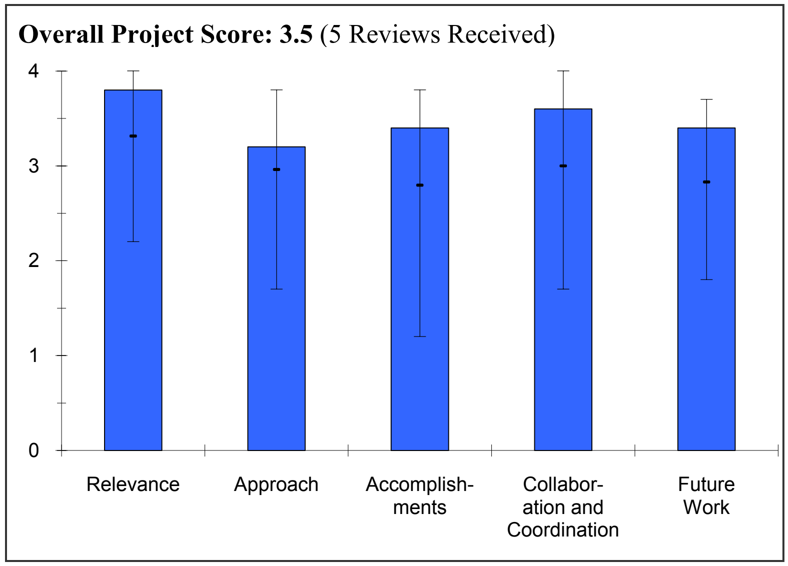
- Complete the project this year.

Project # FC-51: Fuel Cell Testing at the Argonne Fuel Cell Test Facility

Ira Bloom; Argonne National Laboratory

Brief Summary of Project

The overall objectives of this project are to 1) provide DOE with an independent assessment of state-of-the-art fuel cell technology; and 2) benchmark commercial fuel cell technology developments. ANL will: 1) develop standardized test procedures for the evaluation of different stack technologies, 2) characterize stacks and systems in terms of initial performance, durability and low temperature performance, 3) adapt the Fuel Cell Test Facility hardware and software as needed to accommodate the unique needs of different technologies, and 4) address barriers regarding durability and start-up time.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This project is very useful to evaluate in a reliable and reproducible way the performance and lifetime of the different stacks and systems developed in the different projects. With respect to results obtained in round-robin testing, standardized fuel cell testing procedures are really needed.
- It is very important to have standardized test procedures to get through the marketing hype so that informed decisions can be made based on validated data. This project supports this objective.
- This is a very relevant project, to verify manufacturer performance and R&D improvements.
- The project objectives are to assess independently a wide power range of fuel cell deliverables and benchmark commercial fuel cell technology development. These project objectives address performance and durability of the DOE Hydrogen Program.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The proposed approach to test, on the one hand, different stacks and systems using existing U.S. protocols, and, on the other hand, to compare those protocols with international protocols is adequate.
- The test procedures refer to transportation applications. But, since DOE objectives have evolved, in particular for fuel cell applications, protocols for stationary applications should be added.
- The use of an old stack is questionable. By definition, it is old technology. We should really be using the best available technology. An older stack that has been sitting on the shelf for years could have unknown shelf life issues that could give imprecise data or lead to incorrect conclusions. Recommend that the Program provide enough funding to the project to allow it to purchase current technology and not be forced to use cast offs.
- The project is able to provide very detailed stack performance characteristics. With the current resources, it is adequate to show performance, although it is not able to do it in a statistically significant manner (no repeat measurement for multiple stacks). Given that each stack has a large number of cells, it is still adequate for the task.
- This project develops standardized test protocols for evaluation of different state-of-the-art fuel cell technologies with the ultimate goal to address fuel cell commercialization barriers such as durability and performance.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- The presented results are good. However, more details on the test conditions (pressure, temperature, RH, etc. and for current-voltage (i-V) curves (dwell time, sampling time, etc.)) would provide all the needed parameters to evaluate the data. References to the protocols used could also be given.
- For system testing, the results showed that the tested systems are working well. A long-term graph would have been appreciated. As previously mentioned, the graphs are lacking information to be well understood.
- For protocol comparisons, the comparison between DOE and the Fuel Cell Testing, Safety and Quality Assurance ($FCTES^{QA}$) i-V protocols is very interesting. It shows that, as for single cells, if the testing conditions are stable, the values remain comparable even for different ways of recording.
- Now that it has been demonstrated that the two tests are comparable, the partners should work to develop a common standard.
- The team demonstrated an extensive evaluation of two 5-kW stacks as planned. The results were used to verify the standardized tests and the comparison of two protocols have been initiated. To date, the project has generated performance reference points for dynamic cycling tests based on two test protocols as originally planned.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- Collaboration with other institutions seems correct. Participation in the European project $FCTES^{QA}$ and in TC105 WG11 is appreciated and should continue in the future.
- All the major players are involved.
- The PI has been able to set up relationships with key industry manufacturers.
- There is a good selection of partners (domestic and international) to fulfill the project objectives.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- No specific slide on the future work is presented.
- Testing $FCTES^{QA}$ protocol A of the dynamic load cycling should lead to interesting results for stacks.
- Good plans competently executed.
- The future research objective is clear. The team will complete accelerated durability tests under the $FCTES^{QA}$ dynamic cycling profile. This test will finalize the benchmarking of commercial stacks and at the end, the report will be delivered to DOE.

Strengths and weaknesses

Strengths

- The strengths of the project are being able to test full systems and to compare existing testing procedures from U.S. and international as European stack testing procedures. Implication in international standardization, in particular TC105 WG11, is also appreciated.
- Achieved the objectives on schedule. Well planned and executed.
- Comparison of international fuel cell testing procedures.
- The experimental procedures are well defined, executed, and the results clearly presented.

Weaknesses

- The main weaknesses of this project are the lack of different stacks as an old 2002 stack has been used and the lack of details of the testing conditions which leads to a more difficult evaluation of the quality of the work.
- Use of an old stack is false economy.

- Old stack hardware was used for testing.
- Standardized test protocols should be application-specific rather than generic.

Specific recommendations and additions or deletions to the work scope

- Regarding the DOE objective evolution, in particular for the fuel cell applications, protocols for stationary applications should be integrated. Procedures already exist (IEC62282) and a new version is under discussion for small polymer electrolyte fuel cell systems (<10 kW, TC105 WG4). Applying it may be useful for experimental validation
- For protocol comparisons, the comparison between DOE and the European FCTES^{QA} protocols should go on as further testing procedures should be released in the following months. Moreover, a new work item proposal has been proposed to TC105 to establish i-V curve procedure as an IS. Experimental comparisons will be very helpful to establish valuable international procedures.
- Please provide an aggregated public presentation of the results. I realize the results are considered proprietary. However, an aggregated industry result could be presented in lines with the feedback provided by NREL's vehicle Tech Val program. This feedback would be of value for manufacturers as well as the research base to see, for example, the distribution of polarization curves or operating parameters in industry.
- Protocol verification tests are recommended for dynamic cycling tests. It is suggested to reverse the order of FCTES^{QA} and dynamic stress tests to exclude the effects of the experimental order.

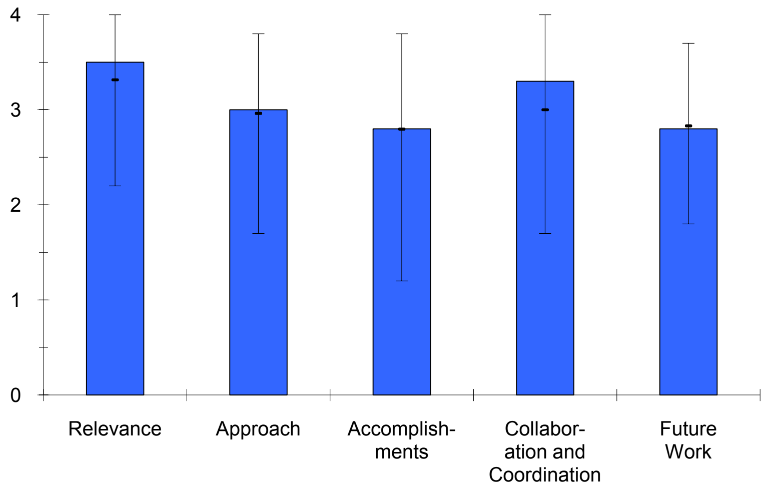
Project # FC-52: Technical Assistance to Developers

Tommy Rockward; Los Alamos National Laboratory

Brief Summary of Project

This project supports Los Alamos National Laboratory (LANL) technical assistance to fuel cell component and system developers as directed by DOE. LANL is expected to include testing of materials and participate in the further development and validation of a single cell test protocols with the U.S. Fuel Cell Council. This task also covers technical assistance to Working Group 12, USCAR, and the Fuel Cell Technical Team (FCTT). This assistance includes making technical experts available to the FCTT as questions arise, focused single cell testing to support the development of targets and test protocols, and regular participation in working groups and review meetings.

Overall Project Score: 3.0 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project provides the expertise and equipment of LANL to companies and agencies. This is a smart way to leverage the DOE investment and to provide to organizations answers to technical questions. If executed well, it has the potential to make very valuable contributions to the Hydrogen Program.
- A solid resource for all fuel cell developers.
- A good idea to provide these services, some of which are sophisticated and may not be available to new and resource-limited developers.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- It was not clear how clients are solicited, but those that seek support appear to be well supported. It would be useful to have feedback reports from each client—did they receive value for the effort?
- Delivers services to a wide range of customers. Offers both consulting support and on-site training.
- The array of services is broad. Should consider incorporating similar services for other types of fuel cells beyond PEM.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Need feedback from clients to make a determination of value and progress, otherwise it is just a collection of facts with no appreciation or interpretation of results.
- Good variety of services delivered in past year.
- Most capabilities appear at a mature level. Provided assistance to a wide array of developers and researchers.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- This project is all about collaboration. Several clients were supported. Could they have done more? How do they reach out to potential clients—do they advertise the service? How are priorities established.
- Wide variety of customers served.
- Appears good collaboration among developers and service provider.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- No plan was identified. Need to coordinate with overall strategic objectives of the Hydrogen Program to ensure the efforts are "strategic."
- Continues current work. Would like to see plans for expanding services or reaching or communicating capabilities more customers, to increase number of customers served; e.g., what is the continuous improvement plan.
- Planned classes on hands-on training for various techniques.

Strengths and weaknesses

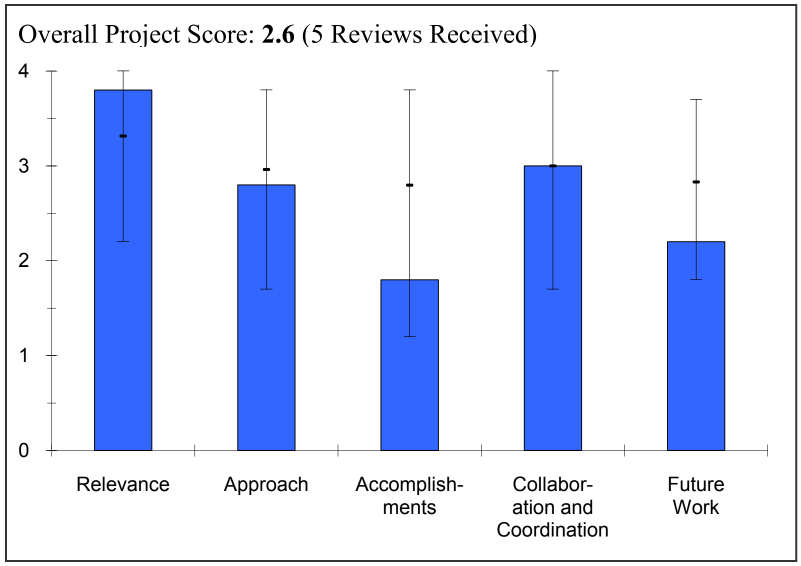
Strengths
- LANL has excellent people and equipment resources. This project has the potential to leverage them in a strategic way.
- A resource that is used by many fuel cell entities.
- Support from LANL.

Weaknesses
- Not clear if the effort is "strategic." Needs to work with DOE program to ensure "clients" have relevant and important issues to solve that are important to the Hydrogen Program. This project should not be a "make work" project.
- The quality of the review presentation was very disappointing. Hard to follow. Did not follow the format. A very half-hearted effort. LANL can do much better than this.
- Possibly the need for more communication of capabilities and increased utilization.
- Currently appears to be limited just to PEM.

Specific recommendations and additions or deletions to the work scope

- Develop a strategic test plan in coordination with DOE that targets clients rather than a first come, first served approach.
- Add continuous improvement plan—how will you reach more people each year or what new services will be provided in future year. Also, what is your feedback mechanism (beyond AMR) to determine if the services you are providing are what is, or will, be needed by fuel cell developers?
- Include other fuel cell technologies. Help developers study decay mechanisms, impact of impurity and variability of operating conditions.

Project # FC-59: Improved, Low Cost, Durable Fuel Cell Membranes
James Goldbach; Arkema



Brief Summary of Project

The objectives of this project are to: 1) develop a membrane capable of operating at 80°C at low relative humidity (RH) (25%-50%), 2) develop a membrane capable of operating at temperatures up to 120°C and ultra-low RH of inlet gases at <1.5 kPa, 3) elucidate ionomer and membrane failure and degradation mechanisms via *ex situ* and *in situ* accelerated testing and develop mitigation strategies for any identified degradation mechanism, and 4) use commercially-available matrix materials as a low cost approach.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Membrane development is in line with DOE objectives.
- Low cost, durable fuel cell (FC) membranes are necessary to meet DOE cost and durability targets.
- The project is in line with DOE goals and targets. The project targets use low cost, high temperature, and low RH membranes, which would enable system simplification and cost reduction.
- Developing a polymer with good mechanical properties and with high conductivity under hot and dry conditions is relevant to DOE objectives.
- Development of membranes that can operate over a wide temperature range, including 120°C and at low RH is crucial for being able to simplify the balance-of-plant (BOP) and lower the cost and size of the thermal subsystem. This project seeks to address these concerns.
- The project also maintains relevance by attempting a low cost approach and by seeking to understand failure modes associated with chemical and mechanical durability.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The approach of blending a structural polymer such as polyvinylidene fluoride (PVDF) with a conductive ionomer is valid. However, the loss of conductivity resulting from the low conductivity of the structural polymer must be recovered by the properties of the ionomer. Much of the project has involved an unfruitful search for an ionomer to meet or exceed Nafion®.
- Blending a highly conductive, low cost ionomer with a stable non-conductive polymer is a solid approach.
- The approach to make blends with PVDF may have cost benefits. The approach to make blends with Virginia Tech block copolymers that do not meet the conductivity targets themselves is questionable—blending with PVDF will reduce the conductivity further. Biphenylsulfone: hydrogen form (BPSH) 60 does not meet conductivity targets, so blending with that does not make sense. Crosslinking highly sulfonated (BPSH 100) type materials to improve stability appears to be a better approach.
- The project is well designed and well organized, with a clear focus on simultaneously improving conductivity and mechanical properties. The blending approach is a reasonable approach to take in addressing these issues.
- The approach begins with the attempt to combine a low cost material with mechanical strength Kynar with a conductive polyelectrolyte. In this sense, the project approach is adequate relative to the other membrane projects in the program.

- The approach falls down when the following is considered: why would the polyelectrolytes used be likely to be more conductive than Nafion? No morphological or conductivity data are given to justify why conductivity should be higher, either before or after mixing polyelectrolyte with Kynar.
- Given the low performance shown by BPSH in prior projects, it is unclear how this route would assist with high temperature conductivity.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.8** based on accomplishments.

- Very little progress is evident since last year. The performance and conductivity of the Kynar/M70 blend are still significantly below that of Nafion 211.
- The initial scale up of the fabrication process was successful.
- No durability data were presented for M70.
- Unfortunately, the polyelectrolytes are not conductive enough, and the membranes still cannot compete with state-of-art perfluorosulfonic acid (PFSA) membranes at dry conditions. Also, no analysis was given to prove the low cost aspect.
- Arkema has improved performance at low RH, but performance is still below that of Nafion 212. Arkema did not show high temperature (120°C) performance. The project showed some improvement in durability in open circuit voltage (OCV) hold tests and should demonstrate this improvement in RH cycling and membrane electrode assembly (MEA) durability tests as well.
- Although the approach is interesting, the results speak for themselves. In two and a half years of work, the project has yet to generate a material with conductivity as high as the baseline Nafion. Progress has been made in improving the conductivity, but the project is now reaching a dead end. Increasing the acidity of the proton-donating groups is apparently the only route forward for this material, but the chances of success of this route are nil.
- Although progress has been made relative to the past year's results, the fact remains that the improved materials still perform worse than NR211 at all RH at 80°C.
- The most positive aspect of the project is low gas crossover for M70 and M43.
- BPSH-based membranes showed even lower conductivity than the M-series samples.
- Microscopy data were obtained, but it is not clear how these data will lead the way toward a morphology-based conductivity improvement.
- The OCV and RH cycling testing on M43 was completed last year; no new durability testing on M70.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Collaborations include a catalyst developer or manufacturer as well as experts in cell testing and microscopic characterization.
- Ionomers from the Virginia Polytechnic Institute and State University (Virginia Tech) were evaluated. Johnson Matthey Fuel Cells, Inc. (JMFC) made the MEAs. ORNL provided transmission electron microscopes (TEMs), but I don't see how that added value to the project.
- The collaborations between partners are good.
- The McGrath group is an excellent choice for collaboration.
- Regardless of results, collaboration clearly exists and is evident throughout the project. No contributors appear to be listed gratuitously.
- The Oak Ridge National Laboratory (ORNL) contributed to understanding morphology, which is a topic that should be well explored in order to understand whether theoretical limitations of conductivity are close to being reached.
- The Virginia Tech collaboration is evident in the provision of alternative ionomers, as well as in nuclear magnetic resonance (NMR) studies.
- The collaboration with JMFC is less obvious. It appears this likely has to do with MEA preparation.
- M70 x-y swelling data appear to be missing.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.2** for proposed future work.

- The project is virtually finished. Testing and characterization will continue.
- There's not much time left. Test M70 for durability.
- The project is set to end in September. The proposed work should bring it to a logical conclusion.
- The future work plans may be reasonable for improving the properties of the existing materials, but the chances of meeting DOE targets using these materials, or any derivatives of the materials that could conceivably be developed before the end of the project, are poor. The materials may be of interest in other applications, but from the standpoint of making progress toward DOE targets for hot and dry operation, the project is at a dead end.
- The future work section mentions some worthwhile efforts such as continued OCV and RH cycling. However, the project has existed for some time, and it would be interesting to know if the project is bumping up against theoretical limitations. Rather than just saying that membrane formulations will be "optimized," it would be refreshing to hear about specific prospects for improving one of these four aspects: 1) morphology to align conductive groups better, 2) acidity of conductive groups, 3) possibility of improving water retention without increasing swelling, and 4) all of the above in the context of minimizing PVDF.

Strengths and weaknesses

Strengths
- The membrane mechanical and chemical durability target achieved with M43 (last year's data). That they even did the tests should be to their credit.
- The potential cost benefit to PFSA is a strength.
- The project combined the resources of some knowledgeable polymer experts to investigate high conductivity without sacrificing mechanical properties.
- Use of a low cost component (Kynar) for mechanical strength is a strength.
- Arkema's willingness to adapt alternative ionomers within the concept of mixing with Kynar is a strength.
- Arkema's prior year use of durability protocols (OCV and RH cycling) for evaluating membranes was impressive.
- The collaborations are used well within the proposed work plan.
- The materials show low gas crossover.
- Arkema's reporting of in situ diagnostics (high frequency resistance (HFR) and hydrogen crossover) for in situ tests is a strength.

Weaknesses
- The ionomer chemistry was not disclosed, so the FC community cannot benefit from the findings.
- Blends have had poor conductivity.
- Arkema failed to incorporate alternative development pathways. The primary strategy for achieving high conductivity under hot and dry conditions has failed. A path forward has not been demonstrated.
- There has been continued optimization without really attacking the issues regarding the boundaries of proton conductivity.
- Arkema is still not obtaining the desired conductivity measurements.
- There are likely better alternative ionomers that could be used than the BPSH series. The concept may work better with other polyelectrolytes.

Specific recommendations and additions or deletions to the work scope

- It is too late to change the scope. Provide some cost estimate to justify the basis for the low cost claim.
- With the limited time available, all durability and FC testing work should be curtailed, and resources should be oriented toward increasing conductivity.

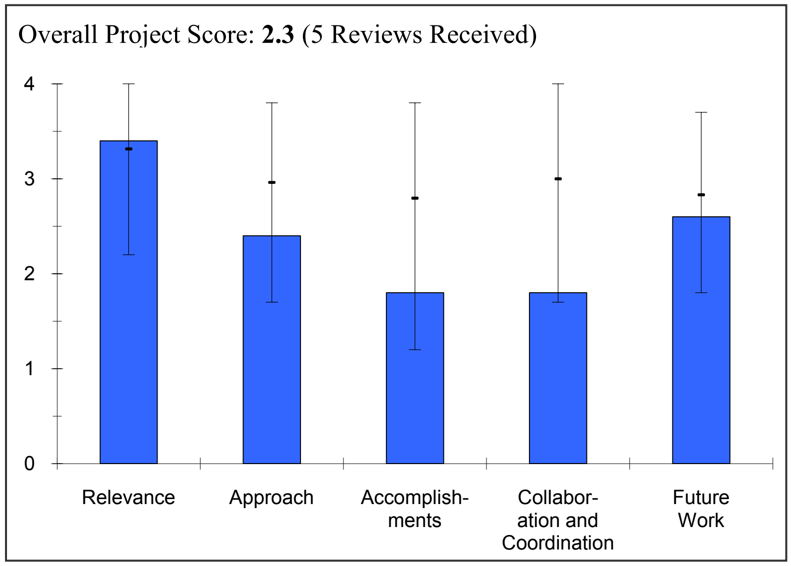
- The project could benefit through interaction with a theoretical group that could predict conductivity as a function of morphology, acidity, water uptake, etc. This would help to avoid studies with ionomers that will not perform well at low RH.
- Efforts involving FC polarization tests or production scale up could be minimized in favor of providing a full understanding of what material properties and components can be used to increase conductivity without sacrificing crossover or durability.

Project # FC-60: Protic Salt Polymer Membranes: High Temperature Water-Free Proton-Conducting Membranes

Dominic Gervasio; Arizona State University

Brief Summary of Project

The objective of this program is to make a solid water-insoluble anhydrous proton-conducting electrolyte membrane that has: 1) good membrane performance, including a) proton conductivity that surpasses the 2009 target of > 0.1 S/cm at 120°C and 50% relative humidity (RH), b) effectively having no co-transport of molecular species with a proton, and c) good mechanical strength and chemical stability; and 2) good electrode performance, with the assisting catalyst to promote reduced activation overvoltage.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Membranes with high conductivity at high temperature and low RH are relevant to DOE objectives.
- The development of an anhydrous proton conductor could enable significant system simplification and cost reduction of polymer electrolyte membrane fuel cell (PEMFC) systems.
- The project is mostly aligned with DOE targets and goals. High temperature membranes are an enabling technology that will allow system simplification and cost reductions.
- The project mostly aligns with DOE objectives. The ionic liquid studies are not directly relevant to the program, but to the extent that these studies could lead to immobilized ionic conductors, they are still relevant. The relevance is limited by the poor progress to date—a project that doesn't meet objectives or advance understanding cannot be said to be relevant.
- The DOE Hydrogen Program is still in search of membranes that can operate at reduced humidity and wider temperature range, including temperatures as high as 120°C. The intention of this project is to fabricate membranes capable of both.
- There are no aspects of the project that deviate from the intentions of the DOE program. Both low water and no water proton conduction are relevant to the intentions of simplifying the balance-of-plant (BOP) and reducing the cost and size of the thermal loop.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- The approach has changed dramatically since the beginning of the project from protic ionic liquid (PIL) to solid conductors based on indium tin phosphate (ITP). The ITP has excellent conductivity, but is brittle and somewhat porous. These issues are being addressed by adding a polymer to seal the pores and provide flexibility.
- The doped ITP should be the sole focus for the rest of the program.
- PILs present a unique approach to developing anhydrous proton conductors. Although the focus seemed to switch to ceramic proton conductors, such as ITP blended with organic polymers. More focus on actually making membranes is required.

- The idea of immobilizing ionic liquids for proton conduction at low RH is good. Work on non-platinum catalysts and FCs with PILs is outside the scope of high temperature low RH membranes. The work lacked focus on developing membranes and barriers to developing membranes that conduct at low RH.
- The project has lacked a clear and consistent focus. The lack of a well-organized approach has decreased the prospects that relevant and useful materials or discoveries will be produced.
- The approach seeks to generate proton-conducting membranes that require little or no water by deriving materials from functional groups found in PILs. Since PILs are known to have high proton conductivity, and since it may be possible to make membranes that are water insoluble, the approach is well conceived.
- Although it is reasonable for the approach to screen samples with thermal gravimetric analysis (TGA), there is no mention of durability experiments given a successfully conductive membrane.
- There is also no mention of preliminary ionic liquid conductivity that would be prerequisite to fabricating a membrane derived from the given ionic liquid.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.8** based on accomplishments.

- The original protic liquid approach was not successful in meeting project and DOE technical targets.
- Progress demonstrating the doped ITP has been slow.
- Liquid PILs do not meet DOE performance targets. One would not expect an improvement with ILs tethered to solid polymer backbones.
- Ceramic (ITP) shows good conductivity but cannot be made as an impermeable membrane. ITP polymer blends have lower conductivity. ITP has not been proven to be stable in an FC environment or even liquid water. No conductivity vs. RH data or mechanical properties was shown.
- Immobilized ionic liquids had poor conductivity. ITP showed some promise and had conductivity that met intermediate targets, but it does not have good mechanical properties. No improvement was made within the past year on improving mechanics or conductivity.
- No independent testing or verification of ITP conductivity was presented. There was no discussion or mention of durability of this material under FC conditions (RH and potential cycling).
- It is not clear that any of the materials developed and investigated will prove applicable to FCs. The project has yet to demonstrate a membrane capable of meeting the 0.02 $\Omega\text{-cm}^2$ at 120°C at 40-80 kPa water target, discounting the "pure" ITP material, which does not constitute a membrane. Having failed thus far to demonstrate achievement of the earlier 0.1 S/cm at 120°C and 50% RH in a membrane (again discounting non-membrane ITP material), the project is more than one year behind schedule, and odds of achieving even this interim target before the end of the project are low.
- The 2-fluoropyridinium triflate (2-FPTf) was touted as being a stable ionic liquid, but conductivity is too low for applications that must operate at times around 40°C (~ $6\text{x}10^{-3}$ S/cm). Polarization curves bear this out and also show mixed potential possibilities on catalysts at lower temperatures.
- Conductivity of ITP at low temperature is also too low (0.01-0.03 S/cm). ITP is to be blended with polyphosphazenes, but polyphosphazene conductivity does not appear to be reported.
- An attempt was made to report higher oxygen reduction activity in ionic liquid presence. However, the experiment was not noted to be a rotating disk electrode (RDE) (no revolutions per minute (RPM) value shown) and no limiting current reference could be used to understand the four electrons versus two electrons reduction. Mixed potentials were evident on polarization curves, which could imply that reduction currents could be associated with desorption of ionic liquid species from platinum, not actual oxygen reduction.
- In general, the project should be demonstrating more progress at this stage with respect to membrane conductivity, and all the properties expected with a membrane sample (swelling, gas crossover, durability, etc.).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- Collaborations are with strong companies, but their public history in PEMFCs for automobiles is small.
- A membrane electrode assembly (MEA) or stack developer might prove beneficial.
- It's not clear at all what role the subcontractors play.

- It is not clear what Boeing's role was. Collaboration between other partners was good.
- The role of collaborators in the project is unclear. A "collaborations" slide was included, but based on the content of the rest of the slides, it seems that this is basically a solo effort.
- Technical roles for the University of Akron and Boeing are not entirely clear from the presentation materials, except for the fact that they are mentioned as collaborators.
- Interactions with the project have not evidenced a collaborative spirit. Instead, suggestions have been cast aside to find ILs or ceramics that demonstrate high conductivity throughout the entire temperature range of interest (including temperatures as low as ambient).
- Beyond Akron or Boeing, there is little evidence of collaboration beyond submissions to Bekktech and University of Central Florida (UCF).

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Doped ITP will be further studied and tested.
- Making and testing membranes of ITP blended with organic polymers such as polyphosphazene should be interesting. Future focus should be on conductivity, hydrolytic, and mechanical stability of these materials.
- Plans to look at blends or composites of ITP and polymers or stabilized ionic liquids may help overcome mechanical issues.
- The project is unlikely to meet the original goals, but under the circumstances, if work is continued on the project, then the proposed work is reasonable. An emphasis should be placed on validating ITP results, given the revelation that free phosphates played a role in earlier, apparently erroneous, reports of high conductivity in neat ITP.
- The future work points to ITP polymer blends as a possible material with which to deliver membrane results. Unfortunately, the conductivity of ITP itself is low at lower temperatures. The prospects of increasing the conductivity with use of a polyphosphazene are questionable.
- The evaluation of platinum surface area in the context of ITP suggests that somehow ITP could be expected to increase surface area. The intention of whether the ITP is to be used as an ionomer or not is unclear. Otherwise, this activity does not seem reasonable, and it does not speak to the specific activity of the catalyst or Tafel phenomena. Again, it was not made clear in the results that the reduction current observed in the presence of ionic liquids were not related to desorption processes or 2-electron reductions.

Strengths and weaknesses

Strengths

- The unique approach to making anhydrous proton conductors separates this project from others in the high temperature membrane working group (HTMWG) program.
- The PI has some interesting and unconventional ideas. It is nice to see outside-the-box thinking.
- The initial approach of attempting to create membranes from immobilized ionic liquids was well founded.
- Prior to fabricating membranes, the project attempted to screen conductivity of some of the conductive species.
- The project appears to have a certain degree of curiosity about interactions of the conductive species with other parts of the MEA.

Weaknesses

- There were poor collaborations. The project could benefit from more expertise on polymer chemistry and membrane processing. There were no responses to reviewers' comments from previous years.
- The project lacked focus. Immobilized ionic liquids had low conductivity.
- The approach was scattered and disorganized, and the experimental work seems sloppy (free phosphate in ITP that in earlier reviews was asserted to be free of such residual species).
- The project has not focused on either delivering membranes or delivering a verdict against the use of proposed materials. Instead, it attempts to make the best of what it has, despite the fact that further development may lead to a dead end.

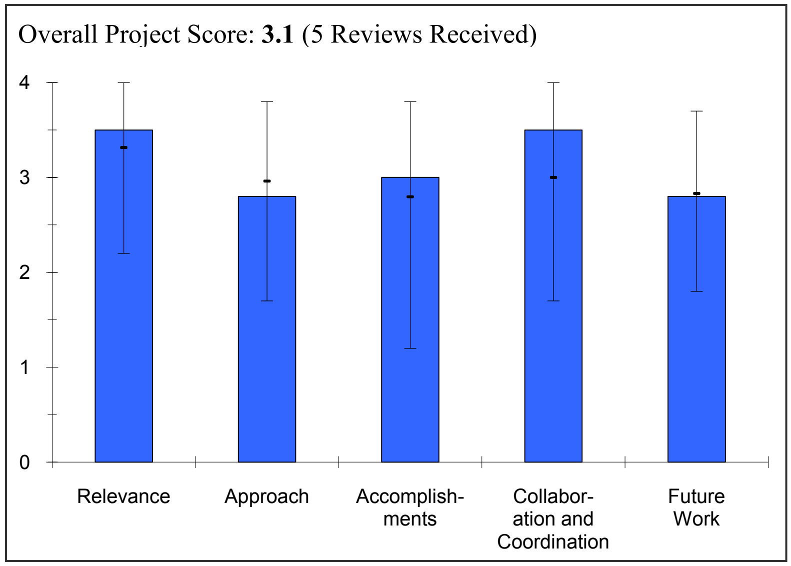
- Screening measurements—such as proton conductivity—on conductive species have not been interpreted as motivation to look at more conductive materials. Instead, membrane fabrication and FC testing are carried forward for materials that are not worthwhile.
- Deeper interpretation of kinetic data is needed to make the case that ionic liquids assist with oxygen reduction.

Specific recommendations and additions or deletions to the work scope

- Try blending ITP with Nafion®. That may at least help with low temperature conductivity. Conduct more stability testing and test the mechanical properties of membranes. Hold off on FC testing until conductivity and stability are proven. Test conductivity vs. RH.
- The value of this project to the hydrogen program is questionable. None of the approaches being pursued are likely to contribute significantly to the objectives of the program, so everything here looks like a good candidate for deletion, starting with the non-platinum catalyst component.
- If there were enough time remaining, some degree of proton conductivity modeling (as function of temperature) might be helpful toward directing the project to more promising species.
- Collaboration with electrocatalyst experts, such as Argonne National Laboratory personnel, would be helpful toward understanding whether the reverse sweep currents from voltammetry in oxygen are meaningful.
- In general, continued surveying of conductivity throughout a wide temperature range is useful. In the meantime, FC testing could be eliminated until worthwhile membranes are fabricated.

Project # FC-76: Biomass Fuel Cell Systems
Neal Sullivan; Colorado School of Mines

Brief Summary of Project



Overall objective for this project is to improve durability and performance of solid oxide fuel cell (SOFC) systems while lowering costs. Task 1 is to develop SOFC materials for robust operation on biofuels, including sulfur- and redox-tolerant materials to broaden SOFC operating windows and nickel-free, perovskite-based anodes with novel cell architectures. Task 2 is fuel processing of bio-derived fuels, including methane from anaerobic digesters of waste-water treatment plants, fuel-processing for biomass-derived liquid fuels (butanol), and decreasing the cost of fuel-processing balance-of-plant (BOP) hardware. Task 3 includes modeling and simulation to: 1) develop chemically reacting flow models of fuel-processing hardware, 2) use model-predictive controls to integrate system hardware, 3) conduct thermal modeling of hot-zone system components, and 4) employ system modeling to explore the benefits of balance of planar-component integration.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This is a really excellent project. It supports biomass to hydrogen, which is a very important hydrogen pathway.
- Colorado School of Mines (CSM) is looking at a 1-kW system using renewable hydrogen sources.
- CSM stated the application was for military 1- kW battery charger and/or generation set. DOE should not be funding work for the military. I doubt this is their only application. CSM needs to be clearer about their applications. In addition, this size range is on the border line for use of non-jet propulsion type 8 (JP8) fuels. If CSM scales it up (which they should), the Department of Defense will want to use a real logistics fuel in their proposed application.
- This technology is in line with DOE program objectives, as it directly supports SOFC development, while also providing expertise and training at university settings.
- The FC section of the Multi-Year Project Plan (MYPP) on the Fuel Cell Technologies Program (FCT) website indicates the program goal is to "develop and demonstrate FC power system technologies for transportation, stationary and portable power applications." This project is supportive of that goal; however, the MYPP objectives supporting that goal do not relate well to this project and appear quite dated. The technical approach section in the MYPP indicates that SOFC work is pursued under the Solid State Energy Conversion Alliance (SECA).

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The project has developed novel ceramic heat exchangers. There is extensive use of modeling, both as part of the design effort and then for conducting validation based on experimental results. Excellent collaboration with W.L. Gore & Associates (Gore) on ceramics really leverages both the monetary investment by DOE, but, more importantly, brings the expertise of Gore to bear on solving the problem.
- CSM has a solid team.

- CSM was unaware of others who had done work using ceramic microchannel reactors previously.
- CSM does not discuss the sealing issue, which will be critical for this project, especially since their target applications will be thermal cycling.
- Anaerobic biogas has a lot of impurities. CSM needs to discuss how they will handle the impurities, especially sulfur.
- Partial oxidation reactors have in the past produced small amounts of ammonia. CSM needs to analyze their reformate for this.
- CSM is not using water in their reformer. If the reformate is run to the reactor dry, it will coke up eventually.
- CSM is using a tubular SOFC configuration. CSM will have power density problems, which is especially important for transportable power generation. The low power density will be exasperated by their catalyst selection, which has never demonstrated high power in the literature.
- This approach is quite interesting. However, the mass and energy balance of the system was not shown. It was unclear why partial oxidation was used and what product market the device is pursuing.
- There has been considerable strontium titanate-based SOFC anode work done elsewhere, including SECA at the Pacific Northwest National Laboratory (PNNL). There does not appear to have been a thorough assessment prior to defining this work. Furthermore, R&D should focus on the technical or cost barriers to commercial viability, such as the cost or performance advantages of the work which, if successful, should be quantified to include specific targets; e.g., desired hydrogen sulfide tolerance, etc.).Similarly, the anaerobic digester gas (ADG) reforming work is not novel and does not advance the state-of-the-art, as the technology is readily available and in use (e.g., FuelCell Energy's (FCE's) molten carbonate fuel cell (MCFC) installations). The butanol work is more interesting; however, here again there is a solid base of existing R&D (injector design, catalyst development, etc.) looking at reforming pump diesel and JP8 (far more difficult fuels). It would be logical to evaluate butanol reforming using state-of-the-art technology prior to developing a R&D plan.
- High temperature heat exchangers are a high cost element of the SOFC system. BOP super alloy-based heat exchangers (HEX) are very expensive on a $/kW basis, with questionable life. Ceramic HEX have been pursued by others; e.g., Blasch and Acumentrics in their hybrid design, among others. A cost target ($/kW net system) should be established, and the actual projected unit cost used as one metric to track technical progress. The work to date has shown nominal manufacturability and technical viability. The attempts to integrate HEX/reformer dilute the effort and are overly aggressive—proving that the suitability of HEX function alone would be an important success.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- CSM has achieved excellent results in a short time frame. Some problems have been identified, but solutions have also been developed.
- The modeling work is completed.
- They have some initial data on their reformer.
- This is a newer project, so only a limited amount has been done.
- I was surprised that they did not know about other ceramic reactors and heat exchangers that have been built. They need to do a better literature review so they can learn from what others have done.
- The FC testing has all been short term. It will be interesting when they begin some long-term testing.
- Transportable generators need high power density, which was not demonstrated.
- As stated above, the effort does support the DOE/EERE goal as delineated in the latest MYPP. However, the technical and cost targets and justification for the individual elements of this project are ill-defined. There is not a clear relationship between commercialization barriers and the "problems" being addressed within this project.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- There is close collaboration with Gore. A great relationship that has been competently leveraged.
- They have good collaboration with Coorstek.

- They need to contact Ceramatec (part of Coorstek) to learn about the tape casting and reactor design experience that Ceramatec has.
- The university was working closely with Coortek.
- CoorsTek is a well-regarded ceramics company and is an excellent partner/supplier for the ceramic HEX components. I understand that many of the details regarding the manufacturing and cost are held proprietary, lessening the value of the overall effort.
- Other organizations; e.g., PCI, could be approached with respect to diesel reforming and its applicability to butanol.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The approach is very focused and aggressive.
- They need to develop a critical path to be able to achieve their goals. SOFC with perovskite electrodes have been demonstrated.
- They do not mention sealing. This will be more difficult than they think, especially if they are going to thermally cycle the heat exchangers and SOFC, which their stated applications demand.
- They need to address the contaminant issues. There is nothing in their future work dealing with this.
- Design and construction of a 1 kW SOFC stack will be difficult. This project is over ambitious for the funding amount.
- The ceramic HEX work addresses a substantial need with respect to low cost SOFC systems. CSM can address both manufactured cost and robustness or reliability. See previous comments with respect to the lack of clear quantified technical objectives or targets.

Strengths and weaknesses

Strengths
- Great leverage of partner resources. CSM has a focused plan that is well executed. CSM is hitting their targets and have good leadership.
- Colorado School of Mines has some very good modelers with experience modeling microchannel reactors and heat exchangers.
- The team is strong.
- Coorstek has great ceramic capabilities.
- The project team appears to have excellent facilities and equipment, as well as capable personnel.
- In particular, the ceramic HEX work with CoorsTek's participation addresses a critical need for high temperature FC systems.

Weaknesses
- Their system efficiency would be higher if they used steam reforming.
- They need to develop a critical path.
- The sealing of ceramics is very difficult. They need to start work on this immediately. If they have the technology for sealing, they should show data on thermally cycling it.
- Tubular SOFC have low power density, as does the catalyst they have chosen.
- The effort appears diluted given the funding—anode development, biofuel reforming (ADG and butanol), ceramic HEX, and modeling. Additional focus on specific objectives, with respect to the ceramic HEX for example, could yield more definitive results.
- The project elements on anode development and fuel processing are not positioned to advance the state-of-the-art.
- The project lacks definitive, quantifiable technical and cost targets that tie to program-level metrics; e.g., $/kW, desired fuel and associated sulfur tolerance, etc.

Specific recommendations and additions or deletions to the work scope

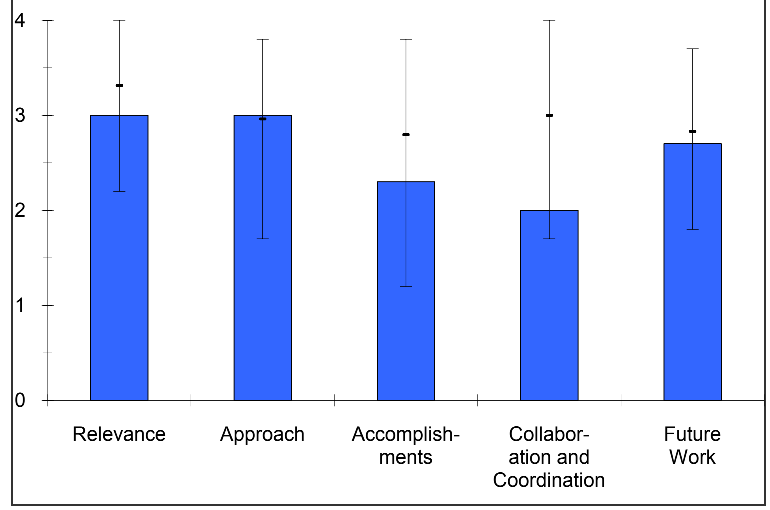
- Provide more funding and turn them loose.
- CSM needs to include impurities in their biogas reforming or explain how they will clean the gas.
- They need to work on the sealing of ceramics. CSM needs to demonstrate the ability of their ceramic reactors, heat exchangers, and stacks to thermally cycle.
- CSM is going to coke their reactor unless they add some water to the system either by pumping it in or doing anode gas recycle, but CSM is not planning on either. This issueneeds to be addressed.
- It was unclear what market segment this device is pursuing. The presentation was for a nearly complete system. It would be beneficial to focus the development with some market study and understanding of product specifications being pursued.
- If this is a biomass FC system, it would be needed to show why partial oxidation was selected. We did not see the performance of the partial oxidation portion of the process. Process flow diagram and mass and energy balance would be very useful to have. Additional consideration of BoP is necessary to provide guidance on product pressure drop and efficiency optimization.

Project # FC-77: Fuel Cell Coolant Optimization and Scale-up
Satish Mohapatra; Dynalene

Brief Summary of Project

The overall objective of this project is to optimize and then scale-up the process to make Dynalene fuel cell (FC) coolant with a great deal of reproducibility. One key ingredient of the coolant (a nanoparticle) will be produced in 100 L batches in a pilot-scale operation and the effect of various process parameters on the size and charge density of the particles will be determined. Another technical objective of this project is to optimize the filtration process for the nanoparticles to minimize the cleaning time for different-scale operations.

Overall Project Score: 2.6 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project directed toward the development of a long-term, low electrical conductivity coolant for FC operation is relevant to improving FC performance and durability.
- This project addresses FC system thermal and water management issues, which is a barrier listed in the Multi-Year Program Plan (MYPP).
- The coolant is applicable for future elevated temperature polymer electrolyte membrane (PEM) operation up to 122°C.
- The project addresses the scale up of an already developed coolant. Hence, it has high industrial relevance.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- This project builds on Small Business Innovation Research (SBIR) Phase I and II projects, and the current approach is directed toward scaling up the production of the developed coolant. It is a very focused approach to prove the availability of this product on a timely schedule for expanded application by FC suppliers.
- The project uses a low-conductivity fluid consisting of glycol, water, corrosion inhibitors, and nanoparticles to prevent the buildup of ions in coolant fluid. This allows the system to run without a deionizing filter, decreasing complexity and cost. The nanoparticles were not identified, so the effect of these materials on cost cannot be evaluated. Also, once the nanoparticles become saturated, the ionic concentration in the fluid will rise. This could be a major flaw. The emphasis in this project is on smaller FCs, 1-100 kW.
- The PI was not able to come and it was not possible to get all technical details answered by the substitute.
- A technical approach for the nano-particle production is taken. Such work is needed and often underestimated in order to attain stable production processes.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- The progress to date seems to be adequate, although past progress was supported largely by SBIR funding.

- Started out as a Phase I and II SBIR project. The current goal is to scale up from 0.5 L to a 10-100 L pilot plant. Reviewer would have expected the project to be at this point after the Phase II SBIR. It appears to have optimized the coolant fluid composition, although this was not presented.
- The progress seems to be good since a pilot plant is under development. Little to no technical information about the progress is provided. For that reason, the progress cannot be evaluated and was rated fair.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Collaboration with a university partner (subcontractor) was noted, but the role of the partner or nature of the collaboration was not apparent.
- No collaborations were identified.
- At this stage, the project does not need extensive collaboration. The existing collaboration with Lehigh University seems to be sufficient.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The future work plans focus on demonstrating the scale-up of the coolant production and are consistent with the project goals and objectives.
- The goal is to scale up to a 10-100 L plant by the end of August and resume research at these larger scales.
- Too little information is provided in order to evaluate the future work.

Strengths and weaknesses

Strengths

- They have experience based on previous SBIR support.
- This is a potentially valuable coolant that will be more compatible with smaller FCs and can simplify the system.
- They are very advanced in their status and are close to production.

Weaknesses

- None apparent.
- The saturation of nanoparticles is potentially a limiting factor. No cost comparison with other coolants was provided. One would need to see a cost comparison with standard coolants (using a deionizing filter) and see long-term test data to fully evaluate coolant effectiveness.
- No original equipment manufacturer (OEM) or system integrator data about the acceptance of the coolant was provided. No cost information was provided. No benchmark to existing cooling oils was provided.

Specific recommendations and additions or deletions to the work scope

- Independent validation and performance evaluation in an operating FC system by a National laboratory would help to establish the claimed benefits of this coolant.
- If the project is to continue, it should get a partnership with an FC manufacturer.
- No information about potential applications was provided. No tests by system integrators or OEMs were quoted. It should be made clear that the fluid really suits coolant requirements, a fact that is stated by the developer, but not proven by any facts in the poster.

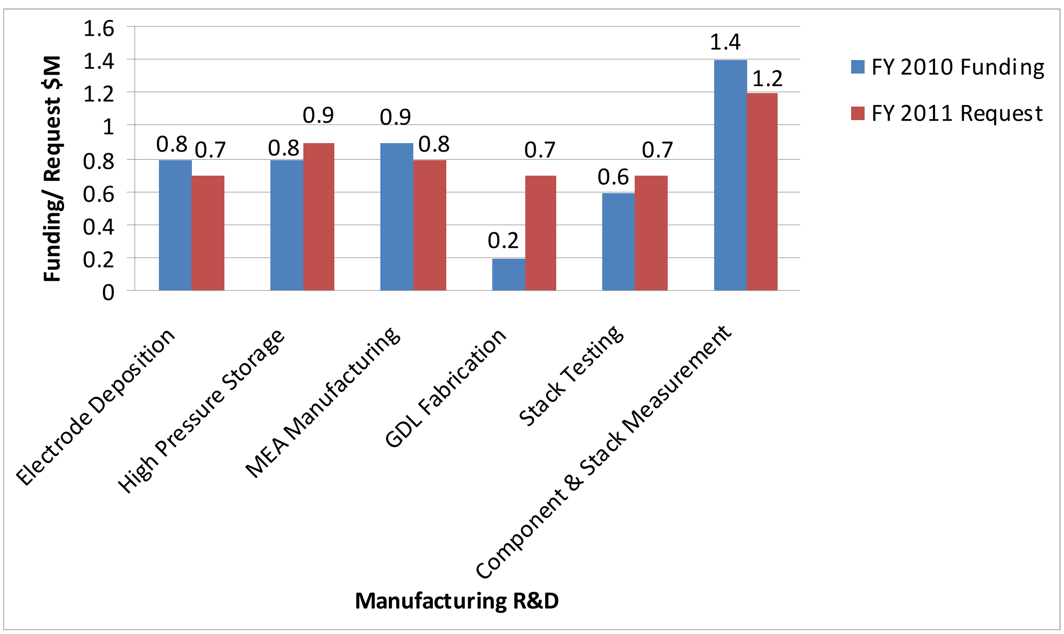
2010
Manufacturing R&D
Summary of Annual Merit Review of Manufacturing R&D Sub-program

Summary of Reviewer Comments on the Manufacturing R&D Sub-program:

The Manufacturing R&D sub-program develops and demonstrates technologies and processes to reduce the cost of components and systems for fuel cells, hydrogen storage, and hydrogen production, to enable the growth of a strong domestic supplier base. The activities focus on near-term cost goals for early market applications. In FY 2010, eight new manufacturing projects were reviewed. These projects addressed fuel cell membrane electrode assembly (MEA) manufacturing and fabrication of catalyst-coated membranes. In addition to new manufacturing R&D on low-cost, durable MEAs, the sub-program added work on gas diffusion layer production and fuel cell stack in-line testing. One project addressed lower-cost manufacturing of high-pressure containment vessels for hydrogen storage.

Manufacturing R&D Funding:

Funding for the Manufacturing R&D sub-program was level for FY 2009 and 2010 at approximately $5 million per year. All current projects are scheduled to continue through FY 2010 with future efforts subject to appropriations.



Majority of Reviewer Comments and Recommendations:

Manufacturing projects were rated high to average with seven individual projects rated 3.1 or higher. Overall ratings ranged from 3.6 to 2.7. All projects were judged to be relevant to the DOE Hydrogen Program's activities, with good or adequate technical approaches employed. In most cases, project progress and accomplishments were judged as satisfactory; however, several projects were observed to have approaches that needed improvement in terms of modeling/testing integration, concept validation, or industry input. Project teams were judged to be strong for most projects, with partners having demonstrated experience and expertise in the required technical disciplines. In general, reviewers felt that more effort should be devoted to quantifying and validating potential cost reductions. Lower manufacturing costs were judged to be an important rationale for continuation of the projects in the future.

The highest ranked (3.6) project was "Adaptive Process Controls and Ultrasonics for High Temperature PEM MEA Manufacturing." Reviewers considered this project to be highly relevant, with an excellent approach, substantial progress, and strong technology transfer and collaborations.

The lowest ranked (2.7) project was "Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage." This project was judged to be relevant, but it was observed that the approach (if fully successful) will have a low impact on reducing the storage system manufactured cost.

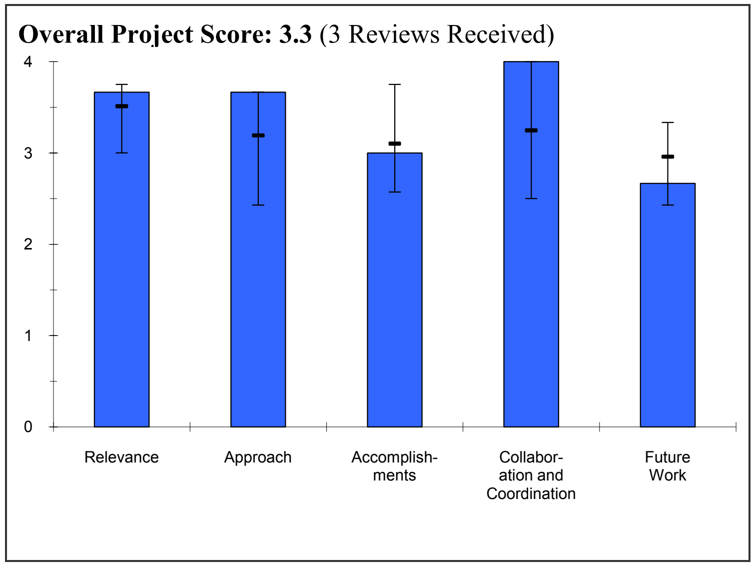
In summary, manufacturing R&D was considered to be a key element for fuel cell and hydrogen technology commercialization. The Manufacturing R&D sub-program was judged to be well-managed, well-organized, and focused on addressing programmatic performance targets.

Project # MN-01: Fuel Cell MEA Manufacturing R&D

Michael Ulsh; National Renewable Energy Laboratory

Brief Summary of Project

The project objectives are to 1) evaluate and develop in-line diagnostics for membrane electrode assembly (MEA) component quality control, and validate in-line, 2) investigate the effects of manufacturing defects on MEA performance and durability to understand the accuracy requirements for diagnostics, and 3) validate and refine existing Lawrence Berkeley National Laboratory MEA model for new application predictions of the effects of defects.



Question 1: Relevance to overall Department of Energy (DOE) objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- The need for lower cost repeat components is extremely important, as is the need for quality assurance/quality control processes.
- This project seems to be generally in alignment with DOE objectives, but would be better if it were not so disjointed. For instance, the way in which modeling is to be used to make practical improvements has not been clearly described. I do not suggest stopping the model development activity, but rather indicating how models are to be realistically validated, parameterized, and implemented to guide application of inline diagnostics.
- MEA cost and performance have been critical to competitiveness of fuel cells.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The approach is valid and should be followed.
- Conceptually, this is a good approach.
- There is an outstanding team of stakeholders involved in the process.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The accomplishments to date are as should be expected per the timeline.
- Given the time and financial support so far, it seems like the implementation of useful techniques is somewhat far away.
- The diagnostics development is focusing on key problem areas of fuel cell materials performance and cost.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **4.0** for technology transfer and collaboration.

- The list of collaborators is impressive.
- There is a strong group to provide vertically integrated input.

- The National Renewable Energy Laboratory (NREL) has assembled an outstanding team of stakeholders including both private industry and research institutions.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The proposed future work makes sense for this exercise.
- See comments above. The project should include a clearer description of how the separate components of this program are combined to result in cost effective application of the diagnostics in an inline environment.
- It is good that NREL is looking at growth rates to try to understand how possible "defects" degrade. It is also good that NREL is expanding into high-temperature proton exchange membrane (PEM).

Strengths and weaknesses

Strengths
- The list of collaborators is good.
- The team did good work on coupling the simulation of defects to the use of diagnostics to test sensitivity. It also did well with the real world demonstration plans.
- This is a team of highly skilled experts.

Weaknesses
- There is a lack of stack testing to detect and interpret the signals for the various defects.
- They need to integrate the model with hardware diagnostics and create an overall plan to provide a feedback loop.

Specific recommendations and additions or deletions to the work scope

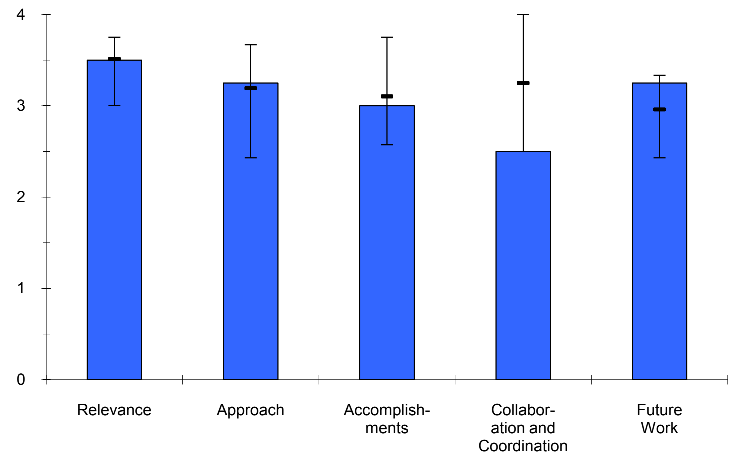
- The reviewer suggests the PIs evaluate their ability to do systems testing.

Project # MN-02: Reduction in Fabrication Costs of Gas Diffusion Layers
Jason Morgan; Ballard Material Products

Brief Summary of Project

The overall objective of this project is to reduce the fabrication costs of gas diffusion layer (GDLs) by 1) improving product quality through the use of online tools, 2) increasing the manufacturing efficiency by reducing the number of process steps and producing material at a wider width, 3) reducing process losses by improving Web-handling equipment, and 4) eliminating scrap through improved product uniformity. The goal is to produce high performance GDLs at lower cost at high volumes in the near-term.

Overall Project Score: 3.1 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project is related to polymer electrolyte membrane (PEM) fuel cell cost reduction (reducing the cost of GDLs) and improving performance/cost by understanding the relationship between process parameters and critical GDL properties, as well as identifying GDL requirements for improved performance.
- Cost reduction and optimal water transport are critical to commercialization of PEM fuel cells.
- It is exactly this type of real-world, real-scale installation that will allow fuel cells to reach commercial viability and acceptance. Good job! All of the cost models in the world do not compare to having to do the actual implementation of the process equipment and produce large quantities of usable material. This type of real-world project for further understanding of process problems and variables should be supported even more by DOE.
- The project is relevant to DOE goals. However, the inability to share microporous layer thickness and specifics regarding the GDL that the group is making does limit the usefulness and relevance to the general community.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The team's use of cost breakdown and development of new, lower cost process technology is good.
- The team showed good management, timeline, and progress. This project is very practical, yet critically needed.
- The approach is good in general. However, the group should be performing more *in situ* fuel cell experiments to validate the manufacturing changes made to the products to get better feedback throughout the project. From the way it was presented, it seems that the investigators will vary parameters first and then run all fuel cell experiments later, but this should be a more integrated process.
- It is not clear how acceptance standards for GDL materials have been established and what those standards are. It appears that the only customer involved is Ballard Power Systems (BPS). This project would benefit significantly from the involvement of other customers besides BPS.
- The team used a good approach to addressing critical processes and equipment that affect GDL quality and limit production capacity. However, since there was little detail shown concerning what defects the online inspection systems are addressing, it is difficult to assess whether the proposed approach is adequate.
- This project recognizes the need to develop a deeper understanding of the relationships among critical process parameters, the performance of the resulting GDL, and the modeling effort that should be underway if showing progress in that direction.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- This project has a large degree of "trust me" information because these statements and accomplishments are not backed up with any data.
- Much of this project is hard to grade because there is little information on many portions. For example, this project has made GDL substrate improvements. However, there is no data to document the improvement, the amount of improvement, or to show that these are state-of-art materials. The materials are not available to other developers, so there is no independent verification of these improvements, nor even polarization curves, thermal conductivity data, porosimetry, or permeability data.
- There are clear improvements in process conditions and process changes to eliminate micro-cracking.
- The team has shown cost reductions to $14/kW for the GDL at 1/5th the volume of the commercialization target. The PIs state that there is a roadmap to get to the DOE target, but it is unclear what this roadmap is. It also appears that these cost estimates only include materials and labor and not capital costs and overhead. These cost estimates are not consistent with the methodology used by, for example, Directed Technologies Inc. (DTI).
- If the DOE goal is truly to have 500,000 fuel cell vehicles in production per year, more investment into exactly this type of project is needed. There is very good progress made here and very good hands-on knowledge development.
- The technical accomplishments towards cost and process were good.
- The project would have been more interesting if more data could have been provided (like microporous layer thickness) and fuel cell experiments were run.
- The team showed good progress in reducing GDL costs toward the established DOE goal of $4/kW.
- This project appears to be on track to meet its next critical project milestone.
- It is not clear to what extent online measurement and inspection tools will be used for real-time process controls.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- There is some minor collaboration with Hickner/Pennsylvania State University —otherwise this project is entirely BPS.
- The collaborations seem lacking. The lack of an existing market for this quantity of MEAs is worrisome. Who is going to buy all of these MEAs or all of this GDL material?
- The groups worked well together.
- The collaboration among project partners appears to be well coordinated. However, this project would benefit from additional input from other customers and potential customers.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The team's future work includes more inline methodology and reducing the number of individual processing steps.
- Their future work plan is in line with current and continued progress.
- The proposed plan seemed good, but the specifics of the plan were limited.
- The plan going forward is well thought out and consistent with addressing the critical barriers to cost-effective, high-volume production methods for paper GDL.

Strengths and weaknesses

Strengths
- Real-world application development is sorely needed in the industry. Good job to BPS and DOE for funding this project!
- This project addresses critical barriers to successful commercialization of fuel cells, namely cost-effective, high-volume manufacturing capacity and effective online inspection and measurement techniques to improve GDL product quality.
- The integration of online measurement and inspection tools, and models of the relationships among material properties and performance, should lead to improved process controls and product quality.

Weaknesses
- The team could use a partnership with an auto manufacturer or another external MEA/stack supplier as an outlet for products.
- There is no other customer besides BPS involved in this project, so the acceptance standards established by BPS may not satisfy other customers' needs for GDL materials. The addition of at least one outside customer to evaluate product quality and performance would add significantly to the credibility of the project's results.

Specific recommendations and additions or deletions to the work scope

- More data need to be shown to back up the statements made.
- There should be independent verification of statements, such as statements about improved GDL materials or, at minimum, polarization curve comparisons.
- It is recommended that one or more external customers be added to the project team to provide additional input on product acceptance standards and assess the resulting GDL quality and performance.

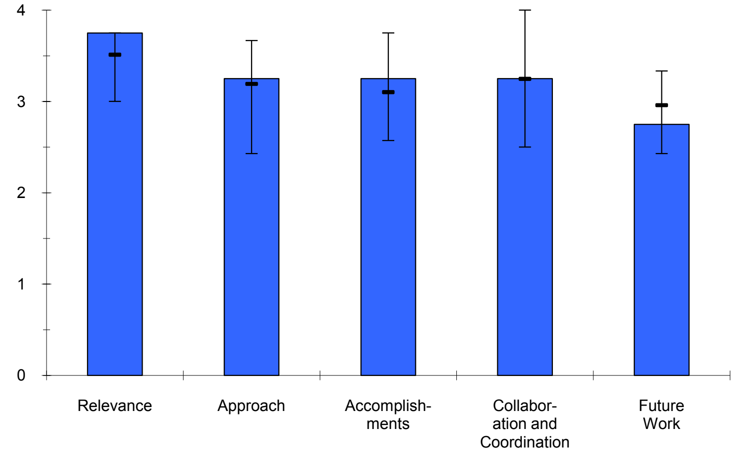
Project # MN-03: Modular, High-Volume Fuel Cell Leak-Test Suite and Process

Ian Kaye; UltraCell Corporation

Brief Summary of Project

The project objectives are to 1) design a modular, high-volume fuel cell leak-test suite capable of testing in excess of 100,000 fuel cell stack per year (e.g., 50 fuel cell stacks per hour), 2) perform leak tests inline during assembly and break-in steps, 3) demonstrate fuel cell stack yield rate to 95%, 4) reduce labor content to six minutes, and 5) reduce fuel cell stack manufacturing cost by 80%. The objectives for the past year were to 1) develop leak-test methods and 2) design and fabricate a leak-test suite prototype.

Overall Project Score: 3.3 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Although repeat components are currently the major cost driver for the stacks, as the component costs drop the labor costs will become significant. Starting the development of methods to reduce labor cost at this point in time is appropriate.
- Leak testing has high potential to reduce manufacturing costs and support high-quality manufacturing.
- The concept of online quality control is an important cost-reduction topic. This project is an initial step towards reducing the burn-in time and increasing the throughput of the production process.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The approach is sound and innovative.
- The testing process and protocols appear to be thorough.
- The testing appears to be focused on the system only after it has been fully assembled, which raises the question of whether there would be any potential benefits of testing done prior to the final system assembly.
- Project feasibility is relative to progress as the project proceeds. The reviewer thinks that for this project, feasibility is primarily based on cost savings and processing rates. Since this presentation is the second report on progress, the reviewer would have liked to see data supporting the feasibility of the original/overall objectives of the project.
- Leak detection is only performed after the system is fully assembled, meaning that rework could be a major contributor to cost.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The accomplishments to date are appropriate and impressive.
- Assuming a high-volume scenario, the type of testing appears to be so time-consuming that there would be a potential for a lot of faulty units to be manufactured while a defect was being identified and assessed. This would translate to the risk of a high level of scrappage and cost.

- This project appears to be very specific to an UltraCell product. The reviewer questions how transferrable this would be to other technologies.
- Software development by CTS in support of the leak-test suite and the controls necessary for automating the process seem very impressive. The mechanical analysis and modeling done by the Pacific Northwest National Laboratory (PNNL) also appear impressive, but it was not clear how this information was being used in the leak testing/assembling process.
- The objectives for the last year as stated in the presentation (slide 3) were met, but with respect to the milestones on the next slide, it would appear that a major objective over the last year would be the go/no-go decision based on the feasibility of 50 parts per hundred (pph), if 5 pph was achieved by February 2010. This objective is not listed for the past year and progress towards the 5 pph goal was not provided, nor was a suggested revision given to this date on the go/no-go decision. The objectives achieved and not achieved should be addressed.
- Progress is being made and the PIs appear to be meeting their objectives. We should ask the question, "Are they on the right track to reach high-throughput, low-rework quality control?" The approach does not include identification of defects prior to assembly, which is a flaw in the analytical method.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The collaboration is adequate and suitable for this type of project.
- The team has good partners, but they appear to be specific to UltraCell's needs.
- The team is working with PNNL, but the speaker was unable to discuss or explain the results of the model. The modeling results should be explained. There was no explanation of what Mound Technical Solutions, Inc. does for the program.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The future work plan is rational and appropriate.
- The speaker spent very little time on future work, though he had plenty of time left to discuss.
- The future work, as stated, includes a plan to test/evaluate and validate leak-test suite. However, on the last slide under the summary, the progress section stated that they had already tested the leak-test suite. What is the difference between testing done and testing planned?
- This section does not include anything with regards to finalizing the leak-test suite (e.g., components, tests, and procedure components) as stated verbally during the presentation.
- There was nothing listed that directly supports achieving the quantitative objectives of the project.
- The future work discussion was poor even though the speaker had time left.

Strengths and weaknesses

Strengths
- The process is much better than that used prior to this project.
- It is extremely important to increase quality and reduce fabrication costs. Therefore, this project's deliverables are very relevant to DOE objectives if enough procedural information is provided that the entire industry can benefit.
- This approach has improved the prior quality control processes and increased throughput.

Weaknesses
- The reviewer questions whether this fixture and testing process would be used in a high volume scenario. This is very good as a transitional tool specific to UltraCell's needs and priorities.
- The PI admitted not understanding the analysis or modeling work done by PNNL, but he should be able to explain conclusions derived from each effort and how this information is being applied to the leak testing/assembly process.

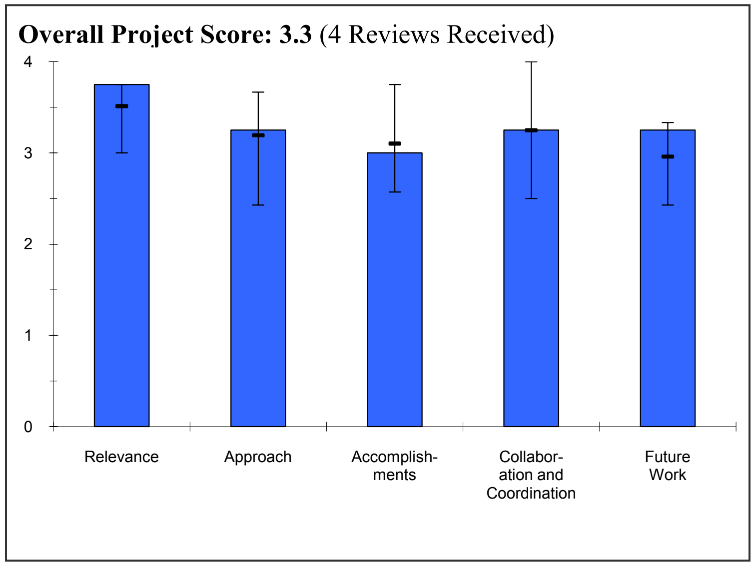
- The reviewer would suggest a time analysis of each component in the process flow chart on slide 5 as a driver for procedure revision and optimization as well as serving as a metric for the parts per hour goals. The PI said the process is dominated by the burn-in/break-in process and that the criteria for achieving this point is subject to argument, which further justifies the need for a time breakdown. What will be considered acceptable by UltraCell before a product goes out the door?
- The approach can lead to extensive rework of the process, but it is valid for the limited performance and durability of their application. The process does not appear to be valid for automotive or stationary processes and there are potential problems with the start/stop degradation with their approach.

Specific recommendations and additions or deletions to the work scope

- The reviewer would recommend they work on assessing the transferability to other products and technologies. Perhaps PNNL could have addressed this better than UltraCell had someone from PNNL been part of the presentation.
- A suggested addition would be to develop an approach for accelerating the break-in process, since this appears to be a potential bottleneck for achieving process rates.
- The team should consider how to reduce the rework process and should demonstrate that they do not introduce start/stop degradation. The PI should find a way to demonstrate that it is valid for larger (i.e., greater than 5-kW) fuel cell power systems.

Project # MN-04: Manufacturing of Low-Cost, Durable Membrane Electrode Assemblies Engineered for Rapid Conditioning
Colin Busby; W.L. Gore

Brief Summary of Project



The overall objective of this project is to develop unique, high-volume manufacturing processes to produce low-cost, durable, high-power density, three-layer membrane electrode assemblies (MEAs) that require little or no stack conditioning. This objective includes 1) a manufacturing process that is scalable to fuel cell industry MEA volumes of at least 500,000 systems per year, 2) a manufacturing process that is consistent with achieving the $15/kW$_e$ DOE 2015 transportation stack cost target, 3) the product made in the manufacturing process should be at least as durable as an MEA made in the current process for relevant automotive duty cycling test protocols, 4) a product developed using the new process must demonstrate power density greater or equal to that of the MEA made by the current process for relevant automotive operating conditions, 5) a product of three-layer MEA roll-good (anode electrode + membrane + cathode electrode), and 6) a stack break-in time that is reduced by at least 50% compared to the product made in today's process and break-in strategies employed must be consistent with cost targets.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- The objectives are well-aligned with the Hydrogen Program objectives for commercialization, including high-volume manufacturing of low-cost, durable MEAs.
- The scale-up, cost durability, high-volume cost model for MEA production was relevant.
- Projects such as this are critically needed for future commercialization of fuel cells in general. Kudos to W. L. Gore and Associates and DOE for funding this work!
- Reduced cost seems likely via this approach. Overall, the plan is well thought out including increased understanding of the underlying physical processes limiting manufacturing improvement.
- This project addresses stack manufacturing cost by reducing process steps, scrap, and non-reusable materials (liners). Investigators should quantify the potential for process rate improvement of the direct, two-sided coating over current methods. The investigator claims durability and stack-conditioning time improvements. These must be substantiated going forward.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- This project has a solid approach to reduce MEA and stack costs. The project is clearly on the appropriate development path with the high-volume, full-width MEA production with minimal coating passes.
- This project also includes what can be considered peripheral or side projects including modeling of heat and water management and modeling of mechanical membrane and electrode properties for MEA stress prediction related to temperature and relative humidity cycling scenarios. It is very unlikely that these side projects will affect the outcome and development of the main project (Gore) and there is little/no information about how these projects will feed back into the core project for MEA production.

- The project showed good use and combination of modeling with real-world application.
- Successful implementation of direct coating should reduce catalyst coated membrane cycle time. Investigators have identified an alternative path, if the primary path is not successful, although improvements based on an alternative path must be quantified. The technical challenges of an alternative path, such as membrane-membrane bond cycle time and interfacial strength, must be quantified if this path is taken. The incorporation of mechanical and transport modeling should aid development. Investigators should verify the relevance of uniaxial stress testing to stress conditions during fuel cell operation.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Most of the results to date appear to be performance-related verification with little information on the progress for cost reductions. Most of the performance-related data appears to be on Pt loadings significantly beyond the 2010 DOE targets, but not approaching the 2015 DOE targets, and there is no path forward shown on how the PI intends to get to the target loading.
- The team is still somewhat early in project cycle, but the members are making good progress in getting equipment designed and installed, models in place, and the initial testing done.
- No miracles have occurred, but the progress is well within target expectations.
- Equipment installation and commissioning is a big step forward. Two-sided coating has been demonstrated, which is an impressive accomplishment. Performance testing looks to be headed in the right direction if transport issues can be solved. Investigators should work toward an understanding of the cause of the transport issue (e.g., electrode porosity, hydrophobicity, gas diffusion layer (GDL) properties).

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The work with Pennsylvania State University (PSU) and University of Delaware was discussed in terms of approach, but no results from other partners were offered.
- I do not understand why parts of this project exist. This project's objective is to reduce MEA production costs. However, two partners are exclusively modeling either durability or mass transport. There are much bigger, more complete projects in these areas and there were specific calls for those types of work. How do these efforts fit in with a manufacturing proposal? These efforts, while they may be good, should have been proposed through the specific calls from DOE for durability work and water management modeling.
- There appears to be little collaboration achieved to date. However, project collaborators will likely come in now that the baseline setup and validation is nearly complete.
- Even though the number of collaborators is fairly small, the effort seems very well coordinated. There also do not seem to be wasteful activities by partners with weakly related tasks.
- The team's collaborations appear to be appropriate and the partners exhibited good use of modeling and university participation.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Stack testing and water management measurement and testing are very good plans. The reviewer is looking forward to seeing real-world production data and analysis!
- This project addresses the current issue of water management. The reviewer hopes to see examples of uses of models at the next review and would also like to see more detail on the implementation of new processes.

Strengths and weaknesses

Strengths
- The team showed a good approach and plan.

- The team had a clear target, method, and integration.
- The project looks to advance process technology and thereby reduce manufacturing costs. Modeling efforts support the main process development task. In particular, investigators should be praised for bringing on new project partner (PSU) to address the emerging issue of water transport.

Weaknesses

- There appears to be weak collaborations to date, but that may be better defined as the project progresses.
- There are no large weaknesses. The risk is moderate, but this seems appropriate for this sort of program.
- The potential rate improvement of new processes should be addressed. In other words, a 25% cost reduction is laudable, but it does not by itself address scale-up to high volume (i.e., the barrier listed in the Overview slide).
- Improvements in durability and conditioning time must be verified.

Specific recommendations and additions or deletions to the work scope

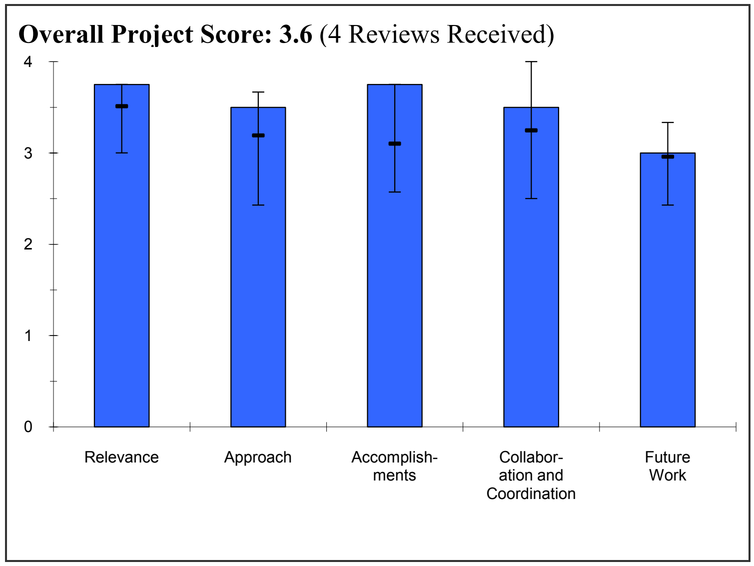
- This project includes what can be considered peripheral side projects, such as modeling of heat and water management and the mechanical stress modeling. While these are fine tasks, they do not seem to fit in with the solicitation call for manufacturing technologies. There was a separate call for water management studies. It is unlikely this work will lead to a manufacturing cost reduction in the short period of this project.
- This project needs to move quickly to DOE target loadings for 2010 and 2015. The work shown is at loadings significantly beyond DOE targets. Regardless of other improvements, this project will not meet long-term cost targets for MEAs.
- The investigators did not address the variability in electrode uniformity or changes in requirements for quality control as a result of the new process, particularly when higher rates are achieved. Therefore, the investigation of quality control needs is warranted.

Project # MN-05: Adaptive Process Controls and Ultrasonics for High Temperature PEM MEA Manufacture

Raymond Puffer; Rensselaer Polytechnic Institute

Brief Summary of Project

The high-level objective of this project is to enable cost-effective, high-volume manufacture of high-temperature (160º-180ºC) polymer electrolyte membrane (PEM) membrane electrode assemblies (MEAs) by 1) achieving greater uniformity and performance of high temperature MEAs by the application of adaptive real-time process controls (APCs) combined with effective *in situ* property sensing to the MEA pressing process and 2) greatly reducing MEA pressing cycle time through the development of novel, robust ultrasonic bonding processes for high-temperature PEM MEAs. This year, the project will focus on process optimization, initial APC implementation, stack testing, and low-temperature ultrasonics.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This project demonstrates a great application of new technology to fuel cell MEA manufacturing. The reviewer gives kudos to Ray Puffer, Rensselaer Polytechnic Institute (RPI) and DOE for funding and working on a project with real-world benefits to fuel cell commercialization.
- This project has potential to support a substantial cost reduction in MEAs.
- High-temperature PEM MEAs will likely play a major role in fuel cell system commercialization. The adaption of manufacturing and quality assurance methods relevant to phosphoric-acid-doped polybenzimidazole (PA-PBI) membranes and MEAs will provide an important contribution to DOE's and the industry's commercialization goals.
- APCs have the potential to increase MEA manufacturing quality and, therefore, reduce cost.
- The project would have more relevance if it had a low-temperature MEA supplier as a member of the team.
- The project is focused on solving critical issues for PEM and high-temperature PEM fuel cell manufacturing. The project targets one of the most important bottlenecks in the production of PEM fuel cell systems.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- Technical barriers were well addressed. It is somewhat unclear how scale-up will work and if the technology is applicable to roll-to-roll processes.
- The project hopes to improve the uniformity and performance of UltraCell's high-temperature MEAs through adaptive process control over the hot pressing cycle. A second objective is to reduce the cycle time for the pressing operation through the use of ultrasonic welding techniques. UltraCell has had difficulty in achieving consistent performance from its fuel cells and it is not clear that this effort will improve the consistency of the fuel cells. A better understanding of the cause of the performance variability is needed.
- The approach is correct and addresses high throughput in the manufacturing of high-temperature PEM fuel cells. The PI shows an excellent understanding of the manufacturing process and the approach correctly addresses the manufacturing issues for MEAs.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.8** based on accomplishments.

- The project has made very good progress and results so far, especially since the project is still relatively new.
- Ultrasonic welding can be transformative in terms of MEA cost.
- The project has demonstrated the feasibility of ultrasonic welding with a cycle time of less than one second as compared to about one minute for thermal bonding.
- BASF Fuel Cells (BASF) has identified the reasons for the slight departure of the ohmic region of the polarization curve; this was due to test fixture variations.
- The cost analysis showed potential cost savings from the ultrasonic fabrication process.
- The durability of MEAs made with an ultrasonic process needs further confirmation.
- The technical accomplishments are very good. Many of the program targets are met or the program will meet these targets in the near future.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- There are good collaborators for high-temperature phosphoric acid fuel cells. It makes sense given that the technology is seeing some success now. The team could use better collaboration with major low-temperature PEM/MEA manufacturers as well. Perhaps this could be a future goal as lab-scale work is completed on low-temperature PEM sealing.
- The project has a strong team of partners.
- A solid list of industry and academic partners are on this team. BASF Fuel Cells is an important team member because they supply the high-temperature MEAs for UltraCell.
- The program has established a good team of collaborators. Many of the collaborators have only started their efforts and most of the work load has been done by RPI.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- It is not clear how this technique will scale to larger MEAs. Is it applicable to roll-to-roll processing or is it limited to a stop-and-go, continuous batch (stamping) process?
- The project's proposed future research is appropriately directed.
- A schedule is not presented. A more detailed look at future work could help to dispel the comment that the project is too long.
- There is little indication of the timing of and interaction between future work elements, particularly for low-temperature MEAs.
- It is still not clear why the project is four years long.
- The proposed future work is consistent with the objectives. The performance and durability of the ultrasonically welded components will be a measure of the success of the program.

Strengths and weaknesses

Strengths

- There is real potential to greatly reduce MEA preparation cycle time, which would lower MEA processing costs and introduce online testing capability.
- The results to date have been strong and very relevant to meeting DOE targets for cost and performance.
- This team showed good organization. The ultrasonic welding technique may be applicable to low-temperature MEAs if it proves successful at reducing cost.
- Early results look promising.

- The PI and the facilities at RPI are the primary strengths of the program. The experience at this facility makes it the leader in manufacturing R&D for MEAs and high-temperature PEM components.

Weaknesses

- There is a lack of low-temperature fuel cell or MEA supplier information to confirm applicability of these techniques to low-temperature systems.
- The *in situ* impedance measurement technique needs further confirmation of its applicability to high-rate manufacturing.
- There are no obvious weaknesses.

Specific recommendations and additions or deletions to the work scope

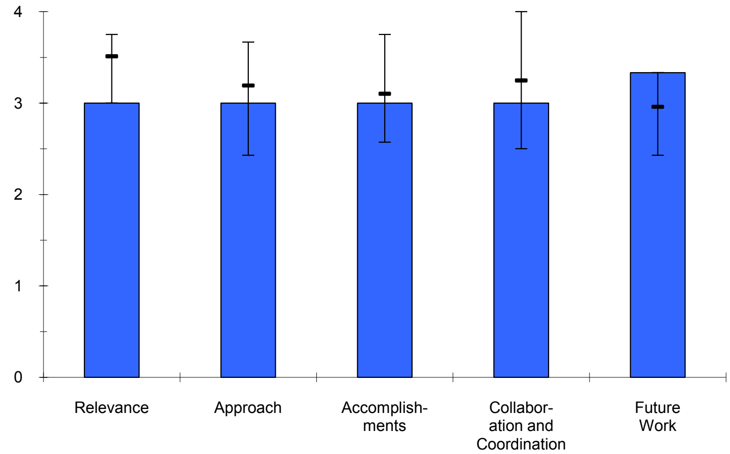
- The reviewer suggests adding a low-temperature MEA manufacturer as a collaborator.
- The team should provide a better definition of scale-up plans and risks.
- The team should have a dialog with low-temperature MEA suppliers to determine the feasibility of adapting this process to their systems.
- The project is having early success. If the rate of achievement continues, DOE may wish to revisit the objectives and funding and increase both.

Project # MN-06: Metrology for Fuel Cell Manufacturing
Eric Stanfield; National Institute of Standards and Technology

Brief Summary of Project

The objective of this project is to develop a knowledge base of engineering data relating performance variation to manufacturing process parameters and variability. The approach is to fabricate experimental cathode-side flow field plates with various well-defined combinations of flow field channel dimensional variations. Then, it quantifies the performance effects, if any, and correlates these results into required dimensional fabrication tolerance levels.

Overall Project Score: 3.0 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project was separated into three separate tasks: P1, P2, and P3. P1 was "Cause-and-Effect: Flow Field Plate Manufacturing Variability and its Impact on Performance." P1 missed the mark by sticking to 25 psi and did not measure appropriate feedback that would be useful to the industry. P2 was "Non-Contact Sensor Evaluation for Bipolar Plate Manufacturing Process Control and Smart Assembly of Fuel Cell Stacks." P2 is a generic look at existing measurement devices, so there is some usefulness for newcomers to the industry. P3 was "Optical Scatterfield Metrology for Online Catalyst Coating Inspection of PEM (Fuel Cell) Soft Goods." P3 seems to be the most useful project. The project appears to support the overall objectives of this program.
- The project has relevance to the DOE program objectives to reduce the cost of fuel cells. Successful completion of the project could establish the link between component (i.e., plate and catalyst coated membrane [CCM]) characteristics and performance and durability of a cell or stack.
- This project is likely to provide pre-competitive information that the entire industry can use to help achieve DOE's ultimate objectives.
- The initial part of the project characterizes the performance of the PEM fuel cell as a function of the design of flow fields. It is not clear that this is a measure of manufacturing capability. If this data can be correlated with quality control measurements, then the project is relevant.
- The development of quality control sensors is important. The work has to progress to full-size systems.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- P1 shows inadequate test parameters to generate a useful design table. It had a good intent at the outset, but it lacked industry input into what should be measured and varied. P2 shows good work was done to evaluate a range of equipment and focused on feasible techniques that industry can use.
- P3 shows that technical barriers are being well addressed, and this project could have a great impact if inline operation can be achieved. The activities are science-based and appear promising. The main suggestion here is that the overall perspective be better spelled out. Some activities seem to have limited impact, perhaps because of insufficient order of magnitude analysis such as the aspect ratio limitation.
- This project is a collection of three separate tasks that may not have a great deal of interaction, but success in any one of the areas will improve manufacturing processes for fuel cell components.
- The non-design-specific nature of the work is good.

- An evaluation of non-contact measurement techniques appears to be applicable to plates, electrodes, and membranes.
- Optical scatterfield metrology shows promise for determining Pt in real time.
- There are three tasks in this topic. Two of the tasks, P2 and P3, are in the outstanding category. P1 is a good program. Overall, the rating was outstanding.
- The development of quality control technology is systematic and builds on previous successful experience. The project has a strong focus and should provide a benefit to all MEA and fuel cell component manufacturers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- P1 missed the mark because of bad inputs used in design of experiments.
- P2 shows pretty good work has been done to provide newcomers to the industry with an array of techniques; however, it offers no breakthrough work.
- P3 shows progress is going well but a little slow. It would be nice to see more work on calibration. For example, what does this feedback on X wavelength show on a known sample?
- Much activity appears to be ongoing. However, the presentation makes it difficult to assess progress with much certainty.
- There was initial skepticism concerning the applicability of the optical scatterfield technique to measuring platinum loading. The initial results with the scatterfield microscope are in agreement with 3M's in-house analysis for loadings of 0.1, 0.15, 0.2 g Pt/cm^2. The National Institute of Standards and Technology (NIST) has also looked at traditional catalyst layers. So far this technology still holds promise.
- The Optical Scatterfield Metrology is a higher risk activity that can provide great worth. The demonstration of angle-resolved scatterfield microscopy indicates the instrumentation will have sensitivity to 0.01 mg Pt/cm^2. This is a critical milestone, and if it had failed, it would have set the project back or even stalled it completely. The data from the scatterfield microscopy are very promising.
- The non-contact sensor activity shows promise to be a quality control measurement of flow field depth and width. The speed of the process was not discussed and is an important factor.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- P1's lack of broad industry support on inputs was a miss.
- P2's evaluation of existing instruments was well performed. P3 shows good leverage of different organizations. It is good that informal interactions seem widespread. However, it is not clear how deep the interactions are, how beneficial to the project they are, and whether feedback exists.
- The collaboration is good, with numerous interactions with manufacturers.
- This is a good team for P1. It was unclear who the collaboration was with for P2, which looked like a shopping list of organizations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- On P2, it seems like not much breakthrough going on. There is a lot of inspection equipment available. It would have been nice to determine the most important parameters to measure, and which needs less measurement, as a means of speeding up throughput. Overall, the effort is pretty well thought out and can help newcomers to the industry.
- P3 is the most interesting project. P1 seemed to miss the mark on what could have been a noble project.
- More specificity would be very helpful in evaluating the plan. That said, it appears that the next goals are well considered.
- The proposed future work is clearly laid out and includes relevant go/no-go decisions points. The reviewer gives kudos for the clarity of the presentation in this regard.

- The future work continues the successful efforts already achieved by P2 and P3. Future work for P1 does not open new doors, and it is not clear if the subtle differences in flow fields will be observed in this level of test.

Strengths and weaknesses

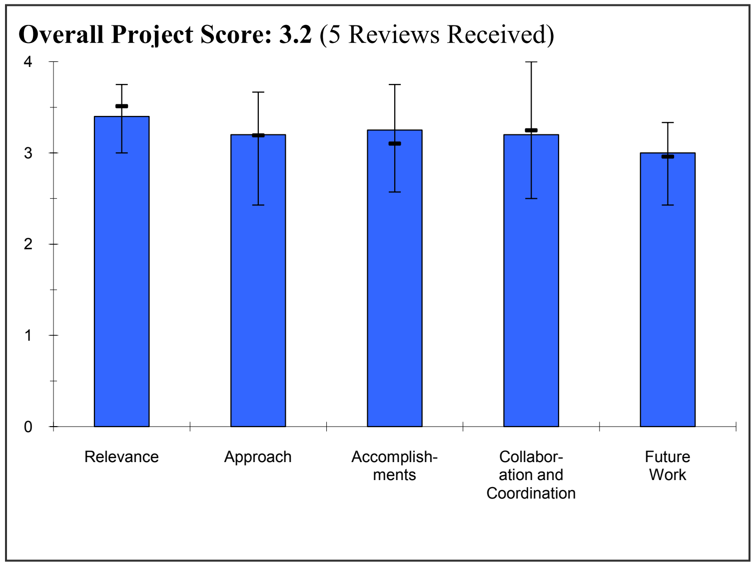
Strengths
- On P3, the optical online metrology for catalyst coating inspection is a strength. It seems interesting and could have a huge impact. Some further information on estimated process times and equipment costs should be provided, as well as how the implementation on inline process could be accomplished. A comparison of the project accuracy with known values or to what the manufacturers say is there would be interesting to know. It would also be beneficial to know if this can pick out size distribution.
- The project uses a science-based approach and focuses on eventual application of technologies.
- NIST is the most logical place to develop online metrology instrumentation in a pre-competitive environment. Successful outcomes would benefit the entire fuel cell industry.
- The presentation was very good. It clearly delineated past progress (FY 08) from more recent accomplishments. The performance schedule with appropriate milestones and decision points was well done.
- The primary strength is NIST, which is a high-caliber, well-focused laboratory. The PI has assembled an innovative approach for developing inline quality control. The approach and the overall program are well focused.

Weaknesses
- On P1, regarding the cause and effect of manufacturability on the cathode, it is important to touch on the impacts of real-world cathode conditions. The 25 psi pressure is not realistic, and lower pressure could have a huge impact on cathode design or even under different utilizations. It seems like many manufacturing-induced issues, such as roughness and warping, are not included, nor are the measured parameters (ones that would be critical to systems integrators) collected. These parameters include pressure drop and correlation to different geometries such as serpentine, parallel, etc. The reviewer does not think this will result in a useful design table for bi-polar plate manufacturers.
- The overall approach and risks were not well presented. The speaker attempted to cover far too much information with the result that much information was lost. In the future, please speak more clearly and spend a bit of time on context.
- Progress has been sporadic due mainly to funding issues.
- P1 should be reviewed. It is not clear that the sensitivity of the electrochemical methods used will be sufficient for subtle changes in the flow fields.

Specific recommendations and additions or deletions to the work scope

- P1 should try to add lower pressure cathode and solicit more information on required feedback parameters from industry.
- P2 should publish a summary for the use of measurement techniques for general industry use into industry manufacturing publications.
- For P3, be sure to keep industry in the loop so as to not repeat the mistake in P1. It would be good to provide some idea of how an inline process would look along with cost. Be sure to map out "the machine showed X, Y on the following: manufacturer supplied gas diffusion electrodes (GDEs) with A, B loadings," and see if catalyst particle size distribution can also be determined.
- The project should start to consider full-size bipolar plates and plates that may be slightly warped. Teaming with a research laboratory that can do segmented cell research may improve P1.

Project # MN-07: High Speed, Low Cost Fabrication of Gas Diffusion Electrodes for Membrane Electrode Assemblies
Emory De Castro; BASF



Brief Summary of Project

The overall objectives of this project are to 1) reduce cost in fabricating gas diffusion electrodes (GDE) with a focus on GDEs used for combined heat and power (CHP) generation, 2) relate manufacturing variations to actual fuel cell performance to establish a cost-effective product specification within six-sigma guidelines, and 3) develop advanced quality control (QC) methods to guide realization of the first two objectives. The objectives for FY 10 are full-length roll coating and online measurement of Pt level and distribution (advanced QC).

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project is related to polymer electrolyte membrane fuel cell (PEMFC) cost reduction (reduce cost of GDEs and membrane electrode assemblies (MEAs)) and improving performance.
- Cost reduction of the MEA/electrodes is critical to commercialization of PEMFCs. This project appears to be concentrating on a technology specific to BASF, which is phosphoric acid-imbibed PBI. So, it has limited impact for the rest of the fuel cell community.
- This project is very well aligned to DOE objectives, assuming DOE objectives are to promote and implement mass-production of high-quality MEAs. Effective and fast inline QC testing will be critical.
- This project addresses mainstream needs for improved cost and quality metrics set by DOE.
- The work outlined in this program is required to meet the performance and cost targets for fuel cell commercialization. Specifically, it is critical to have uniform Pt distribution, especially as the overall loadings are decreased.
- This project supports the DOE program objectives. Its success would lead to manufacturing cost savings.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The approach includes developing high-speed, continuous production and incorporation of inline sensors for quality. The focus is on GDEs for CHP applications, which limits the applicability for other larger sectors such as transportation.
- This project has a very good approach. X-ray fluorescence (XRF) is a very powerful tool if applied and calibrated correctly.
- This project shows a good mix of scientific metrology improvement and practical demonstration of process improvement.
- The overall approach of improving line speed and quality control is solid and should be effective. It would really be helpful to understand how tight of a coating tolerance is needed to balance performance and cost. For example: a report chronicling the cost difference if the loading is 1% high, 5% high, or 10% high would be helpful to know. The impact on performance if the loading is 1% low, 5% low, or 10% low would be very interesting to note, as well.

- The approach is to develop the capability for inline measurement of Pt level and distribution, and porosity.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Modified XRF technology for inline Pt analysis for quality measurements is an accomplishment and shows progress.
- This project has made very good accomplishments so far and exceeded expectations for Pt measurement in a real fashion by XRF. It has already demonstrated sensitivity down to a Pt loading of 0.0017 mg/cm^2, which is well below DOE target loadings. The assumption is that the system works even better at higher Pt loadings.
- It appears that the individual tasks are on schedule and have good prospect of success.
- Considering that it is early in the program, there has been a significant amount of progress. Reaching the design goals of speed, accuracy, and operator safety is a great accomplishment. There appears to be some work left to improve the uniformity of the coating, but the overall progress is good. It will be very interesting to the see the results of the modeling work on how defects in Pt coating will impact cell performance.
- Measurement of Pt loading down to 1.7 g/m^2 has been achieved ahead of plan. BASF believes that they can measure down to 1 g/m^2 with parameter optimization. This was accomplished with modified optics on commercial XRF units.
- The project identified an additive that stabilized cathode ink and reduced agglomeration, resulting in more uniform coating.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The PI is collaborating with Case Western Reserve University for modeling of variation and defects, a baseline model was done.
- Other collaborations appear to be just starting.
- Collaboration is fairly good, and there seems to be more of a supplier/customer interaction, particularly with the supplier of the XRF system.
- Collaboration with other government laboratories and industry is appropriate.
- The roles of each partner are clearly defined and it appears that they are working well together.
- Two collaborators were identified; both play a significant role in the project activities.
- Some informal collaboration is indicated between BASF and Rensselaer Polytechnic Institute (RPI)/Center for Automation Technologies and Systems (CATS).

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Concentration of online analysis methods for production is recommended.
- The PI needs information on project cost and cost reductions.
- PI has a very good plan. The on-roll porosity measurement could be very beneficial as well. The PI has a good plan to test for the effects of defects. The PI could add testing of alternate materials including catalyst coated membranes (CCMs) or other GDE materials.
- The overall plan still seems reasonable and can be accomplished in the proposed timeframe.
- Overall, the future work is clearly defined and should be effective in reaching goals. From the talk, it sounds as though the program may require some work on improving the ink mixing process for commercialization, which was not specified as a future goal.
- Future work for next year appears appropriate and, if successful, will demonstrate a doubling of line speed on a full roll. Accomplishing this is a critical go/no-go decision point.

Strengths and weaknesses

Strengths

- The project directly addresses DOE's goals and the overall goal of practical, commercially-produced fuel cells.
- The project has a well-integrated mix of high- (or medium-) risk elements and low-risk production improvements. The project is likely to provide overall progress toward DOE goals and may provide significant progress.
- BASF has a strong track record of producing GDEs and their work shows a good understanding of what is required to reduce cost and improve uniformity. The project is structured well, with clear goals and reasonable deadlines.
- The project is a focused one.

Weaknesses

- The team needs information on the projected cost reduction.
- Catalyst loadings are not discussed except for analysis less than 1 mg Pt/cm^2. There needs to be a concentration on meeting the platinum loading targets for 2010 and 2015.
- Collaborations could be stronger. BASF may consider including testing with an external GDE or CCM supplier.
- It is not immediately clear how much the work being done directly impacts cost. It would be helpful to understand where the current levels of production and cost are, and where they will be at the end of this project. It would be helpful to show performance data, especially as BASF attempts to move from carbon fabric to carbon paper.

Specific recommendations and additions or deletions to the work scope

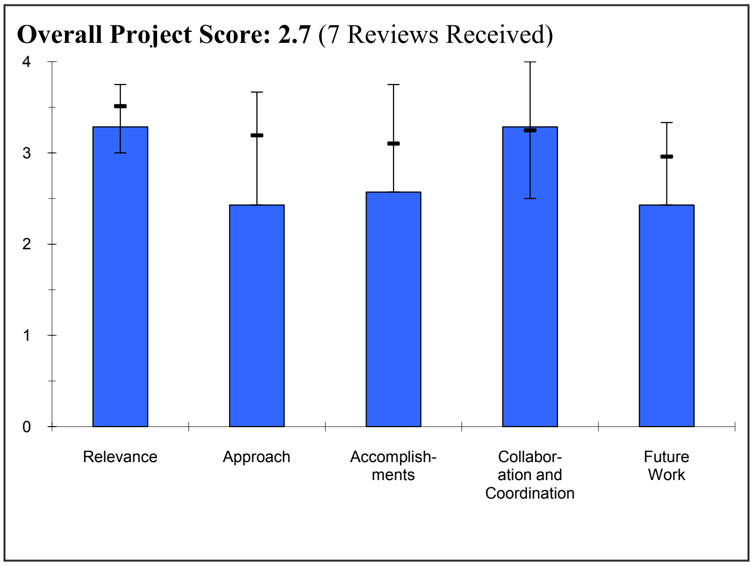
- The team could possibly extend the XRF measurement to other nuclei, F, S, or other metals like Pd or cobalt.
- Is there a possibility to extend this technology to other GDL materials?
- BASF could extend testing to purposely defective GDL material to test the effects of holes, bumps, and other non-uniformities.
- Given the team's successes, it may be good to include additional collaborators such as the RPI/Puffer project (MN005) for ultrasonic sealing of GDEs into MEAs.
- The project should include some research to determine if the technology could be applied to non-fuel cell applications.
- The reviewer recommends continued collaboration with RPI/CATS.

Project # MN-08: Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage Vessels

Mark Leavitt; Quantum Fuel Systems Technologies Worldwide, Inc.

Brief Summary of Project

The overall objective of this project is to manufacture Type IV hydrogen storage pressure vessels, utilizing a new hybrid process with the following features: 1) optimal elements of advanced fiber placement and commercial filament winding, 2) reduced production cycle times by adaptations of high speed dry winding methodology, and 3) improved understanding of polymer liner hydrogen degradation. The project goal is to achieve a manufacturing process with lower composite material usage, lower cost, and higher efficiency.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This project goes straight to improving composite tank efficiency and manufacturing cost by intelligent winding methods and understanding stress-strain relationships for the application of the windings that enable the optimization of the application, while minimizing the material, thus leading to a reduction in cost.
- The project is very relevant to the DOE Fuel Cell Technologies Program as it seeks to reduce the cost of high-pressure hydrogen storage tanks, which is the only technology currently available for storing hydrogen on board a vehicle. Reductions in cost and weight are important, even if the storage density will not be improved much.
- The project is as much a product development effort as a manufacturing process improvement project.
- Cost goals are not clearly defined. The project appears to be of the nature of “let's see how much cost we can take out of our commercial tank.”
- Pressurized hydrogen will likely be a long-term option for vehicles, and this project is very relevant in that it can reduce weight and cost below the already adequate values.
- Storage costs are an important element with respect to enabling cost-effective, on-board hydrogen storage and, therefore, hydrogen vehicles.
- The overall aim of the project is relevant, which is a manufacturing process with lower composite material usage, lower cost, and higher efficiency.
- The project addresses increasing the manufacturing rate of hydrogen storage vessels. The project did not appear to address cost reduction of the materials as a major effort even though the materials have the greatest contribution to cost. From chart 13 of the presentation, the materials cost is 97% of the tank cost. That seems to imply that the program only addresses 3% of the cost.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- The application of optimized stress-strain relationships combined with computer numerical control processes that enable precise winding placements is very nice. The scale-up to commercial scale was kept in sight as the project evolved.
- The approach to performing the work is generally rational and appropriate for addressing barriers. However, a clear path to meeting DOE targets was not presented.

- The presentation contains one slide concerning stress analysis, but it is not clear how this analysis is being used to guide development efforts and provide for sufficient safety factors.
- The project will also examine a new approach to tank fabrication being developed by Lawrence Livermore National Laboratory (LLNL). This task was well described, but it is not clear how it integrates into the overall project. Even with a new fabrication process, given the high cost of fiber, it is not clear that a new process will significantly decrease cost to justify this approach.
- Dry tape winding shows good promise for cost reduction. The approach to advanced fiber placement (AFP) with filament winding (FW) is well thought out and executed since last year through a tank test.
- The approaches to storage cost reduction do not seem to have the potential to yield significant progress towards the storage cost targets. It is not clear that liner blistering is a significant problem, and the cost reduction potential of dry tape technology is insufficiently characterized.
- The approach of this project to reduce the cost of a Type IV pressure vessel through advanced fiber placement, dry winding, and liner degradation needs further development to assess the potential and variability in the cost savings. The additional complexity of the fully integrated fiber placement appears to have increased the processing time. The statistical variation of the input parameters should be included in the cost model, including the composite usage ranges for a high-volume production process. The project presentation should identify the clear deliverables and quantifiable goals for each element of the project (i.e., the liner compatibility's contribution to cost reduction).
- The approach is misguided since the program only addresses a small fraction of the storage vessel cost.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- The technical accomplishments for this project are good. The reviewer thinks more attention needs to be given to techniques to further reduce the costs of the material and quantity (i.e., optimization of windings). If this work were to really achieve its goal of approximately 20% reduction, it would be a significant achievement.
- A cost analysis was presented that showed that material costs dominate. If the cost model that should have been developed by the Pacific Northwest National Laboratory (PNNL) by this time is sufficiently robust, it could be used to guide the effort to promising pathways. This was not discussed.
- Boeing has made good progress demonstrating the AFP technique with their tooling.
- A schedule was not presented and it was unclear whether there are any go/no-go decision points between now and the end of the project.
- The combination of FW and fiber placement trades material cost for capital and processing cost. The analysis shows that the tank cost is projected to be only about 10% lower with the advancements. Any added equipment or processing costs were not described. It really seems questionable whether the cost reduction justifies continued efforts in this area.
- The liner work did not show a clear path to resolving the blistering with cycling results.
- The building and testing of the first AFP and FW tank is a valuable step.
- In reference to the cost analysis on slide 13, additional focus on the composite strength would be valuable, if separate FW and AFP will always be the lower cost option.
- LLNL had a thin-film, aluminum-coated, polymer-lined composite tank 15 years ago. The aluminum coating seems to have been removed in this version, though the coating may alleviate the polymer blistering issue. With a projected cost of more than $20/kWh (and goals set at $4/kWh or lower), the projected cost savings of 11% is clearly insufficient to contribute to reaching DOE targets. The effort has clearly identified material cost as the most significant driver, and reducing the amount of expensive material seems less fruitful than identifying suitable material that costs appreciably less.
- The project shows good results to date regarding the expected manufactured costs of systems.
- The accomplishment of demonstrating a hybrid vessel with the composite usage reduction is very good. The accomplishments in the area of liner compatibility and dry winding appear to have made progress but could have been explained further.
- The technical accomplishments are positive with the AFP showing good results. Tank cost analysis is detailed but demonstrates the problems with this research program. The materials costs are so high that manufacturing improvements will be a minor improvement to cost.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The partners appear to be working well together, as they each bring unique strength to this project.
- The team assembled has the technological expertise to carry out the program. Boeing has tremendous experience in fabricating composite structures for the airline industry. Boeing is attempting to adapt and scale down the processes to tank fabrication requirements.
- The inter-dependence of the work by the partners was not entirely clear.
- Very good coordination among team members matching the division of responsibility with the general program progress.
- The coordination between Quantum and Boeing seems well thought out and executed. The efforts by the national labs seem to be disconnected and “thrown in.”
- The collaboration with Boeing seems to be appropriate and essential for the AFP task. The role of the other project partners could have been expanded.
- The team is skilled and able to meet the objectives of this program.
- The team does not include a materials manufacturer that would be able to reduce the tank cost significantly.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- The reviewer would like to see increased emphasis placed on material cost and utilization reduction. While the effort on manufacturing time and utilization of plant equipment represents a small fraction of the overall tank cost at volume, the effort is necessary to improve throughput and eventually reduce cost. This effort should continue.
- While the elements of future work are identified and seem appropriate, there is little indication of the interaction between some of the elements, and timing is unclear. Decision points are not clearly defined.
- The AFP process does not seem to be able to deliver sufficient cost savings. Most of the future work revolves around the AFP process. The only seemingly useful future work item is the evaluation of alternative materials.
- The next steps for the AFP appear focused, though the potential of this method should be further developed and validated prior to extensive physical testing. The future work for the liner assessment and dry winding could include further details.
- The reviewer thinks the effort is misdirected and should address material cost reduction.

Strengths and weaknesses

Strengths

- This project does a nice job in applying science and optimization to the cost reduction of carbon-reinforced high-pressure tanks without compromising functionality (i.e., maximum allowable working pressure and fill time). Continued effort on winding optimization and manufacturing speed will help to reduce the costs.
- The partners appear to be working well together.
- The teaming is strong. There are good and relevant histories for composites and tanks with Quantum Fuel Systems, Boeing, and LLNL.
- The PNNL hydrogen compatibility study can translate to other polymer/composite hydrogen pipeline developments done elsewhere.
- This project could provide incremental improvements in cost and manufacturing of Type IV pressure vessels.
- The strength of this project is the focus on searching for opportunities to reduce the cost of the pressure vessel, which is the main cost of a compressed hydrogen system.
- The manufacturing team is experienced and capable of fulfilling the objectives.

Weaknesses

- The project should focus more of its efforts on those elements that show the biggest promise for continued cost reduction at medium- to large-scale production volumes. Do not stop the efforts on those elements that do not have as significant of an impact, but focus efforts on the big ones.

- So far, the cost of materials dominates the manufacturing cost of the tanks and calls into question the value of pursuing manufacturing improvements that provide only incremental improvements.
- There is minor concern on the accuracy and stability of the cantilevered, full-head access for AFP end wraps.
- The project is only projecting meager cost improvements. The go/no-go point should incorporate a sufficient material hurdle for cost reduction.
- It does not appear from the information provided that Quantum will be able to meet long-term DOE targets.
- The weakness of the project is the lack of theoretical potential analysis prior to the physical demonstration and development. It may have been conducted but simply not included in the project overview.
- The benefits to be developed will not significantly reduce the cost of the hydrogen storage tank. The project does not emphasize material costs.

Specific recommendations and additions or deletions to the work scope

- A better definition of the project schedule and any future decision points is needed.
- Information on permeation through liner materials should be shared between this project and the Lincoln Composites project. It would benefit both efforts.
- Since this project is for the development of a unique, vehicular hydrogen storage pressure tank, the project team should report on the process of industrial practices, codes, and standards that need to be developed for certification of this tank technology and design. It would be good for the team to engage the Department of Transportation or Canadian Standards Association and a nationally recognized testing laboratory in the process ahead of time. If this is not an issue, that is great, but the reviewer suspects it is. If it is, it can be a cost driver and a very serious barrier to commercialization and should be listed as such in the overview.
- The reviewer would recommend scrapping future AFP work and investigate alternative materials.
- The reviewer recommends including an assessment of potential areas of improvement with pressure vessel manufacturing to ensure the project is focused on best potential candidates for cost reduction.
- The program objectives should be evaluated. The benefits of manufacturing improvements should lead to a 30%-50% cost reduction. If the reduction in cost is considerably less than 30%, the project should be terminated and funds redirected to storage programs that can produce a 50% cost reduction.

2010
Technology Validation
Summary of Annual Merit Review of the Technology Validation Sub-program

Summary of Reviewer Comments on the Technology Validation Sub-program:

Reviewers consider the Learning Demonstration work to be very relevant to the DOE's objectives of conducting independent assessment and dissemination of fuel cell vehicle (FCV) information and providing real-world feedback to researchers and partners to improve technology. The information gathered will help improve technology readiness for FCVs and lead to successful market introduction. This project validated the performance of the technologies under real-world conditions.

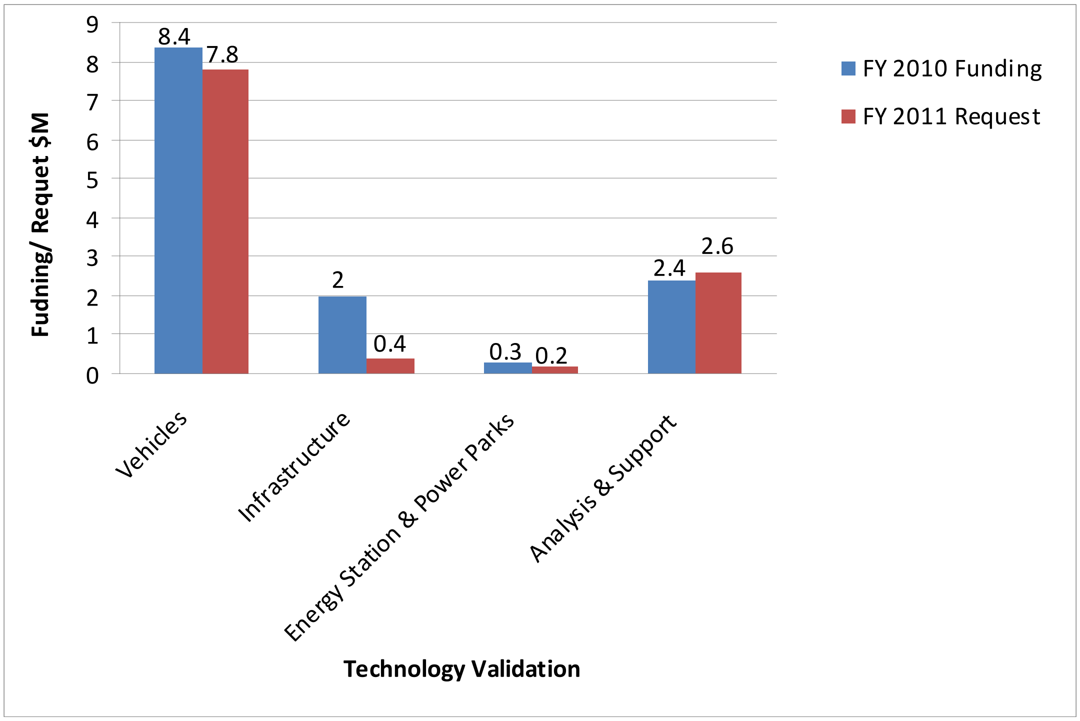
Reviewers believe it is critical that DOE work with the FCV automakers to validate their first, second, and other pre-commercial generations of fuel cell technologies. It is also critical that hydrogen infrastructure technologies be demonstrated in tandem with the FCV deployments. The Learning Demonstration project does both very well. The reviewers suggested that a plan be developed for continued operation and testing of the Generation II vehicles and refueling stations after completion of the present project.

The energy station project concept, which involves producing hydrogen as a co-product of power generation, is also considered very relevant to DOE objectives to improve the availability and reduce the cost of hydrogen for vehicle refueling.

Reviewers stated that the fuel cell bus evaluation project is well focused on the key technical targets. It plays an integral role in assimilating and analyzing data from various fuel cell bus projects across the country. Since the individual bus projects are essentially independent, this project is a necessary component of the overall effort to identify trends, improvements, problem areas, etc., in fuel cell bus developments and operation.

Technology Validation Funding by Technology:

The funding portfolio for the Technology Validation sub-program stresses the continuation of the Learning Demonstration project as it enters its final year. Second generation vehicles from two of the demonstration teams will continue to be operated, and data collection next year will provide information as to the state-of-the-art of these FCVs. Durability and range will continue to be the major data that will be collected and reported. A high-temperature fuel cell energy station will be operating, and data will be collected for at least a six month demonstration of the system. The FY 2011 funding profile is subject to Congressional appropriations.



Majority of Reviewer Comments and Recommendations:

The reviewer scores for the Technology Validation sub-program had a maximum of 3.7, a minimum of 2.2, and an average of 3.0. The major recommendations by reviewers are presented below.

Learning Demonstrations: A future technology validation program should not team a single automaker with an energy company, because this leads to insufficient use of private fueling infrastructure. A future program should instead adopt the German or California Fuel Cell Partnership model, of having every energy station decoupled from a specific automaker and having every party agree to provide hydrogen publicly to all consumers.

A final project report should be developed at the conclusion of the project. The plan should include a presentation of results at the International Partnership for Hydrogen and Fuel Cells in the Economy and the International Energy Agency venues.

Energy Stations: The energy station project concept, which involves co-production of hydrogen and electric power, is very relevant to DOE objectives to improve the availability and reduce the cost of hydrogen for vehicle refueling. The renewable hydrogen concept also aligns with DOE goals and objectives.

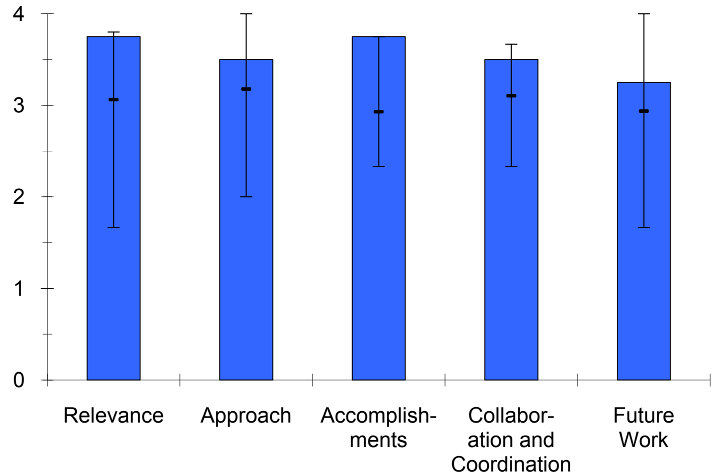
Analysis: Careful analysis of key aspects of vehicle operation (hydrogen cost, stack efficiency, durability, and driving range) demonstrates improvements in all aspects except hydrogen cost projections. The analysis of transient operation improvements demonstrates the technical depth of the program and helps feed information back to manufacturers and researchers.

Project # TV-01: Controlled Hydrogen Fleet and Infrastructure Analysis
Keith Wipke; National Renewable Energy Laboratory

Brief Summary of Project

The objectives of this project are to: 1) provide facility and staff for securing and analyzing industry sensitive data at the National Renewable Energy Laboratory's (NREL) Hydrogen Secure Data Center (HSDC); 2) perform analysis using detailed data in HSDC to: a) evaluate current status and progress toward targets, b) provide feedback on current technical challenges and opportunities into the Department of Energy (DOE) hydrogen research and development program, c) provide analytical results to originating companies on their own data (detailed data products), and d) collaborate with industry partners on new and more detailed analyses; and 3) publish/present progress of the project to the public and stakeholders (composite data products).

Overall Project Score: 3.6 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This project is very relevant to the DOE Hydrogen and Fuel Cell Technologies Program (FCT). The data collected and reported in this project is vital to measuring the progress of the programs and to glean information that is critically important to guiding future directions of the technical work.
- This project is well defined and relates directly to DOE technical targets.
- This is project performs analysis and reporting of crucial testing of vehicles and fueling.
- This work is very relevant to DOE fuel cell objectives of conducting independent assessment and dissemination of fuel cell vehicle (FCV) information and providing real-world feedback to researchers and partners to improve technology. The information gathered will help improve technology readiness for FCVs and lead to successful market introduction. This project validated DOE targets in real-world conditions.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- The approach focuses on the collection of actual, in-the-field operating and performance data in a self-reporting format. Then, the information is disseminated in a comprehensive compilation that allows proprietary information provided by suppliers and users to be protected. The approach is a solid and sound type of data collection and reporting, and its value has been validated by the large number of references to the project, coupled with acceptance by the reporting organizations.
- Although there are only two of the original four original equipment manufacturers (OEM) left to validate the last few objectives. That is a sufficient amount for this project.
- The detailed products that NREL has published have been helpful to the OEMs and all parties involved in the analysis of the results.
- The approach for this project was right from the beginning, and it continues to be right.
- The project is well-designed to address technical barriers set by industry and DOE. The scope of testing covers all of the bases.

- The approach is very reasonable for providing real-world analysis, data collection, and information dissemination efforts, while protecting sensitive information. Composite data products are a good way to provide summaries without revealing any company-sensitive information. (This has been an area of concern for partners). This approach was focused on addressing the key areas of interest to the DOE FCT Program.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.8** based on accomplishments.

- This project continues to make excellent progress. Reported progress includes more than 100,000 vehicles hours and 2,500,000 vehicles miles. The project has resulted in more than 80 public data publications with new results and updates published every six months, which is truly outstanding. The depth of technical analysis and interpretation of collected data is excellent.
- The vehicles tested in this project have been phenomenal. The project continues to relentlessly pursue the technical targets and to improve vehicle fuel cell performance and fueling infrastructure. This is an excellent use of public funds and the statistics speak for themselves (e.g., 2.5 million miles driven).
- The products developed by NREL in response to OEM data needs and public needs have been fantastic (e.g., 80 composite data products published, 20 of which are new this last year).
- There has been good progress in durability for the stack for Generation 2 over Generation 1.
- The project is meeting DOE goals.
- They greatly reduced transients, making durability goals easier to achieve.
- The refueling information is very important, especially in comparison to battery charging.
- Toyota's 431 mile driving range is extremely useful.
- There is a significant number of vehicles (almost 150 vehicles total, over 100,000 hours) and stations (23 total) in data collection effort. It is good to see vehicles with higher hours/miles included. The project continues to track key stations that serve as backbones for hydrogen efforts.
- There are a significant number of composite data products (20 new, 52 updated) in the last year. It is good to keep information up to date, and presented at key venues.
- Careful analysis of key aspects of vehicle operation (hydrogen cost, stack efficiency and durability, and driving range) demonstrates improvements in all aspects.(Hydrogen cost projections are not so optimistic.) The analysis of transient operation improvements of particular interest demonstrates the technical depth of the program and helps feed information back to manufacturers and researchers.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- The basic nature and approach of this project requires extensive collaborative interactions with suppliers and users. Numerous publications and presentations have been accomplished in this review period. There have been collaborative interactions with automotive and energy industry partners, FreedomCAR and Fuel Tech Team, industry and state government organizations, and federal government agencies including the Department of Defense (DoD).
- Everyone is engaged and loves these projects.
- They are collaborating with all the right partners – car companies, industry associations and state groups. They need better marketing and promotion of the program and results to the media, both public and government.
- They are collaborating with industry partners and major constituents of interest to discuss results and methodology. New collaboration with DoD Defense Logistics Agency should be beneficial to both parties.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- The future work plan includes continuing the work already underway to establish operational and performance trends and improvements. Added emphasis with OEMs and infrastructure developers on supporting early market introductions is vital to timely deployment of hydrogen and fuel cell technologies.

- This is an outstanding real-time, real-world learning project that should be fully supported by the DOE through the end of the budget and funding cycle.
- They are a very crucial partner to help facilitate the automaker goal of commercialization in the 2014-2015 timeframe. Data will help educate consumers and local officials where stations and vehicles will be deployed.
- The project is nearly complete, but closeout activities appear to be appropriate to finish up the work (i.e., final report and leveraging of past experience to assist future deployment and validation projects).

Strengths and weaknesses

Strengths
- They employed a proven and tested approach to data collection and information dissemination.
- They made extensive use of analytical multivariate techniques.
- The results are essential for the auto OEMs and the entire value chain, including hydrogen suppliers, refilling stations and carbon manufacturers.
- They have a very comprehensive data collection.
- They enacted a thorough and careful analysis of important aspects of vehicle and station operation.
- There was a wide variety of data products presented, which have been updated to reflect current information.
- There was good partnership with key constituents.

Weaknesses
- No weaknesses noted.
- None.
- They need to promote findings and activities more. If the public and/or government knew of the scope of this project and results and data found, it would greatly help industry fight critics and skeptics who write off FCVs. A website is not enough because most people would not know to look there.
- No significant weaknesses observed as the program is well defined and successful.

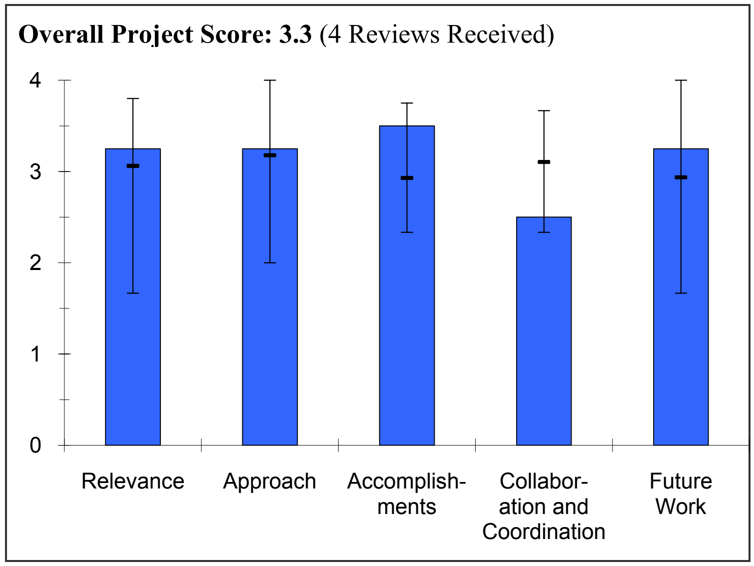
Specific recommendations and additions or deletions to the work scope

- Keep up the good work!
- A final project report dissemination plan should be developed. The plan should include a presentation at the International Partnership for the Hydrogen Economy (IPHE) and the International Energy Agency (IEA).
- The battery analysis could be improved. The life-cycle cost analysis should be looked at and analyzed.
- There should be an expanded presentation of results from primarily fuel cell events to broader auto events, government conferences, etc.
- No additions or deletions identified as the project is nearly complete.

Project # TV-04: Hydrogen to the Highways
Ron Grasman; Daimler

Brief Summary of Project

The objectives for this project are to: 1) record, collect, and report data from fuel cell vehicles and hydrogen fueling stations to validate DOE performance targets regarding fuel cell stack durability, vehicle range, and hydrogen cost at the station; 2) demonstrate the safe installation and operation of service facilities; 3) continuously update safety manuals and provide training; and 4) participate in various working groups to ensure continuous progress regarding hydrogen codes and standards.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This project develops two generations of FCVs and validates operation and performance in real-world use. The project to validate the practical operation of FCVs is very relevant to DOE FCT Program objectives. The program is ineffective if it does not have validation and introduction to potential consumers.
- The light-duty vehicle market is the largest potential market for proton exchange membrane (PEM) fuel cells. It is critical that the DOE work with the hydrogen fuel cell vehicle (HFCV) automakers to validate their first, second, and other pre-commercial generations of fuel cell technologies. It is also critical that hydrogen infrastructure technologies be demonstrated in tandem with the HFCV deployments. This project does both very well.
- The project addresses barriers in FCV performance and durability and development of a hydrogen refueling infrastructure, which are barriers A and C in the multi-year plan.
- The program addresses DOE goals for technology validation and aligns well with Daimler internal program goals. They are moving the technology toward greater commercial readiness. They are addressing not only vehicle technology goals, but also associated issues such as maintenance and codes and standards.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The approach involves the development and introduction of two generations of FCVs, along with the necessary refueling and support infrastructure required for testing and validation of vehicle operation and performance, in comparison to conventional consumer automobiles. The approach is consistent with the goals and objectives of the project and with the DOE FCT Program.
- Daimler appears very committed to rolling out HFCVs to the public in the 2015 timeframe. Their approach seems to address many of the needed technical barriers that must be overcome for a successful transition to commercialization. Two key components of overcoming these barriers are validating cold start and 2,000 hours of stack durability, and Daimler considers those to be principle objectives.
- The approach is to advance vehicle technologies from Generation I to Generation II and prepare for commercialization. The data acquisition system on all vehicles is important for supplying data to NREL. Both the east and west coast operations are useful for obtaining data from differing weather conditions. Most of the operations moved to the west coast in the past year. It is highly desirable that they maintain operation of refueling stations.

- The technical approach is reasonable, with two generations of technology being developed for demonstration in real-world applications. Activities in the maintenance and service and codes and standards provide a well-rounded approach to addressing major issues from vehicle and infrastructure standpoints. They have developed an associated hydrogen fueling network. Was there any overlap in hydrogen fueling with other TV projects?.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- During this period of the project, transition from Generation I to Generation II vehicles was completed. Testing has shown improved performance of the Generation II vehicles. Progress was also reported in the area of safety, codes and standards. The progress appears to be consistent with the project schedule. The project demonstrated successful operation of refueling activities.
- Daimler has shown outstanding progress over the course of this project. After their 30 Generation I vehicles were retired, with six years operational achieved versus the planned two years, they made tremendous improvements on their Generation II HFCVs. From Generation I to Generation II, size is down 40%, power consumption is down 30%, and range has increased 150% (although some of this is due to going from 350 bar to 700 bar compressed hydrogen). It should be very interesting to hear the results as they start Generation II HFCVs on the road.
- Quality vehicles were produced according to standard Mercedes-Benz processes. They commissioned and tested 70 MPa stations in Burbank, California, and maintained operation of a DTE Energy (DTE) station in Michigan, continued good involvement in the area of safety, codes and standards, and convinced station owners to eliminate the personal protective equipment requirement. Their participation in community outreach efforts is commendable. Progress in the development of the Generation II vehicles seems slow and no stack durability data was provided, though it was noted most failures were attributed to hoses, brake pads, etc.; It would have been good to have seen more data on stack durability (claim is for >2,000-hour durability). All testing to date is "internal." It is always questionable if they will be able to keep stations open until greater numbers of vehicles are on the road.
- There were significant accomplishments made on the vehicle side: Generation II vehicles show great improvement over Generation I vehicles in range, power, and fuel consumption. Generation II vehicles are being approached from a near-commercial viewpoint (versus one-off prototypes) with extensive testing and verification.
- Their participation in safety and codes and standards efforts is good and is very important from a safety and performance standpoint.
- Their participation in outreach efforts is also important. They must reach the public with the new advances in performance and technology through venues such as road shows and auto shows.
- They achieved durability and cold start targets, particularly on Generation II vehicles. First generation vehicles exceeded lifetime expectations and several are still in operation.
- They continue to work with DTE Energy on a hydrogen station in Michigan that is open to all users (not restricted to Daimler). They are improving technology of this station with new equipment.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- Interaction and collaboration with project partners was reported and documented; however, additional exposure of the FCVs to the general public for testing and evaluation in a less controlled environment could have been very beneficial, as well as a means of gaining additional input of real-world assessment of the viability of FCVs from a consumer perspective.
- DTE works in the ”green”, or renewable, hydrogen market space, and it is wise for Daimler to validate that type of hydrogen as policy makers will inevitably require hydrogen to be produced from renewable sources over time. NextEnergy is a not-for-profit organization in the Detroit, Michigan, area that is widely respected and works well with the auto companies. They are keeping a permitting database with easy and public access via their website. The DTE hydrogen station in Burbank had a good deal of outreach associated with it. They are

- also working with the International Organization for Standardization (ISO), National Highway Traffic Safety Administration (NHTSA), and Society of Automotive Engineers (SAE) on codes and standards.
- They have three (really two) other partners.
- The collaborations are good and the specific collaborations with NextEnergy on permitting database are particularly noteworthy. They are collaborating with NREL on larger data collection efforts.
- The presentation does talk about the need for OEM collaboration on fueling station deployment, but was this actually done?

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Plans of future work to fully integrate Generation II vehicles into the test fleet are consistent with project goals and objectives.
- The project has a timely and effective approach to bring the Generation II vehicles into the program and will enable this program to have bridged two full generations of automotive fuel cell technology. This approach will provide great data for a DOE Technology Validation program and will help inform the public of the major improvements anticipated.
- The project should prepare for Generation II vehicle demonstrations and continue providing data to NREL/DOE and participating in community outreach activities. There were no timelines provided for beginning customer operations of production-level Generation II vehicles.
- Future plans are reasonable given that the project is near to close-down. Daimler is looking toward production-level vehicles as well as national-level outreach and education.

Strengths and weaknesses

Strengths

- The project is led by a major automobile OEM with support from infrastructure developers.
- The project is providing operational and performance data to NREL for third party, independent evaluation and analysis.
- The project is validating light-duty fuel cell vehicles in multiple markets.
- The project is providing hydrogen via renewable sources.
- The company (Daimler) is clearly bought into the future of HFCVs and is making the most of this technology validation program to further the effort along.
- Real-world vehicle operation has been shown for Generation I vehicles but not much yet for Generation II. There is a good supply of data to NREL/DOE. They kept stations opened and moved most of the operations to west coast where fleets will largely be located. They were an active participant in codes and standards development.
- The codes and standards work is beneficial and important.
- The project has good progress on vehicle technology improvements to build toward pre-commercial markets.

Weaknesses

- There is limited public exposure to the project.
- None.
- There is slow movement to Generation II vehicles and commercialization and an uncertain commitment to keeping refueling stations open.
- Collaborations are somewhat limited to Daimler-internal organizations (only NextEnergy and DTE are listed external partners), though this does not appear to have affected success.

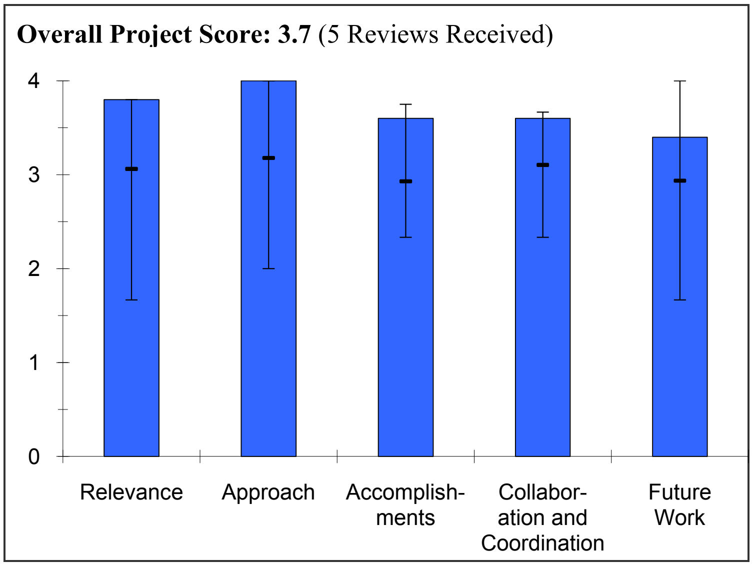
Specific recommendations and additions or deletions to the work scope

- A plan should be developed for continued operation and testing of the Generation II vehicles and refueling station after completion of the present project.
- They should explore methods to increase public awareness of the project and its results. Additional general public exposure will enhance the maximum benefit of the project.

- A future technology validation program should not team an automaker with an energy company, because this leads to too much private fueling infrastructure. A future program should instead adopt the German or California Fuel Cell Partnership (CaFCP) model, have every energy station decoupled from a specific automaker, and agree to provide hydrogen publicly to all comers.
- The project is ending in fiscal year 2010. It appears to have satisfied many, but not all, goals. The project should keep stations open with industry co-funding and get Generation II vehicles into the public sector.
- The project ends September 2010, and there are no recommendations for additions or deletions at this late juncture.

Project # TV-05: Hydrogen Vehicle and Infrastructure Demonstration and Validation
Gary Stottler; General Motors

Brief Summary of Project



The overall project objective is to deploy a system of hydrogen fuel cell electric vehicles integrated with a hydrogen fueling infrastructure to operate under real-world conditions to: 1) demonstrate progressive generations of fuel cell system technology; 2) demonstrate multiple approaches to hydrogen generation and delivery for vehicle fueling; and 3) collect and report operating data. The past year's objectives were to execute next generation of fuel cell technology by: 1) working with vehicle operators to obtain hours and data; 2) collecting, analyzing, and reporting data from program vehicles and fueling locations; 3) completing, operating and maintaining fueling stations, and providing data; and 4) producing and submitting an interim technical report. The current year's objectives are to: 1) complete technology insertion into vehicle fleet, and 2) collect, analyze, and report data from technology insertion and baseline vehicles.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This project involves real-life vehicle demonstration and performance validation making it quite relevant to the goals and objectives of the DOE FCT Program. In particular, Project Driveway, a meaningful test and evaluation by a sample representing consumers, is relevant in that self-serve refueling is also a part of the project.
- The project is relevant to the DOE technical targets. The relevance to General Motors (GM) is apparent as they want to develop HFCVs. Their cost share speaks for itself. All of the multi-year program targets are being addressed by this project.
- This type of project is extremely important to the future of cleaner transportation and a reduced dependence on petroleum. They introduced a transportation infrastructure that also significantly reduces GHG (greenhouse gas) emissions as well as other criteria pollutants. GM is a domestic automaker, and the ability to shift to the electrification of the vehicle is critical to its continued existence and the hundreds of thousands of domestic jobs associated with GM. It is also critically important to team with an energy company, in this case Shell. Low-cost, convenient hydrogen infrastructure is critical to the success of any GM rollout of HFCVs.
- This is definitely critical to DOE objectives and meeting and surpassing technical targets.

Question 2: Approach to performing the research and development

This project was rated **4.0** on its approach.

- The approach centering on real-life vehicle testing and refueling is very innovative. Testing under various climatic conditions, including cold weather driving and starting, is very important. It was beneficial to have an emphasis on gathering vehicle durability and refueling codes and standards, including local permitting officials.
- Again, the statistics provided since the onset of this project demonstrates GM's level of technical, financial, and company commitment to achieving results.

- Deployments in New York City, Washington D.C., Los Angeles, and Sacramento are all future early markets and serve to educate the public about the promise of HFCVs. The technical and human factors learning associated with this effort have been enormous according to GM. This technical approach is focused on demonstrating the HFCVs, and understanding both the customer experience and maintenance and servicing requirements. These are all important elements of a pre-commercial technology validation. The project had a broader validation effort across multiple regions and climates by partnering with Shell, and in some cases building their own, to have a total of eight hydrogen stations..
- They opened eight stations in key areas (Washington, D.C., New York City), which allows a real retail experience for users. It is also important to show potential customers that fueling with hydrogen is very similar to fueling with gasoline. There are more than 100 cars in the hands of the public, government representatives, and celebrities.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- Progress during the past period was good, especially the introduction of Generation II vehicles with notable technical improvements. The determination that hydrogen station performance was lagging vehicle performance is an important conclusion derived from this year's progress.
- They have excellent customer interface and feedback and five years of service station maintenance. They have introduced 100 phase-2 vehicles since 2007. It was a significant accomplishment to normalize customer refueling. They have good geographic variation, which helps to demonstrate the vehicles in various driving conditions. They have progressive engineering based on experience. There have been improvements to Generation II based on lessons learned from Generation I. They have demonstrated fuel cell stack durability and product functionality and have met customer expectations. They are using commercially available renewable hydrogen at various locations. Permitting has been a huge lesson learned and now the codes and standards are better defined as a result of these projects.
- The improvements made between Generation I, Generation II, and Generation II's tech insertion have been very promising. The NREL data will eventually reflect much of these improvements, but other areas can already be seen. Fuel cell durability has increased to the point that it can now meet customer expectations in terms of service life. The driving range has already increased, but it is expected to improve another 15%-20% in the Generation II tech insertion phase. Education and codes and standards can be helped by having the Environmental Protection Agency (EPA) use an HFCV GM Equinox to transport employees for two years.
- They have increased range and stack durability to help meet customers' expectations. They predict range improvements will progress even more in the future.
- The project exhibits good cold start capability.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- Collaborations, including those with federal, state, and local governments, were very good. The interactions with the general public were particularly important, and the project partners seem to work well together
- There was good collaboration with NREL on methodology.
- They are fully engaged at the state and local levels with the right players on codes and standards.
- Air Liquide has been very supportive on transportation and dispensing equipment.
- They are putting their vehicles into a wide variety of users' hands, which will gain a wide variety of feedback. GM is working with NextEnergy on codes and standards development. They are also helping inform the best hydrogen dispensing and distribution methodologies throughout these pilot efforts.
- They are partnering with key companies, namely Disney, and government agencies like the United States Postal Service (USPS), which could lead to potential purchases.
- It is key for all automakers to work together on codes and standards.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- Future plans to continue to operate and demonstrate Generation II vehicles while upgrading technical performance is very reasonable for the last year of this project. It is important to keep the emphasis on durability as well as performance improvement.
- Generation II durability on vehicles will be tracked and evaluated.
- GM appears committed to commercialization of HFCVs in the 2015 timeframe, clearly making the case that plug-in hybrid vehicles (PHEV) and battery electric vehicles have their limitation in terms of size and range. They plan to make as many as 1,000 HFCVs in their Generation III rollout, and a technology validation program built around this larger volume would help solidify the education and codes and standards. Additionally, hydrogen infrastructure needs should be addressed concurrently in a future phase. GM's initiative to improve the Generation II vehicles based on experience and feedback is welcomed.
- They have more vehicles with increased durability, while decreasing the size and weight of the fuel cell and its components. They are increasing range but need to get to 300-350 miles.
- GM has learned a great deal as the result of this program, in terms of both the vehicle-related elements and the fueling infrastructure elements. The degree of sophistication in the fueling has been evidenced by GM's ability to identify Hawaii as an important early market. Their ability to develop a strategy and identify a coalition of partners and stakeholders to develop this opportunity.

Strengths and weaknesses

Strengths

- This was a sound approach.
- There were good interactions among the project partners.
- There were extensive collaborations with many organizations including the general public.
- Real-world durability has increased. Many lessons have been learned on the fuel cell stacks and many improvements have been implemented. The ease of use and cost of infrastructure have been valuable to evaluate.
- Generation II crash tests were conducted on four vehicles without incident of hydrogen leaks.
- The project addresses the light-duty vehicle market, which has the potential for the largest environmental and petroleum reduction benefits. The project also addresses many locations that will be early markets for HFCVs and codes and standards through the work with NextEnergy. The project addresses hydrogen infrastructure with an energy company (Shell) that leans forward more than others in alternative energy pilots.
- The project exhibits real-world testing and key partnerships.
- The project exhibits tremendous progress of the fuel cell system in terms of performance, volume, weight, and cost.

Weaknesses

- There needs to be more on assessing and evaluating refueling operations in order to bring station development up to a par with vehicle performance.
- None.
- None.
- The range needs to increase, but it should occur in next stages of program
- They have not been able to identify a sellable approach to initial fueling infrastructure development.

Specific recommendations and additions or deletions to the work scope

- The project should place more emphasis on refueling station operations, including performance enhancements and cost reduction for hydrogen production and delivery. A prime objective in the final year should be a focus on evaluating and recommending which directions should be taken to reach the cost target for hydrogen of $3 per-gasoline-gallon equivalent.
- None.

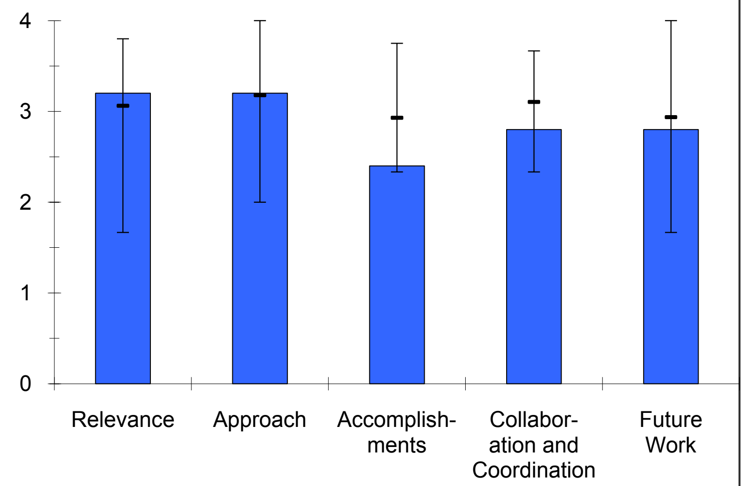
- Another technology validation phase is needed beyond September 2011. DOE should ask GM and others to propose a framework that would best meet their needs.
- The future work identified by GM is appropriate if it is possible to stay the course.

Project # TV-06: Validation of an Integrated Hydrogen Energy Station
Ed Heydorn; Air Products

Brief Summary of Project

The overall objective of this project is to determine the economic and technical viability of a hydrogen energy station designed to co-produce power and hydrogen. The project will utilize a technology development roadmap to provide deliverables and go/no-go decision points. The concept for this project was FuelCell Energy's (FCE) molten carbonate fuel cell (MCFC) plus Air Products' (AP) hydrogen purification system. Design, fabrication, and shop testing of the demonstration unit have been completed. The demonstration operation will be beginning in mid-2010 on renewable feedstock at the Orange County Sanitation District (OCSD). The project will validate process economics based on system performance.

Overall Project Score: 2.8 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project is relevant because it demonstrates both hydrogen production and electrical power generation. In addition, it is important that a high-temperature fuel cell (molten carbonate) is demonstrated (a fuel cell technology that is fairly unique to this DOE program).
- The decentralized hydrogen energy stations might be an interesting alternative to central production and should be evaluated.
- The decentralized co-production opportunity offers a chance at high overall efficiency.
- The negative consequences of stranded hydrogen infrastructure are that it is not economically viable, and it puts too much pressure on federal, state, and local governments to subsidize these efforts. The problem is eliminated wherever the infrastructure can have a dual use. Combined heat hydrogen and power (CHHP) through high-temperature, stationary combined heat and power (CHP) fuel cells provide a partial solution to this stranded hydrogen infrastructure problem and should be pursued with vigor.
- The project evaluates the economic and technical viability of a hydrogen energy station co-producing electricity and hydrogen fuel. This is Barrier I of the multi-year plan.
- The project concept is very relevant to DOE objectives to improve availability and cost of hydrogen for vehicle refueling as co-product of economic power generation. The renewable hydrogen concept also feeds DOE goals and objectives.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- This project has employed a sound approach structured to assess the economic and technical feasibility of a co-generation mode of operation. The project is about 89% complete. The approach addresses significant DOE goals and objectives with a unique methodology.
- There is practical field testing in the works. The real-world fueling station and energy supply were sound approaches.

- This project combines both CHHP projects with renewable feedstocks (partially) through landfill gas at the OCSD in Fountain Valley, California. The involvement of both federal (DOE) and state (California Air Resources Board, or CARB, and South Coast Air Quality Management District) funding is the model for future projects where multiple stakeholders can participate. FuelCell Energy's MCFC is an excellent choice for the stationary fuel cell. The location of the CHHP project is ideally suited for early market hydrogen infrastructure by being in Southern California.
- There is a logical progression of the project from feasibility evaluation, to system design, to construction and deployment and operation. It is a very slow process occurring over 10 years. Energy stations would facilitate the introduction of renewable hydrogen as a transportation fuel. There is good system design with over 80% potential efficiency, and it is a good site selected for a demonstration at a municipal wastewater treatment plant. The project will need a low-cost clean up system for biogas, which could impact economics, and the solution for this was not addressed in the presentation.
- A four-phase approach is careful and logical. The proof of concept for hydrogen energy stations (e.g. whether it will it work and whether the power cost be reasonable) makes sense. They are approaching the deployment phase in 2010.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **2.4** based on accomplishments.

- The project is in its fourth and final phase including operation, testing, and data collection. Validation testing has been completed, but it is unfortunate that on-site testing will be fairly short-term given that the project is scheduled to end in March 2011. The schedule should have been accelerated to allow more actual on-site operation and testing, including operation on actual digester gas.
- There has been evident progress on shop validation.
- While progress continues to be made, there have been significant delays. The original date for initial operating capability was November 2009. This slipped to May 2010 in time for the National Hydrogen Association conference, only to slip again to what is now July or August 2010. There appears to be solid reasons for this slippage, and AP and FCE want to make sure they have a good system-level product before operating it. However, a lot of confidence has been lost on this technology due to these delays.
- This has been a very long project! The most significant goals have not yet been accomplished, although an integrated hydrogen energy station is planned to open in California this summer. Ground has been broken for this station. The project will continue on with funding for the OCSD coming from CARB with project initiation in September 2011. So, it appears it may not be truly operational for some time yet. All testing up to this time has been done at FCE in Connecticut using an old molten carbonate fuel cell stack. Operation with simulated digester gas feed worked well. A new stack will be used in the OCSD demonstration. No economic analysis has been presented, although that was proposed to have been done by this point. DOE has been providing some assistance for the project. Good progress has been made in modifying the fuel cell to take out hydrogen from the anode gas and good purity data was presented.
- The system was successfully validated by shop testing, and safeguards against failure were verified. The project successfully produced a significant amount of hydrogen (and met design parameters for production and purity). The system was tested with simulated digester gas to verify operation.
- They have identified a location for an energy station near digesters to provide a renewable fuel source, but the location is not ideal. Will pipelines between the station and the production site be an issue?
- It appears that the schedule has slipped quite a bit. The 2009 presentation stated that operation would begin fiscal year 2009-2010 is now planned for deployment in summer 2010. There was no real discussion of the reasons behind schedule slip, nor was it really highlighted.

<u>Question 4: Technology transfer/collaborations with industry, universities and other laboratories</u>

This project was rated **2.8** for technology transfer and collaboration.

- There were good collaborations with industry, state, and local government agencies, plus a university, although the real collaboration begins with the on-site operation and testing.
- There was a low level of technical collaboration.

- Air Products has done a good job of keeping federal, state, and local authorities involved throughout this effort, mostly out of necessity as these are their funding sponsors and permitting authorities. But, they have collaborated to the general degree necessary. The only exception is the constantly slipping operational dates; this has hindered the ability of the various stakeholders to schedule an event to publicize the opening of this facility.
- The project had good industry, government, and university interactions and collaborations.
- There were significant and extensive collaborations with industry, government, and university, as well as good collaborations from the funding side.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The plans for completing the project seem reasonable given the current status of the project.
- They have a clear perspective on application in Orange County.
- Air Products would like to see continued DOE involvement beyond March 2011, when the funding runs out, but they were not clear as to what that might be. They will operate the station for three years under a California Air Resources Board/California Energy Commission contract, so perhaps DOE does not need a follow on project. By then, CHHP technology will likely have evolved to a degree that would favor a new CHHP project somewhere else, rather than continuing with this effort.
- The project ends March 2011 and the energy station will not open until Sept. 2011. Future funding will be provided by CARB. They plan to scale up first to a 400-500 kg/day DFC®1500 system, then to an 800-1000 kg/day DFC®3000 system, though it is unclear if this will happen in the CARB funding cycle (three yrs, $2.7 million).
- Future work appears reasonable as they are addressing economics and scalability in this proposed work. The project will need to stay close to the new schedule in order to be successful.

Strengths and weaknesses

Strengths
- Good approach involving demonstration of a number of important issues – high-temperature fuel cell, hydrogen-electricity co-production, hydrogen pipeline to fueling station, and operation on digester gas fuel.
- They have strong partners.
- The approach is to demonstrate decentralized energy supply with high efficiency as an alternative to central production.
- They demonstrate high-efficiency via CCHP (47% steam methane reformer, >80% CHP).
- The project demonstrates an extension to a renewable source.
- The project demonstrates renewable hydrogen production during off-peak electricity demand hours using CHHP technology. The hydrogen station is in close proximity to a future build out of HFCVs. There is a strong team with leaders in hydrogen (AP) and FCE for MCFC stationary fuel cells.
- The project exhibits a reliable fuel cell technology and made a good choice of a demonstration site at OCSD. The system design appears solid, although the biogas clean-up system, and its efficiency is not clear.
- They are addressing both vehicle and distributed energy concepts (both current issues).
- The project exhibited a very high efficiency of the entire system.
- These proved to be innovative technologies for the hydrogen production step.
- The renewable hydrogen aspect is valuable.
- The project had significant leveraging of multiple funding sources to complete the project.

Weaknesses
- There was only a very limited time for on-site full operation and concept demonstration. It is unfortunate that time for only limited on-site operation is possible given the significant investment of time, effort and funding expended on this project to date.
- The project demonstrated uncertain scalability.
- The economic aspects (cost and maintenance) of decentralized purification are still unclear.

- The project is running out early, prior to the actual demonstration starting September 2011. (CARB support is required in addition to DOE.)
- A realistic estimate on the hydrogen co-production price was not given due to dependence on upcoming analysis, economies of scale, and optimal size.
- There were too many delays in reaching operational capability.
- The project had very slow progress in meeting objectives. It is unclear what problems can be anticipated in the fuel cell scale up. No system economics analysis has been presented for the OCSD, Fountain Valley station.
- The project has not done much economic analysis yet (mostly proof-of-concept testing). This is not really a serious problem yet, but it must be addressed now that the concept has been proven.
- Scheduling delays have pushed the project timeline back.

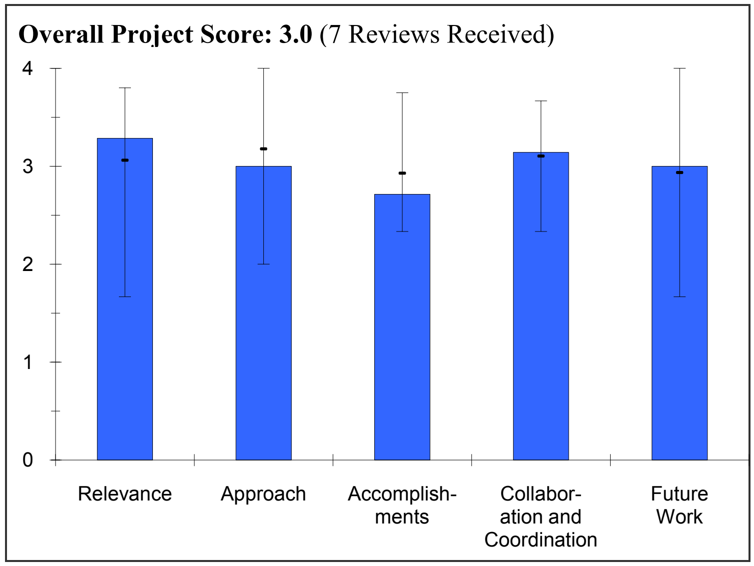
Specific recommendations and additions or deletions to the work scope

- It is strongly recommended that the DOE and California agencies take steps to ensure that this facility continues to demonstrate operation, including performance testing and data collection and analysis, and meaningful, realistic economic evaluation of operation in the co-production mode using digester gas fuel and producing hydrogen with sufficient purity for use in FCVs.
- They should concentrate on analysis of overall efficiencies, economics, and clear statements on cost per kilowatt hour and kilogram hydrogen.
- They should communicate technology and project barriers more clearly.
- They should communicate partner contributions more clearly.
- They should show progress from the past year in a clearer way.
- None.
- Encourage AP to speed up the OCSD demonstration. AP and the CARB can fund the effort going forward. Hopefully, data will be provided to the DOE.
- The project is near completion and it is recommended that they complete the future work as described. Economic analysis will be very important to build interest in the concept.

Project # TV-07: California Hydrogen Infrastructure Project
Ed Heydorn; Air Products.

Brief Summary of Project

The objectives of this project are to: 1) demonstrate a cost effective infrastructure model in California for possible nationwide implementation; 2) design, construct, and operate five hydrogen fueling stations; 3) collect and report infrastructure data; 4) document permitting requirements and experiences; 5) validate expected performance, cost, reliability, maintenance, and environmental impacts; and 6) implement a variety of new technologies with the objective of lowering the costs of delivered hydrogen.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This project is relevant in that it focuses on demonstration of hydrogen fuel station operation and addresses a number of DOE FCT Program goals and objectives.
- This was a congressionally directed project. Fortunately, AP has made it relevant to the DOE multi-year program plan.
- The continuous demonstration of different refueling pathways, mobile and stationary, extensive liquid hydrogen truck-in data, and the pipeline supply is still pending.
- There was no funding in 2009 and 2010. Did that reduce activities?
- It is critically important to investigate all types of hydrogen infrastructure, so that it can be economically scaled to the demand and associated requirements. This investigation will help inform infrastructure choices, as well as cost estimates during the pre-commercialization and early commercialization phases of HFCV rollout.
- This strongly supports DOE program. It is crucial to build and operate hydrogen stations around California, because that is the target location of automakers' vehicle rollouts.
- This project addresses the barrier of lack of hydrogen refueling infrastructure performance and availability data, and the cost of delivered hydrogen, which is barrier C of the multi-year plan. Five hydrogen refueling stations in California are to be operated to provide a model for nationwide implementation.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach involves locating hydrogen fueling stations in various locations and operating environments to demonstrate viability and assess economic feasibility. The project has a good concept of emphasizing the introduction of new technology and employing several hydrogen production and distribution approaches.
- This project has some similarities to other AP projects funded by the DOE.
- There is no detailed insight into the impacts of different delivery and dispensing methods. It would be especially interesting to do an economic comparison of these impacts.
- There are different stations with multiple sources. Do you already see advantages of one of the demonstrated hydrogen delivery and station storage technologies based on demand?
- AP looked at all different practical configurations in hydrogen infrastructure, including pipeline-fed hydrogen stations, tube trailer delivery to low-demand locations, attractive retail-like stations, and renewable hydrogen

stations. They accurately described the barriers they were attempting to overcome and progress made or not made against those barriers.
- There are some stations still being built.
- The project should work with OEMs to establish station requirements. They should also select suitable sites, complete required agreements and permitting, and collect and report on-line performance data to the DOE. There is value in operating several stations using different refueling mechanisms. Some stations in this project are questionable (e.g., only one vehicle in operation at Placerville station), and it is questionable whether this is a good use of limited resources. Although we do get some cold weather, higher altitude data show that we can get hydrogen to outlining locations, but not at low cost. Most projects are co-funded by industry (Nissan and Shell) and California government (CARB and SCAQMD). The plan for the Orange County, Fountain Valley demonstration of energy station is useful for addressing renewable hydrogen and co-production barriers. The Torrance pipeline station demonstrates that we can get low-cost hydrogen to the user. The University of California, Irvine (UCI), evaluation of air quality impact is useful.
- They fall short in being able to meet the objective of documenting a cost-effective infrastructure model that could become a national model.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The progress seems rather slow. This project has been underway for over four years, and only two stations are operating with only six months left in the project.
- The system has demonstrated 5-10 pounds per hour, or 100 kilograms per day and greater than 200 kilowatts of electricity.
- Fuel Cell Energy has determined how to get the main stream slip out of the anode.
- They decided to use waste water from OCSD in Fountain Valley.
- There was no evident progress in 2009, and there is no commissioning of a new station.
- AP generally accomplished what they set out to do. The pipeline station is important in evaluating the most economical delivery modes. The Long Beach tube trailer station was probably the least effective as it received very little demand considering the general location it was in. The station near Lake Tahoe was appropriate and useful for the small demand it was supporting. It was a smart approach to choose California for all of their station demonstrations as this state's zero-emission vehicle mandate and low-hydrogen costs in the Los Angeles basin will likely be the first site for HFCV commercialization.
- The demand for some stations' hydrogen has been increasing, and progress is being made on the construction of the Torrance station.
- It is difficult to completely separate out accomplishments for this project from the other AP project, TV-06. There has not been remarkable progress since the fiscal year 2009 review. The project is very behind schedule on the Torrance and Orange County Sanitation District/Fountain Valley demonstrations. These demonstrations are still not online, and it will be some time before they are, especially the OCSD demonstration. UCI's successful 350/700 bar station is exceeding expectations for use with plans for expansion. There have been no boil-off losses since demand is high enough. It was good to site this in Southern California. The new delivery concept trailer that deployed can dispense hydrogen at 10,000 psi.
- AP had good results for the Fountain Valley project, and it has potential to be a difference maker in terms of identifying a cost-effective model for delivering renewable hydrogen. The new delivery concept could also help boost sales of the smaller stationary fuel cell products.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- There were good collaborations among project partners.
- This project has good collaboration.
- Why are they not integrating and doing an independent analysis for cost evaluation of different station and delivery concepts?

- AP held a number of events relative to their various station openings. They also did a good job with the California regional authorities (e.g, CARB and AQMD). Where they fell short is in their reluctance to share information within the industry as a whole. This reluctance is partially understandable from the standpoint of protecting their intellectual property. They could, however, be a bit more forthcoming to the public at large as to what impact these stations would have to the ultimate cost of delivered hydrogen. This in turn would help the DOE and other government entities realistically evaluate and project hydrogen costs in the future.
- There were good collaborations and partners with OEMs and energy companies.
- There was a lot of collaboration with UCI.
- There was a good mix of industry, government, and academic partners.
- AP has developed a strong team of collaborators.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- With only a few months left in the project, the plans for future work are logical and reasonable. It is essential that all stations begin operation and collect operational data prior to completion of the project.
- They have an aggressive timetable to finish this project. They will be installing the pipeline, fueling station, and energy station.
- Commissioning of the Torrance pipeline fueling station is now ahead. The new progressive pipeline fueling is of high interest because the liquid-based stations are state of the art.
- AP provided lessons learned and suggestions on what infrastructure would work better in specific situations. They had sound ideas for furthering this effort in a way that would demonstrate lower cost and more readily available hydrogen in future phases.
- They have continued construction and operation of five stations.
- The project ends in December 2010. Future work includes continuing operation and commissioning the Torrance and Fountain Valley/OCSD stations capable of dispensing hydrogen at 350 and 700 bar. They continue to report operational data to the DOE.
- The future actions are appropriate. However, they need to be more aggressive in the delivery and in the public outreach and education aspects.

Strengths and weaknesses

Strengths
- There is strong support from the project partners, i.e. state and federal agencies.
- AP has the ability to leverage all the various collaborators to negotiate a successful project.
- AP did well in the practical test of setup and operation of different delivery methods and station concepts.
- AP demonstrated a variety of hydrogen storage and delivery technologies at a wide range of locations across California.
- AP had varied hydrogen production and delivery at each station.
- There is a good mix of industry, government, and academic partners. There are a variety of refueling options being explored with ranging cost to the user.
- The scope of the work is appropriate.

Weaknesses
- The project appears to be running out of time with major objectives left to be completed.
- An economic analysis should be part of this project at each stage. Economics should be expressed in terms of cost per kilogram for hydrogen delivered.
- There were few activities in 2009 and 2010 with little new recent findings.
- There is no comparison of different pathways and station concepts in terms of economics.
- The funding and the project might end before all stations are completed.
- Progress has been slow. It is also questionable whether they can keep all stations open.
- The delays in completing important projects, such as the Fountain Valley project, are a concern.

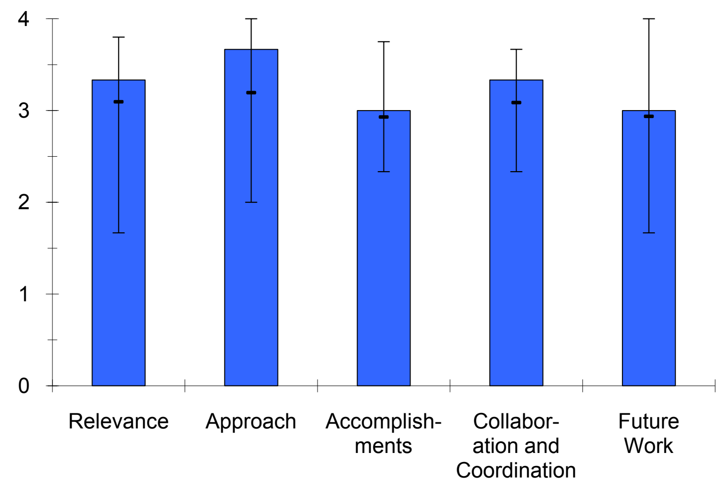
Specific recommendations and additions or deletions to the work scope

- Plans should be made to continue operation, data collection, and analysis (including economic evaluation) following completion on the project. Station operators should be identified, and a smooth transition to these new operators should be initiated as soon as possible.
- A safety plan should be developed before the project is finished. In the event this project is successful, it could be the blueprint for other similar projects. An analysis of the economics of the system needs to be conducted.
- AP should investigate and compare the economics of different station concepts (liquid hydrogen station versus pipeline station), evaluate challenges of different technologies, and give cost breakeven points dependent on demand.
- The project started in fiscal year 2005 but did not receive DOE funding in either 2009 or 2010. If necessary (it may not be), they could consider funding at low levels to maintain data collection efforts. The industry and California government should shoulder most of the costs going forward. The project has been behind and reached logical phase out.
- The scope is appropriate. The timeliness of the execution needs to improve.

Project # TV-08: Technology Validation: Fuel Cell Bus Evaluations
Leslie Eudy; National Renewable Energy Laboratory

Brief Summary of Project

Overall Project Score: 3.2 (3 Reviews Received)



The overall objective of this project is to validate fuel cell technologies in transit applications. The objectives are to: 1) analyze fuel cell (FC) bus performance and cost compared to conventional technologies to measure progress toward commercialization; 2) provide lessons learned on implementing fuel cell systems in transit operations to address barriers to market acceptance; and 3) harmonize data collection efforts with other fuel cell bus demonstrations worldwide in coordination with the Federal Transit Administration (FTA) and other U.S. and international partners. The objectives for 2010 are to: 1) complete analysis and report results on first generation FC buses; 2) begin data collection and analysis for next-generation fuel cell buses at Burbank, SunLine, and Public Transportation Authority for western Alameda and Contra Costa Counties (AC Transit); and 3) conduct crosscutting analysis of FC bus status at all sites.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The project is relevant to the data collection objectives of the technology validation program. It looks at a significant early fuel cell market and gathers important operational data.
- It's unsure that buses are a great fit for fuel cells relative to other advanced technologies. The program has so far been able to provide a lot of data that validates this. While the program has been valuable in terms of providing data to the bus OEMs to enhance and expedite their development efforts, it is missing targets for cost and performance that will support a product that is competitive with all technologies.
- The project is well focused on the key technical targets. It plays a key role in assimilating and analyzing data from various fuel cell bus projects across the country. Since the individual bus projects are essentially independent, this project is a necessary component of the overall program to identify trends, improvements, problem areas, etc. in fuel cell bus developments and operation.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The data collection approach is sound and follows standard procedures for such collection in a cost-effective manner (i.e. analysis of largely existing data).
- The annual status report is a good summarization of results and progress.
- Collecting data on both first-generation and second-generation fuel cell buses marks a good opportunity for comparisons.
- The sharp focus on assimilating and analyzing data from the various fuel cell bus projects should yield valuable information on public transit applications of fuel cell busses. The approach is consistent with the stated objectives of the program and with the Technology Validation program. Good approach to compare fuel cell bus performance parameters with conventional technology and also to include some refueling data.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- They have completed analysis of four separate first-generation fuel cell bus fleets. Fleets were California-centric, but this would be expected).
- The project demonstrated a good summary of key information (miles traveled, fuel used, fuel economy, miles between repair calls). The cost analysis is also very helpful to show general trends in fuel, operational, and capital costs.
- Collecting and analyzing information on fueling stations illustrates very different fueling needs for buses versus light-duty vehicles.
- A good amount of data is shown for gen 1 busses. It is too early in timeline to show gen 2 data, but it appears that these evaluations have been started. Comparison of results with conventional CNG and diesel busses highlights both improvements (e.g., better fuel economy) and problem areas (e.g., reliability). Refueling data is also important, particularly refueling time which impacts transit system operations.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Collaborations with transit agencies encompass a great many of the properties operating fuel cell buses. Also the project did well in working with major manufacturers of buses and some additional key organizations (CARB and CALSTART) both here and internationally.
- Good collaboration with the fuel cell bus projects across the country.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The future work plans continue on a successful path.
- The future activities follow logically from the program's objectives; that is, analyze the latest data coming from the current and future fuel cell bus projects.

Strengths and weaknesses

Strengths
- Collaborations with many transit agencies, FTA, and others to collect and analyze information from most FC bus projects in U.S. is a strength.
- Their data analysis, publications, and presentations are strong.
- The project has a good ability to provide key performance data to OEMs.

Weaknesses
- None identified.
- The targets for cost and performance, which will result in fuel cell buses that are competitive with other technologies, were not presented.

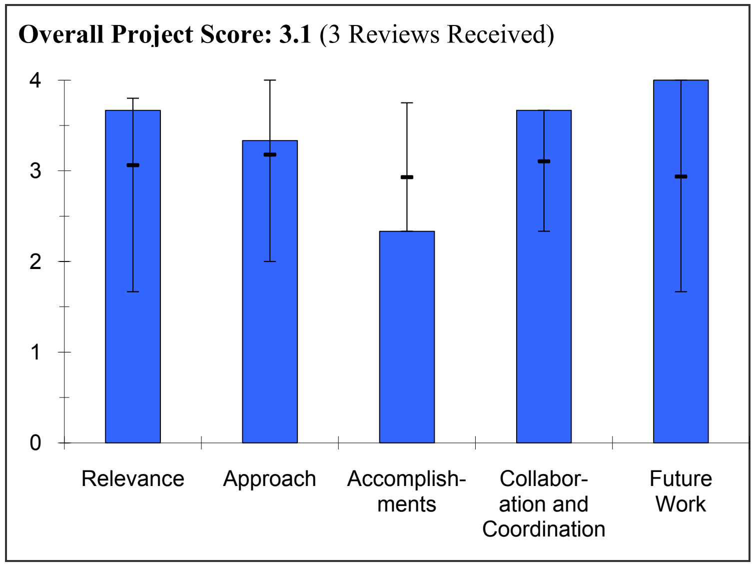
Specific recommendations and additions or deletions to the work scope

- None at this time.
- The project should include targets for cost and performance, as well as the development of road maps to achieve these targets.
- This project should continue for as long as the bus demonstration projects are providing meaningful data to be collected and analyzed. By assimilating results across a range of environmental conditions and from different manufacturers and operators, a much clearer picture of the status of fuel cell bus technology should emerge.

Project # TV-09: Hawaii Hydrogen Power Park

Richard Rocheleau; Hawaii Natural Energy Inst.

Brief Summary of Project



The objectives of this project are to: 1) install hydrogen fueling station infrastructure at the Hawaii Volcanoes National Park (HAVO) by August 2010; 2) support the operations of the National Park Service (NPS) hydrogen plug-in hybrid electric shuttle buses for 24 months through January 2013; 3) conduct engineering and economic analysis of the HAVO fueling station and bus operations on different routes, grades, elevations, and climatic conditions; 4) validate fuel cell system performance in harsh environments including high sulfur dioxide levels in the atmosphere; 5) position HAVO as an alternative-fueled vehicle test bed for the NPS; 6) provide a high-level of public outreach with hydrogen technologies; and 7) attract new partners and applications for the Big Island to support the development of hydrogen transportation infrastructure.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Hawaii is an excellent location to demonstrate renewable hydrogen technologies. They have the highest electricity and fuel rates and require fossil fuels to be delivered from afar. The multi-faceted approach chosen in this effort is also of critical value to the DOE FCT Program. A system-wide approach using multiple, available applications in a site with two million visitors will be very informative across a variety of areas.
- This is a truly pioneering project in terms of type of vehicle, renewable fueling infrastructure, and collaborators.
- The project addresses a number of different aspects of hydrogen infrastructure and hydrogen production from renewables, and supports FCV testing under a variety of conditions.
- This is a good, broad project that should generate useful, real-world information on systems performance versus key technical targets.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The multi-agency approach was very good from the standpoint of raising the necessary funds for a large demonstration and getting buy-in from a variety of stakeholders. It did, however, introduce more complexities and led to significant delays to this project. Working with HAVO will allow maximum visibility for this project. Teaming with the Hawaii Electric Light Company (HELCO) for renewable electricity for the hydrogen production is also a sound approach. Using the plug-in hybrid electric and hydrogen hybrid vehicles is a novel approach. The poor fit for a fuel cell bus at HAVO should have been anticipated earlier by doing accelerated market research.
- Progress has been hindered by delays in processing the necessary approvals for various elements of the program.

  The project has a broad approach focused on gaining information for overcoming key technical barriers, including hydrogen production from renewables cost and durability, hydrogen refueling infrastructure

performance information, and FCV performance and durability including the validation of fuel cell system performance in harsh environments.
- There is a strong public outreach component to this project.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- There have been lengthy delays in this effort, tying up significant DOE research and development funds for a long time. The acoustic information, acoustic data analysis, NEPA study, and the multi-agency agreements have all contributed to these delays. While some of these delays are to be expected in a project as technically aggressive as this, others appear to be caused by the lack of sufficient pre-planning and/or earlier resourcing of this effort.
- Progress has been hindered by delays in processing the necessary approvals for various elements of the program.
- The project demonstrated good progress but has experienced some slow down due to external constraints and lack of supplier information or data.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- Hawaii Natural Energy Institute (HNEI) has done a great job in bringing a number of different entities into this project. The NPS Climate Friendly Parks initiative is a good fit for what is being demonstrated. The state of Hawaii, Hawaii Electric Light Company, Office of Naval Research (ONR), U.S. Army, and DOE teaming will lead to a wide dissemination of results. Delays in this project may have been limited through detailed project and scope collaboration with many of these same entities early on. The acoustic issue, in particular, may have been remedied somehow, perhaps by locating the station further away from residences (if possible).
- This project has helped establish good relationships between the DOE and other federal agencies.
- There is an excellent diversity of relevant and significant partners embedded into the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **4.0** for proposed future work.

- HNEI is constantly exploring ways to have these efforts provide an even bigger impact to its stakeholders. The latest is with respect to their discussions with Puna Geothermal Ventures about their geothermal power. This dialogue could result in affordable hydrogen over a wide ranging area. They also continue to facilitate productive discussions with DOE and ONR on technical projects of mutual interest.
- The project has identified opportunities for renewable energy storage of potentially great value to the program.
- Future activities are in line with the project's timeline and milestones.

Strengths and weaknesses

Strengths
- The project did well in renewable hydrogen production. The NPS and HAVO involvement helped maximize the education component. The multi-agency involvement across a wide variety of stakeholders was beneficial for this program.
- The project had a good scope and coalition of state and federal partners.
- This is a good, strong project that balances a broad approach with sufficient detail to gain valuable data toward technical targets and barriers.

Weaknesses
- The project encountered significant delays to its schedule.
- There were delays in processing agency approvals.

Specific recommendations and additions or deletions to the work scope

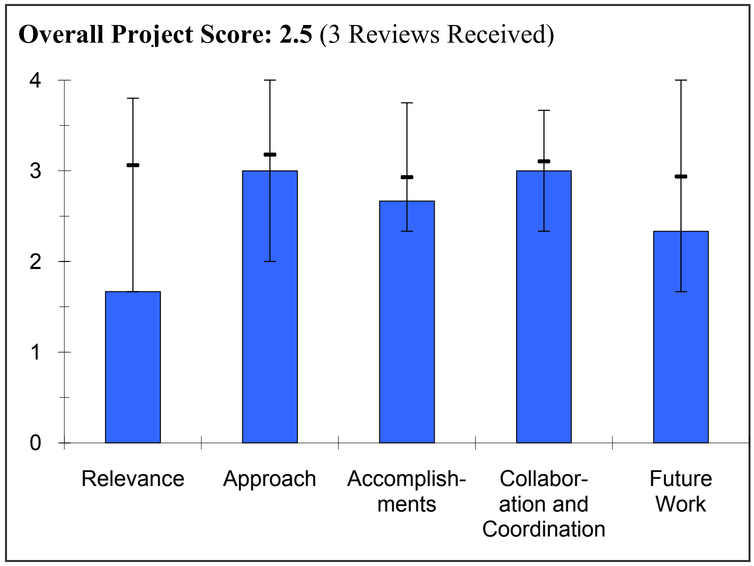
- The project should accelerate its activities.
- The project should integrate with renewable energy storage efforts on the Big Island.

Project # TV-10: Tanadgusix (TDX) Foundation Hydrogen Project/PEV Project

Connie Fredenberg; Tanadgusix Foundation

Brief Summary of Project

The main focus of this validation and demonstration project is to procure and evaluate the performance of commercially available and/or custom-made, alternative-fueled vehicle(s) on St. Paul Island, Alaska. Initially, the vehicles will be used for the eco-tourism operation owned by TDX Corporation, but the goal is to have village-wide use. In the winter of 2008, when fuel could not be delivered to the island due to sea ice, fuel was rationed to five gallons per vehicle each week at a cost of $12 per gallon. For this project, excess electricity from the existing high penetration wind-diesel system on St. Paul Island will produce the transportation fuel.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.7** for its relevance to DOE objectives.

- This project should be moved to the Plug-in Hybrid Electric Vehicle (PHEV) Vehicle Technologies Program as it is no longer considering the use of fuel cells. It is still relevant to DOE objectives, but not the DOE FCT Program objectives.
- This project has changed focus from hydrogen-powered vehicles to PHEVs.
- It appears from the presentation that this is a wind power and plug-in electric vehicle (PEV) project and not a hydrogen project. The only mention of hydrogen was the decision not to include an electrolyzer because of costs, storage, etc. Furthermore, the project is geared very specifically to the conditions present at the location of the study, and there is little relevance to other areas or applications.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- This approach has been changed to a PHEV project. The project did the right thing in making this change, as sticking with an HFCV (hydrogen fuel cell vehicle) project posed problems in terms of hydrogen infrastructure. Additionally, this island has some of the best wind you will ever see. An island in the Aleutians with this much renewable energy and so much trouble to import petroleum is a perfect site for leveraging the renewable energy into a transportation fuel (electricity).
- This small Aleutian island has a source of renewable wind energy, which can be employed for PHEVs. This is a critical need since vehicular fuel is very expensive and difficult to supply in the winter months. The application is perfect and the need is great.
- The approach is acceptable regarding the wind power, but the development of PEV ATVs has very limited application outside of this project.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The principal investigator (PI) had to make a decision as hydrogen refueling of a HFCV was not practical. It makes a lot more sense to move to a PHEV Van ($90,000). Apparently, it has been so widely accepted that plans are in place to purchase another custom van for this terrain.
- One PEV has been placed in operation on the island.
- Other than completing some limited studies, the only experimental results were obtained on a single vehicle, and these results indicate some severe problems associated with the use of an electric drive vehicle under the harsh conditions encountered in the area. These problems include motor overheating and stoppage, difficulties with sealing the battery compartment, steering rod issues, potential corrosion, and a driving range significantly less (about 50%) than expected. The results would suggest not continuing with more vehicles until such problems are resolved.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Collaborating with the Glenn Research Center is a positive. Also, other remote tribal villages in Alaska will learn about powering vehicles with excess renewable electricity and will likely follow suit. The vehicle maker is in Oregon, so this also helps the domestic manufacturing base.
- The necessary collaborations are in place.
- The project includes good collaborators.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- This will be moved to the Vehicle Technologies Program, and future activities will be explored with that management team.
- The future work is reasonable and focuses on additional vehicles and charging stations.
- The future plans include procuring a second PEV ATV and possibly a medium-sized pick-up truck. However, as stated above, major problems emerged in the first vehicle's operation, and no plans were presented to address these.
- There were no plans presented for incorporating a hydrogen element to the project.

Strengths and weaknesses

Strengths
- The project did well at exhibiting renewable transportation with excess wind-generated electricity.
  The project demonstrates to remote customers in our nation that they can reduce petroleum dependency and associated greenhouse gas emissions by switching to electrification of their vehicles with renewable power.
- This project showed an excellent renewable energy application driven by a major need.

Weaknesses
- Resources devoted to fuel cell technologies are (out of necessity) being diverted to PHEV demonstrations.
- None.

Specific recommendations and additions or deletions to the work scope

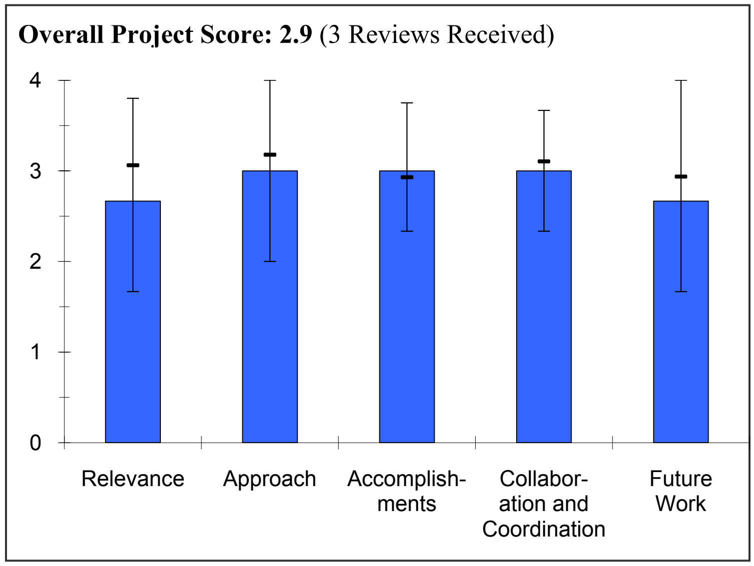
- None.
- Since the wind turbine produces the least amount of energy in the summer months, which is when the vehicle use is at a maximum, it may be beneficial for a small hydrogen electrolyzer or fuel cell system to be incorporated to store winter wind energy and then provide additional electricity in the summer.
- The project should be funded under a different DOE office or funding agency.

Project # TV-11: Texas Hydrogen Highway - Fuel Cell Hybrid Bus and Fueling Infrastructure Technology Showcase

David Hitchcock; Texas Hydrogen Highway

Brief Summary of Project

The objectives of this project are to: 1) provide public outreach and education by showcasing the operation of a 22-foot fuel cell hybrid shuttle bus and hydrogen fueling infrastructure; 2) showcase the operation of a zero-emissions vehicle for potential transit applications; and 3) to advance commercialization of hydrogen-powered transit buses and supporting infrastructure.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- This project demonstrates fuel cell operation in conjunction with a fueling station. The project, while relevant to the goals and objectives of the DOE program, certainly is not unique since there are many fuel cell buses that have been operating for years. The value of this project appears to be increasing public awareness in a new area (Texas).
- Public showcasing of a hydrogen-fueled transit bus is relevant to the public knowledge and acceptance of hydrogen vehicles.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The project has a reasonable approach based on providing public and government educational opportunities in an effort to promote understanding and acceptance of clean technologies. However, there appears to be few unique or innovative features of the project. There is very limited information given regarding the fueling station or refueling operations, and no mention was made of providing data and operational information to NREL (National Renewable Energy Laboratory) for the National Fuel Cell Database.
- The fuel cell bus and hydrogen refueling station were funded by non-DOE sources.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The bus has been delivered and showcased in several locations within the region (Texas). The project appears to be on schedule.
- The public was given significant hands-on exposure to the hydrogen-fueled bus and fueling station.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Collaborations are primarily with project partners (subcontractors). The project has significant interactions with sponsors where the bus has been showcased within the region, which is consistent with the project's goals and objectives.

- The project has significant interactions with other Texas institutions.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Future plans include a final event with state officials before the project ends in September 2010 and appears to be adequate.
- This project ends in September 2010, and one final outreach event is planned prior to that date.

Strengths and weaknesses

Strengths
- The team was able to perform the key duties necessary to procure and operate a fuel cell bus and fueling infrastructure in a large state where fuel cells and hydrogen have little presence.
- The project provided public exposure to an operational hydrogen-powered bus and fueling station.
- The bus and fueling station were provided by non-DOE funding sources.

Weaknesses
- None.

Specific recommendations and additions or deletions to the work scope

- The project should ensure that operational data is shared with NREL.
- The project should engage the partners in a coordinated public outreach that includes the American Recovery and Reinvestment Act programs being implemented at several Texas locations.
- None.

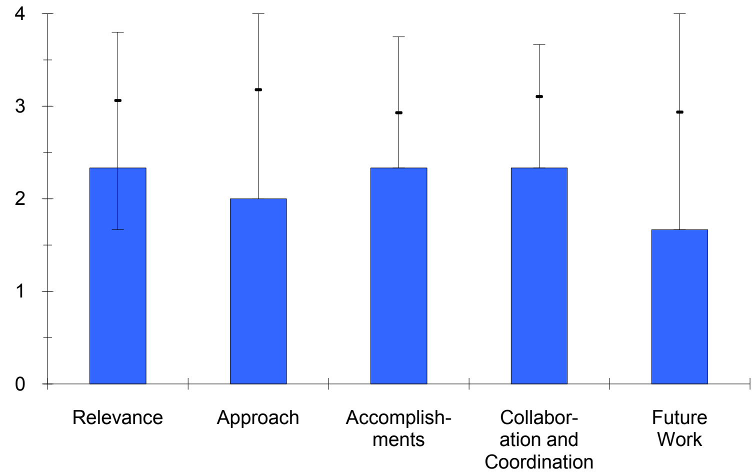
Project # TV-12: Florida Hydrogen Initiative

David Block; University of Central Florida

Brief Summary of Project

This project seeks to develop DOE and Florida's hydrogen and fuel cell infrastructure by: 1) creating partnerships for applied demonstration projects; 2) sponsoring research, development, and demonstrations in hydrogen and fuel cells technology; 3) facilitating technology transfers to create, build, and strengthen high-growth, high-technology companies; 4) developing industry support for applications; and 5) developing unique university-level education programs.

Overall Project Score: 2.2 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.3** for its relevance to DOE objectives.

- This project appears to act as a clearinghouse for fuel cell projects by soliciting and funding hydrogen and fuel cell projects. It is not clear why the selected projects could not be funded directly by the DOE through a competitive process.
- This proposal, as initially conceived, had some degree of relevance to DOE research, development, and demonstration objectives. The significant changes over time, however, make it less relevant. The fact that this was a Congressional addition, with limited influence by the DOE over its scope, did not help.
- This project appears to take a broadly based approach that includes a number of stand-alone projects.
- The individual projects cover a wide variety of hydrogen-related topics, with the only continuity showing is that all of the projects take place in Florida. That said, all of the projects are focused on hydrogen technologies and, in that sense, are relevant to the overall objectives of the program.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- The approach appears to be consistent with the goals and objectives of the project.
- The Florida Turnpike project never materialized due to the lack of orange waste (40% planned - 3% actual) that was originally envisioned. The next idea of a renewable fuel cell exhibit that would be rotated among the Florida museums also never materialized. Instead, they only put the exhibit in an Orlando museum. In summary, the technical approach failed that commitment test as key players dropped out across the board.
- As stated earlier, this work consists of a number of unrelated projects ranging from engineering demos and studies to fairly specific materials studies related to fuel cell membrane electrode assemblies (MEA) and electrocatalysts. Some of the projects would appear to be better suited for support under different parts of the overall hydrogen program so that their impact could be better integrated with other ongoing work in their respective topics.
- The present approach appears too broad and lacks sufficient focus in the individual topic areas to result in significant progress toward overcoming the technical barriers in the timeframe of the project.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- Given the goals and objectives of the project, progress appears to be reasonable. Progress may have been hampered by a change in leadership of the project; however, new projects have been solicited and selected and are in the process of being initiated.
- This project was earmarked by Congressman Weldon, who has since been voted out of office, and it has had a number of setbacks. The original university is out of the picture and was replaced with Florida Atlantic University. The Florida Turnpike project never went through due to a lack of orange waste product from which to produce renewable power. The quote from the Netherlands for renewable methanol was too expensive to follow through on as a substitute. The prime contractor changed during this effort. To their credit, three project tasks are being envisioned to replace those cancelled, which include two-year efforts on leak detection, low-cost catalysts, and MEA durability. Nineteen proposals were received and five were selected.
- There appeared to be major differences in progress among the individual projects. Some of the engineering studies could be rated at 3 (good), while other projects showed progress at the 2 (fair) level. Overall, a project rating of 2 seems appropriate within the grading system of these forms, but an overall ranking of 2.5 would be preferred.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- Collaborations appear to be primarily with the organizations involved in the project's selection process.
- A lot of collaboration was done, but few lasting commitments were made with the various project stakeholders, which in turn resulted in an overall effort that has become somewhat piecemeal as opposed to strategic in nature.
- Collaboration among institutions is good, particularly with the engineering projects.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.7** for proposed future work.

- Information on future activities was not presented.
- To their credit, they are working with Enerfuel and others in the new tasks to see it through to at least a conclusion, if not a successful one.
- Again, it is difficult to assess an overall ranking of future activities since the individual projects varied greatly in their approach. The overall ranking was based on the observation that only one of the projects specifically mentioned future work.

Strengths and weaknesses

Strengths
- The specific projects selected and funding appear to be relevant and should contribute to the hydrogen and fuel cell state-of-the-art technology.
- None.

Weaknesses
- This program had too many cancelled and redirected projects, which became less relevant as they were re-scoped. There was also a lack of commitment from a variety of stakeholders.

Specific recommendations and additions or deletions to the work scope

- The DOE should refund back to Congress any future Congressional aid of this nature.
- They should keep the engineering projects within the current scope and shift the materials studies to another sub-program within the hydrogen office.

2010
Safety, Codes and Standards
Summary of Annual Merit Review of the Safety, Codes and Standards Sub-program

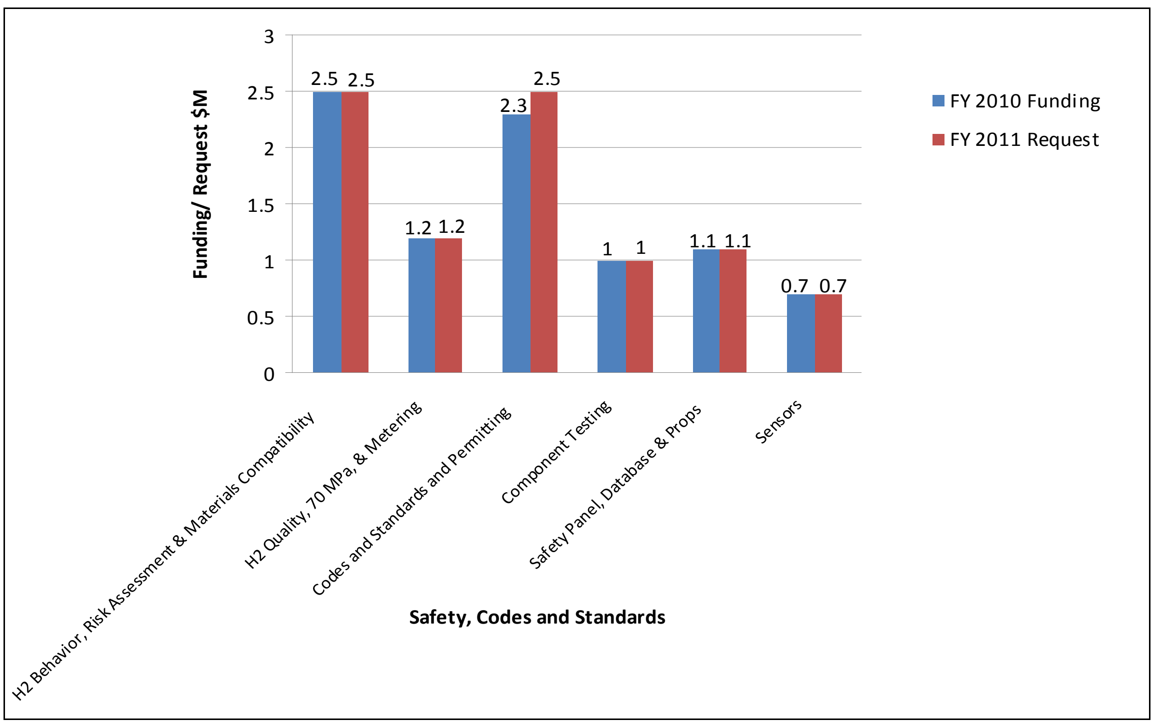
Summary of Reviewer Comments on the Safety, Codes and Standards Sub-program:

The Safety, Codes and Standards (SCS) sub-program supports R&D that provides the critical data and information needed to define requirements and close gaps in safety, codes and standards to enable the safe use and handling of hydrogen and fuel cell technologies. In FY 2010, reviewers recognized the progress, coordination, and organization of each project towards the overall goal of the safe deployment of hydrogen and fuel cell technologies. Reviewers were impressed by each project's breadth and commitment to information sharing and R&D collaboration. Reviewers also recognized each project's success in connecting relevant stakeholders.

Reviewers identified strong sub-program support in the following areas: hydrogen and fuel cell codes and standards permitting and education, hydrogen sensor technology, hydrogen components and material compatibility work, safety training for first responders and researchers, and development of a hydrogen fuel specification. Reviewers also appreciated each project's efforts at leveraging the resources and intellectual capital of academic institutions, standards development organizations (SDOs), national laboratories, government agencies, and industry, as well as other offices in DOE.

Summary of Safety, Codes and Standards Funding:

The sub-program funding for FY 2010 allowed for the continued strong support of the necessary R&D and domestic and international collaboration and harmonization to support hydrogen and fuel cell early market commercialization. The following chart indicates FY 2010 appropriations and the FY 2011 budget request.



Majority of Reviewer Comments and Recommendations:

In FY 2010, 14 SCS projects were reviewed, with a majority of projects receiving positive feedback and strong scores. Reviewer's scores ranged from 3.0 to 3.8, and the average score for projects was 3.4. The project receiving the highest score was Hydrogen Safety Training for First Responders.

National Codes and Standards Template: Reviewers recognized the value of the template and in particular noted the template's comprehensive content and ability to bring relevant stakeholders together. In particular, reviewers praised the data-driven codes and standards analysis resulting from experts' use of the template. Reviewers suggested expanding the template's level of detail so it can be more relevant for stakeholders, including fuel quality efforts, and providing funding to ensure continual updates to the database.

Codes and Standards Training, Outreach, and Education for Emerging Fuel Cell Technologies: Reviewers praised the project's critical role in implementing hydrogen and fuel cell technologies and its focus on forklifts and backup power, two early market deployments. Reviewers also recognized the collaboration with local fire departments and praised holding workshops at locations where hydrogen and fuel cell technologies are deployed. Reviewers suggested increasing project funding to allow for more training sessions and to allow the project scope to expand into other alternative fuels.

Component Standard Research and Development: Reviewers appreciated the project's well-coordinated alignment of its test program with industry and the appropriate SDOs. Reviewers also praised the round-robin safety sensor testing and international collaboration with Europe's Joint Research Center (JRC). Reviewers suggested fostering additional outreach to industry stakeholders to better understand industry needs. Reviewers suggested the project complete a list of components under consideration, to identify gaps.

Materials and Components Compatibility: Reviewers admired the project's focus on forklifts and its relevance to the early market deployment of hydrogen and fuel cell technologies. Reviewers commended the project for its test facilities, thorough and engineering-based data collection, and test methodology. Also, most reviewers commended the direct interaction with codes and standards committees, tank manufacturers, forklift integrators, and working groups. However, reviewers noted that progress has been slow on material system evaluations and the application of the fatigue crack growth law. The fatigue crack growth law is based on the hypothesis of "leak before break," and it is unclear how the testing program will incorporate the hypothesis into its testing procedures.

Hydrogen Fuel Quality: Reviewers praised the rigorous technical R&D approach used to determine levels of constituents in hydrogen. The reviewers also commended the project's contribution of critical data to the International Organization for Standardization Technical Committee (TC) 197 Working Group 12. Most reviewers noted the strong collaboration between investigators, the strong work plan, and the iterative approach to refine data results. Reviewers suggested including more depth on durability testing at the cell level and greater collaboration with fuel providers.

International Energy Agency Hydrogen Implementing Agreement Task 19—Hydrogen Safety: Reviewers recognized the project's important role in international collaboration. Reviewers commended the project's data collaboration, strong link for input into www.hydrogenincidents.org, and efforts to foster international collaboration. However, most reviewers commented that the project's focus is vague and Task 19's goals need to be better defined. Also, some reviewers noted the project needs to increase collaboration with SDOs such as ISO and the International Electrotechnical Commission.

Hydrogen Release Behavior: Reviewers recognized the strength of the project's research protocol, which translates into a "defensible and traceable basis" for codes and standards development. In particular, reviewers praised the outstanding transformation of scientific analysis into actual safety guidance, and also commended the work done on tunnel release. Reviewers identified areas for improvement, including fostering greater industrial collaboration on indoor refueling and clarifying the direction of tunnel release work.

Hydrogen Safety Knowledge Tools: Reviewers praised the project's depth, breadth, and distribution of hydrogen safety resources. Reviewers also noted other strengths such as the applied expertise of the Hydrogen Safety Panel to the online resources. Reviewers suggested improvements as well, such as greater dissemination of the Web site and increased involvement of energy companies.

Hydrogen Safety Panel: Reviewers agreed that the Hydrogen Safety Panel (HSP) provides critical expertise for ensuring the safety of hydrogen and fuel cell projects. Reviewers thought there was an excellent mix of expertise and experience on the HSP and were impressed with its accomplishments thus far. Reviewers praised the HSP's safety recommendations, which are based on incident reviews, as an excellent resource. Reviewers expressed concerns over how the HSP's effectiveness is evaluated and commented that the HSP might be over-funded.

Hydrogen Safety Training for First Responders: Reviewers praised the project's relevance and important role in advancing the safe deployment of hydrogen and fuel cell technologies. Reviewers identified a number of important strengths, including the focus on real-time training, the accurate targeting of relevant audiences, the well-designed curriculum, the hands-on training afforded by the fuel cell prop, and the ability to move the course to a variety of locations. However, reviewers suggested including increased collaboration with the DOD, onsite training on the East Coast, training specific to forklift operation, and greater outreach to more audiences and locations.

Hydrogen Safety Training for Researchers: Reviewers praised the relevance of the course and its sound technical approach. Reviewers noted that the Web site has an excellent graphical layout and the course reaches out to the correct audiences. Reviewers also saw the strength and importance of the technical expertise and facilities at LLNL that were used to develop the training. Some reviewers noted that it might be useful for the course to be tailored to specific laboratory settings. Also, the course might need to be modified for audiences with different education levels.

Optically Read MEMS Hydrogen Sensor: Reviewers recognized the project's good coordination and technology transfer as strengths. In particular, reviewers noted excellent cooperation between the government and industry. Reviewers suggested improving collaboration with nationally recognized testing laboratories like Underwriters Laboratories (UL) during the testing process.

Safe Detector System for Hydrogen Leaks: Reviewers noted the project's successful R&D towards a commercially available sensor. Reviewers also said the project is "well executed" and has fostered "good collaboration with potential customers." Reviewers supported the project's approach toward sensor development and testing, including collaboration with NREL. Reviewers identified significant technological barriers, including cross interference, humidity, and carbon monoxide poisoning. Also, most reviewers noted the project should not suggest to the public to install sensors in residential garages. Reviewers recognized that the project needs to complete a more thorough cost analysis and clearly identify the size of the end-user market.

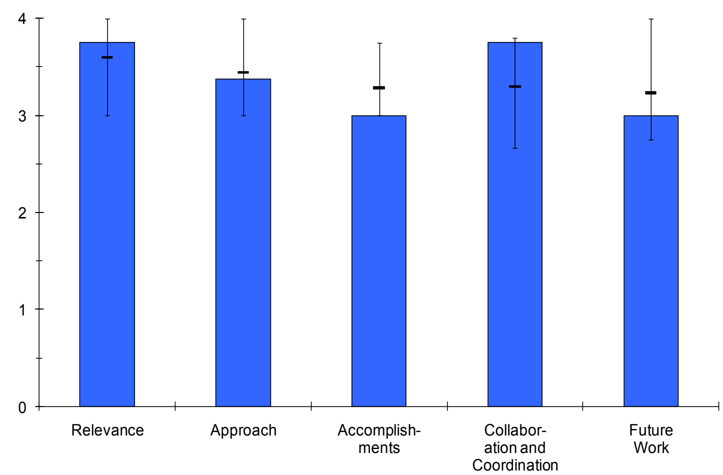
Hydrogen Safety Sensors: The reviewers appreciated the integrated technical approach to R&D and specifically the collaboration between the two national laboratories. Reviewers also supported the inclusion of an industry partner into the sensor testing process. Reviewers praised the project's solid work towards a stable sensor response time, long-term testing, and evaluation of sensor materials and designs to improve long-term stability. Reviewers suggested the project better define the industry partner's role, competencies, and contributions to the project in an effort to improve collaboration. Some reviewers also expressed concern over the project's approach to commercialization in regard to cost goals, performance, and calibration requirements and ultimately the private sector's role in sensor commercialization.

Project # SCS-01: National Codes and Standards Template
Carl Rivkin; National Renewable Energy Laboratory

Brief Summary of Project

The objectives of the project are to: 1) conduct research and development needed to establish sound technical requirements for codes and standards with a major emphasis on hydrogen and fuel cell technologies; 2) support code development for the safe use of hydrogen in commercial, residential and transportation applications with a major emphasis on emerging fuel cell technologies; 3) advance safety, code development and market transformation issues by collaborations with appropriate stakeholders; and 4) facilitate the safe deployment of hydrogen and fuel cell technologies.

Overall Project Score: **3.3** (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Hydrogen and fuel cell technologies, as well as various forms of feed-stock fuels, are not yet an established industry with a legacy record. Instead, it is an emerging technology with many similar, as well as distinctly different properties and safe operating characteristics. Hydrogen and fuel cells are emerging technologies and it is important to have broad engineering and technical freedom to facilitate establishment of commercially viable products. As a custodial government department with many tasks for emerging technology initiatives, the Department of Energy (DOE) has the charge to help ensure that while development is not hindered by restrictive, non-scientifically set requirements, it is facilitated in a socially (including all aspects of life and environment) safe and aptly responsible introduction into the United States and global market.
- The National Codes and Standards Template is extremely useful for organizing hydrogen and fuel cell codes and standards and presenting an overview of the domestic coordination. The R&D that is conducted to establish sound technical requirements is critical to the development of reasonable hydrogen and fuel cell codes and standards that are acceptable to jurisdictions across the country.
- Data driven codes and standards for stationary and mobile applications, supporting the deployment of infrastructure, permitting, quality and safety, are critical to advancing this industry. The codes and standards template that has been developed is comprehensive and addresses all aspects of the technology.
- The project addresses a fundamental need in the path to adoption of hydrogen and fuel cell technologies. The gap analysis on stationary applications is especially relevant, given the current direction of the Fuel Cell Technologies Program.
- This program is a single leg of a three-legged stool. Without it, the stool falls.
- The work aligns with the Program goals to ensure sound engineering practices are developed and used for technical standards and building codes.
- There was an extremely large budget reduction from the 2008 actual, the plan for 2009 and the actual for 2009 which impacted all of the work. However, the Principle Investigator (PI) never mentioned this in the presentation or its impacts on milestones, standards development organizations or the work that was not completed on component testing.
- This project is a critical enabler for progress in hydrogen and fuel cell technologies.
- A comprehensive directory of standards and regulations is a great help to researchers, developers, users and authorities having jurisdiction over buildings as well as the general public.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- It has been almost ten years in the making, but when one reflects back eight or ten years to the many chaotic, separate orbits of the state of technology regarding safety, codes and standards and regulatory work and initiatives, one can appreciate the current state of work and how far we have come. The approaches have at times seemed long and tenuous, but the merits of step-by-step patience and persistent coordination are now starting to pay big dividends for this technology's move to full commercialization.
- DOE and national laboratory involvement in key technical committees is an excellent approach to drive toward the completion of codes and standards. Holding multiple stakeholder workshops to gather input from the field is a good approach to identifying gaps in the codes and standards that need to be addressed.
- The template is comprehensive, the project brings stakeholders together and several research projects have been initiated through the program to develop data driven codes or standards to address identified gaps.
- The project encompasses a very broad range of topics, but manages to do so in a concise and coherent manner. In this case, it is difficult to be "sharply focused", but the project still manages to address all relevant aspects of codes and standards development for hydrogen and fuel cells.
- International cooperation should be emphasized.
- The work supported in 2010 appears more supportive than prior years. Since the budget was reduced, it appears all R&D component testing was stopped, but no data was provided and any impact on current consensus standards was not discussed.
- If there was a gap analysis and an evaluation on program impacts on safety, codes and standards activities, it was not reported and the recommendations on future directions were not clearly indicated.
- Having researchers involved in standardization committees is critical, and this is well implemented. International collaboration in research leading to standards and addressing gaps could be strengthened.
- A comprehensive listing of federal regulations for hydrogen and hydrogen usage was available at one time at www.hydrogen.gov. This listing has been removed, but should be provided to stakeholders for their information. This would be an improvement and would restore this resource.
- The comprehensive listing on www.fuelcellstandards.com is more complete than the codes and standards template. Although the template has value, the fuelcellstandards.com resource is actually significantly more complete. Harmonizing the template with fuelcellstandards.com would be an improvement.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- I have moved up my assessment recently in this area. Several pressing engineering, technical and servicing issues have not been given the attention and/or funding over the years. However, there has been very encouraging progress in this area in recent reporting cycles. I envision continued support in this area. Slides 14 and 15 are outstanding and should be utilized in specific stakeholder coordination meetings.
- The draft gap analysis that the National Renewable Energy Laboratory (NREL) produced for fuel cell technologies shows that there are no significant gaps for fuel cells, but some work is needed on component standards for high and low-pressure systems. This is an important accomplishment to focus future efforts where they are most needed. Analysis support for the National Fire Protection Association Hydrogen Technologies Code (NFPA 2) is also a key contribution.
- Advancements have been made in many areas: refueling, building codes, permitting, pressure vessel safety, fuel quality, sensor development and harmonization of codes and standards.
- The technical accomplishments for this year have made significant progress toward achieving the project goals, bearing in mind that codes and standards work is an ongoing process. As mentioned above, the inclusion of a gap analysis for stationary applications is a good example of the accomplishments of this project.
- The progress is steady. The national codes and standards template is a useful tool for tracking projects, gaps and collaborations.
- The PI has stated that the gap analysis was completed and identified where additional codes and standards work was needed, but did not state it in the presentation.

- The PI stated issues identified component standards, but did not provide any details or if they had made any progress.
- The PI stated modeling and analysis of data was conducted to support NFPA 2, but did not provide any details.
- Future work did not identify how the effort would deal with budget issues, curtailing of the component testing or any details on evaluation or analysis activities.
- Based on the work reported, the only real progress was attending technical committee meetings.
- Project name "National Codes and Standards Template" is misleading for the activities the Project actually covered. Obviously, two templates are available and hence the project goal is achieved. However, main project activities consist of associated needed research and active involvement in and facilitation of standards development organization (SDO) and code development organization (CDO) activities. It is difficult to assess, based on the information contained in the presentation, to what degree these have progressed against (non-communicated) performance indicators.
- Note that templates shown are different from those on the DOE website (www.hydrogenandfuelcells.energy.gov/codes/pdfs/cs_templates.pdf).
- The template is useful, but lacks specifics. Grouping the various aspects of hydrogen standards into the various genres of "Vehicles", "Fuel Delivery and Storage", "Fueling, Service, Parking Facility", "(Vehicle Systems and Refueling Facilities) Interface", "Hydrogen Generator", "Portable Fuel Cells", "Stationary Fuel Cells", and "(Stationary and Portable) Interface" is helpful, but this breakdown is not sufficient for use by stakeholders. More detail is necessary for stakeholders to appreciate the applicable standards and to access them for their work.
- There is a significantly different technology that has been omitted. "Micro" fuel cells are portable and operate at low voltages and currents. This type of fuel cell needs to be separately addressed, since such systems are expected to be routinely used in public spaces, as well as on board planes, trains and automobiles. The regulations for such usages are important since flammable, corrosive, explosive and water reactive fuels for such devices are typically not allowed in public places and on board public transportation. This is a sensitive issue that needs to be addressed carefully.
- Conducting gap analyses on a periodic basis is very valuable. Including industry in these analyses is vital to Project success.
- The Hydrogen Industry Panels on Codes (HIPOC) is limited to the NFPA and International Code Council (ICC), but many more interrelated standards are involved in the codes and standards issue. Either expanding HIPOC to include all interrelated standards or eliminating it in favor of using other aspects of the program would be an efficiency improvement.

<u>Question 4: Technology transfer/collaborations with industry, universities and other laboratories</u>

This project was rated **3.8** for technology transfer and collaboration.

- The U.S. national codes and standards landscape is now at a point of relative predictability. Consistent, patient and persistent efforts have breached perceived silos of code and standards makers.
- The Project has outstanding collaboration with industry, national labs, SDOs and CDOs. Extensive collaboration and coordination efforts with all the key players are highly commendable. These collaborations ensure that multiple perspectives will be articulated, with the end goal of finding common ground and achieving national consensus on hydrogen codes and standards.
- Collaboration is excellent between DOE and all applicable SDOs, industry, the national labs, other federal agencies and local authorities and international partners.
- The project has significant collaboration from all relevant stakeholders.
- The coordination is good, while the collaboration is only as good as the desire of the partner.
- The coordination plan was well conceived and executed. However, it’s difficult to keep everyone focused on a single outcome if there are insufficient funds to support the needed work.
- If budget issues continue, the scope and goals of this effort should change to reflect the reduced funding and ability to accomplish the template objectives.
- I got a positive impression that effective collaboration is in place with the listed collaborating institutions.
- As stated earlier, HIPOC is limited in scope and could either be expanded to include all interrelated standards or eliminated in favor of using other aspects of the program to coordinate between the various standards for hydrogen. This would be an efficiency improvement.

- The collaboration and coordination with national and international SDOs, as well as collaboration with national and international regulatory bodies, is impressive.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Future outlooks based on the PI's outlook summaries being right on track. Ensuring close scrutiny in the integrity of task work and deliverables, as well as analysis and data disbursement and feedback, is critical at this point.
- Future work includes a good combination of component testing, data analysis, collaboration/coordination with SDOs and CDOs and outreach activities to assist code officials and project developers using the codes and standards in the real world. Evaluation of indoor releases of hydrogen from forklift refueling operations is a timely undertaking.
- The approach to and relevance of proposed future research are both good. The plans build on past progress.
- Inclusions of forklifts in the future work as well as ongoing component testing and fuel quality work are all good indications that the future of the project has been well thought-out. It would have been nice, however, to see some more specific timelines for some of the work.
- The identification of low pressure system requirements, plastics and composites, and the need for standards development in this realm will be key in both vehicle and stationary applications. More emphasis should be given on international standardization.
- As stated earlier, the scope of work should reflect the budget to be obligated to the project. The current plan is too broad, based on the expected budget, and cannot support any R&D activities on components, unless industry is willing to provide the funding.
- The presentation discussed future work that was not precisely involved in the codes and standards template itself, but rather indicated component work and release evaluations and hydrogen quality efforts, as well as direct support of SDOs. This listing is not precisely the implementation of the template.
- It would be helpful to get more information on how the template will continue to be updated and how industry input will be obtained.

Strengths and weaknesses

Strengths

- This activity has been at a relatively high pace for the last six+ years. The amount and importance of stakeholders, though significantly reduced over the years, is still significant and should be sustainable through "establishment" of needed rules, regulations, standards, codes and recommended practices.
- The key project strength is the extensive collaborations with industry, national labs, SDOs and CDOs to create nationally accepted codes and standards. The integrative approach of blending R&D activities with committee work on codes and standards makes good sense. Holding workshops in areas where hydrogen and fuel cell projects are happening is also an important contribution.
- This project has been active for several years and has become very comprehensive in scope, which makes it a natural go-to entity to facilitate smaller, more focused, individual research topics. For example, research to support the development of SAE 2579's durability and expected service life test protocol for onboard hydrogen storage cylinders was funded through this project. Similarly focused projects to address gaps (such as sensor sensitivity, for example) can be initiated rapidly through similar methods, since relationships with stakeholders are well established.
- The coordination and collaboration efforts are key and a strength of the program.
- Good history and overall accomplishments in getting ICC and NFPA codes developed and adopted.
- A strength was the focus on the central point on regulations, codes and standards.
- The goal of a comprehensive listing of hydrogen and fuel cell standards is laudable and valuable. The significant level of effort involved is appreciated by all stakeholders. The project and the program are showing excellent effort and progress towards these goals.
- Expanding the template to include the detail necessary for it to be used by the experts, such as standards titles and scope, would be an improvement.

Weaknesses

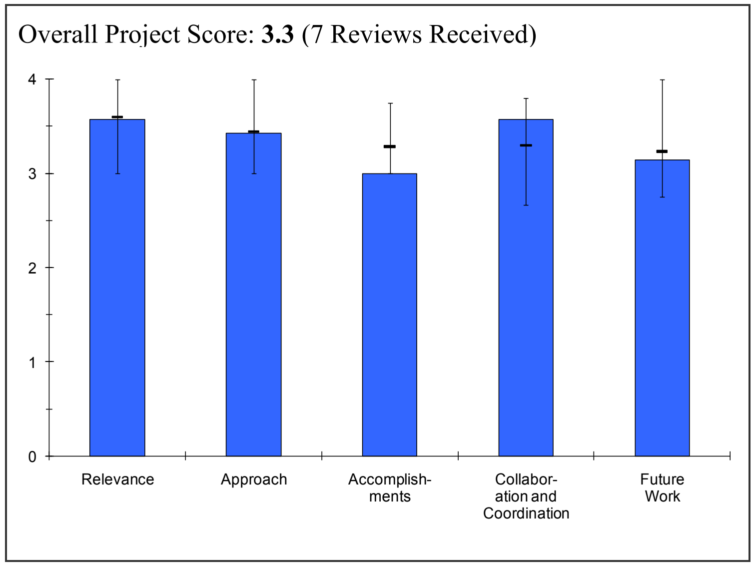
- Funding and ensuring that there is a sustainable flow of historical knowledge is a weakness.
- It seems transfer and key stakeholders pass "point-of-contact" batons.
- R&D results may lag behind codes and standards revision cycles.
- Funding may be insufficient to do much more than maintain the template. Is there a list of unfunded projects to support research to address gaps in codes and standards?
- The Project should refocus activities based on budget realities.
- The dissemination in the international arena about scope of activities and accomplishments could possibly be improved.
- The format of the template is not detailed enough to be really useful to knowledgeable stakeholders. The comprehensive listing at www.fuelcellstandards.com is more useable. It would be helpful to have a more comprehensive template, or just reference www.fuelcells.com or portions thereof.
- The original concept of assigning responsibility to a specific organization for a specific topic has been somewhat lost due to overlap of standards, subsidiary standards and the competitive nature of standards development.
- The American National Standards Institute (ANSI) system of assigning responsibility to a particular organization for a particular topic is in place, and DOE does not need or want to replicate this function. Any idea that DOE is regulating standards work needs to be modified to be sure that ANSI is not superseded.

Specific recommendations and additions or deletions to the work scope

- It may be beneficial to start tracking "joint U.S. Technical Advisory Group efforts" with regard to specific task items within the various International Organization of Standardization (ISO) and International Electrotechnical Commission (IEC) activities. A better understanding of international protocols within the ISO, IEC and Global Technical Regulation (GTR) processes should reap significant dividends.
- The Project should continue along the same path.
- A single comprehensive database for national and possibly international codes and standards as applied to hydrogen and hydrogen systems should be implemented.
- Keep work focused on application of the model codes through training, workshops, and outreach activities. Perform more analysis and modeling activities since they are less expensive and get more technical papers published on these efforts.
- Include balance-of-plant related impurities in the fuel quality standard.
- The permitting of hydrogen refueling stations.
- Expanding the template to include the detail necessary for it to be used by the experts, such as standards titles and scope, would be an improvement.

Project # SCS-02: Component Standard Research and Development
Robert Burgess; National Renewable Energy Laboratory

Brief Summary of Project



The objective of this project is to develop component level hydrogen codes and standards by identifying gaps and working with industry to close those gaps via national laboratory R&D support. Hydrogen infrastructure technology gaps include: 1) a new addition to the American Society of Mechanical Engineers (ASME) Boiler and Pressure Vessel Code test standard for composite overwrapped pressure vessels; 2) new non-communication fill tables for hydrogen vehicle fueling for the Society of Automotive Engineers (SAE) J2601 Fueling Protocol, designed to insure temperature limits are not exceeded; 3) new performance-based standard for temperature activated pressure relief device; and 4) hydrogen sensor performance requirements for hydrogen leak detection for safe alarm and shutdown.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The four projects discussed in the presentation do address areas in codes and standards development where data or adequate hardware (sensors) is lacking.
- This project provides vital technical work which helps to overcome gaps in the current codes and standards for hydrogen and fuel cell technologies. As such, the results are essential to the DOE Fuel Cell Technologies Program.
- The project is aligned with Program needs.
- Hydrogen system component testing performed in this project is critical to the development of performance standards for SAE, CSA and ASME. The test program is generally aligned with industry.
- The project appropriately addresses a number of subjects requiring standardization.
- Component standards are essential to safe and cost effective development of hydrogen and fuel cell technologies.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- These projects are focused on specific technical barriers that need to be addressed.
- The project is directly linked with the national codes and standards template development and addressed gaps which have been identified through that effort. The approach taken in each of the four gap analyses that were discussed is thorough, comprehensive and unbiased.
- The overall approach needs to be clarified and better articulated.
- The work is subcontracted to appropriate outside experts.
- A direct line of communication between the standards development organization (SDO) and National Renewable Energy Laboratory (NREL) technical staff is helpful to industry. Regular updates to those SDOs allow corrective feedback from industry. I would like to see targeted briefings more frequently during the project lifetime summarizing work-to-date and emphasizing results relevant to that SDO.

- The topics addressed respond to priorities expressed by stakeholders. They seem to be appropriately covered either by in-house activity or by subcontracting.
- The technical work appears to be rigorous and well done and based on previous research and experience. The laboratory work is state-of-the-art.
- The primary effort appears to be on sensors.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The progress made is good. The rate of progress is a little slow.
- Most of the accomplishments discussed were for the work on sensor testing. This work has made significant advances in the last year and specifically on the sensor analysis.
- There was little discussion on the progress of the pressure relief device (PRD) hydrogen service suitability. Though it is understood this is ongoing work, more detail would have been appreciated.
- There was too much technical data in the accomplishments section. Bullets are needed regarding specific components under consideration. The presentation makes it hard to discern exactly what has been accomplished in the past year.
- Various subcontractors are providing results.
- Safety sensor testing is showing progress.
- Fueling protocol testing will need to be expanded to include industrial trucks. This is a need for the industry.
- In the absence of any communicated performance indicators or milestones it is difficult to quantitatively assess the degree of progress. From a qualitative point of view, progress certainly seems positive.
- All progress appears to be timely and continues to support industry needs.
- The use of round-robin testing for sensors appears to be an excellent approach. This is a project strength.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- The collaborations are well coordinated, and all the right entities are involved (SAE, CSA, National Institute of Standards and Technology (NIST), etc.).
- This project has a long list of collaborators. It was especially nice to see interaction with the international community. The round-robin testing with Joint Research Center (JRC) is a good illustration of how international collaboration can be used to further a technology.
- It is hard to tell from the presentation how well partners are coordinated and if they are full participants or simply subcontractors.
- The project has good working relationships with subcontractors and some of the many sensor manufacturers.
- Industry and government are both represented in working groups within individual SDOs.
- The project allows for informal updates and real-time information exchange.
- There was collaboration with relevant partners beyond the United States.
- Based on the presentation, the sources of research topics are all good, but could be expanded to include more comprehensive polling of industry stakeholders in order to include more industry and standards developers as well as all national labs and the research community.
- The collaboration on round-robin testing is a strength.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- The plan to collaborate with National Aeronautics and Space Administration (NASA) on tank level stress rupture testing is a good one. Some research to verify a leak before burst failure mode for composite overwrap pressure vessels would improve safety by preventing catastrophic rupture of Type IV cylinders.

- The future work is well presented, and takes into account further progress which must be made on the PRD task. It would have been nice to see some timelines suggested for the work.
- The inclusion of computational fluid dynamics (CFD) modeling for sensor placement is also a very good idea as the project moves forward.
- This was an appropriate continuation of existing subcontracts and collaborations.
- I would like to see permeation testing of plastic materials for use in low-pressure hydrogen applications.
- Completing the sensor work is essential. The next sets of work topics will most likely change as industry input is received. The Program needs to be flexible and funded for emerging issues. One good example is forklift fuel tanks where the issue developed quickly and DOE was quick to implement vital research. This component program needs to be funded at a level that allows such flexibility.

Strengths and weaknesses

Strengths
- The project provides much needed technical information to address gaps in hydrogen and fuel cell codes and standards. The thoroughness of the work, as well as the collaboration with industry and international laboratories, are all project strengths.
- The specific component work is crucial to overall DOE objectives.
- There are appropriate subcontracted efforts. The collaboration with international experts should improve project output.
- There is close cooperation between industry and performing labs.
- The project follows stakeholder prioritization.
- The international collaboration is a strength.
- The technical excellence on display was a noticeable strength.
- The round-robin testing was beneficial.
- The collaboration with national and international stakeholders and labs is a project strength.

Weaknesses
- Is funding sufficient to continue sensor and composite overwrap pressure vessel testing?
- A comprehensive list of components under consideration would be helpful in identifying gaps in hydrogen-specific component needs.
- It seems like a lot of work is required on sensors to generate an "abstain" vote at the international level. One would like to see more active presentation of data to support international standards.
- It is always difficult to ferret out the needs of industry. Additional outreach to industry stakeholders will almost always bear fruit. Additional outreach is encouraged.

Specific recommendations and additions or deletions to the work scope

- Add component testing in a low-pressure hydrogen environment.
- Add permeation testing of plastics for low-pressure hydrogen applications and industrial truck refueling protocols.
- There needs to be microstructural investigations on failed or inadequately responding sensors to clarify limiting factors for detector performance and durability (preferentially to be done in collaboration with external partners).
- More outreach to stakeholders and industry partners is encouraged.

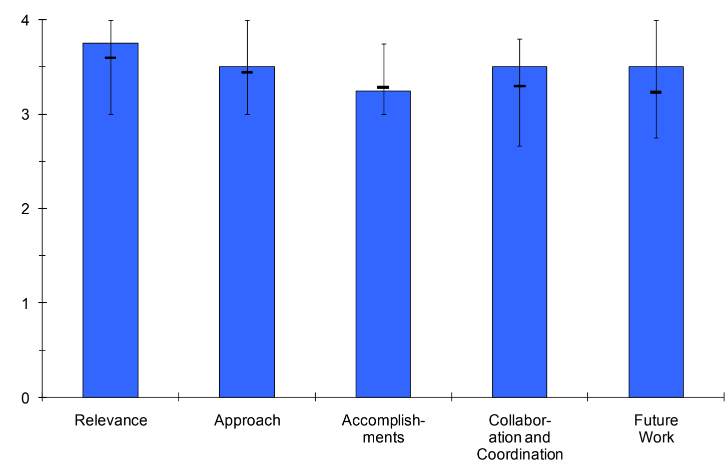
Project # SCS-03: Codes and Standards Training and Outreach and Education for Emerging Fuel Cell Technologies

Carl Rivkin; National Renewable Energy Laboratory

Brief Summary of Project

The objectives of this project are to: 1) advance renewable energy safety, code development and market transformation issues by distribution of information; 2) facilitate the safe deployment of renewable energy technologies; and 3) overcome barriers to emerging fuel cell technologies, and specifically fuel cell-powered forklift vehicles and stationary fuel cells used for back-up power.

Overall Project Score: **3.5** (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This is a keeper. The PI has done an outstanding job with this program as he came onboard during the first "hard lesson" session then refined it to the value-added product that it is today!
- Providing information on safe deployment to code officials and project developers is critical to furthering the implementation of hydrogen and fuel cell technologies. Offering practical guidance on how to apply hydrogen and fuel cell codes and standards will speed up the time required for project development and final reviews.
- This work is critical to the efforts in implementing hydrogen and fuel cell technologies (vehicles and fueling stations, stationary applications, etc.).
- DOE coordinating and leading this project is key. This emphasizes the move to commercialization and demonstrates a real leadership and progressive role of the government.
- The project directly correlates to addressing DOE objectives. The focus on forklifts and backup power is relevant and needed at this stage of commercialization.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- It took several iterations to hammer out a professional and palatable (to the target audience) product, but the final product is truly outstanding!
- Holding workshops in locations where there are actual hydrogen and fuel cell applications makes good sense. Such workshops will make code officials and project developers aware of the codes and standards and how to apply them, facilitate safe technology deployments and help define future R&D needs.
- There was good coverage of many types of hydrogen projects (stationary, forklift and passenger vehicle fueling). It is important to demonstrate the similarities in the permitting processes of these various technologies.
- In-person interactions are proven time and again to be the best way to work with authorities having jurisdiction (AHJs). It's good to see such a focus on those interactions and to see positive results as indicated by the workshop participants. The web-based part is also important since the folks in the workshop and others will have a way to access the same information if they are unable to attend the workshops.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Continued focus on feedback and perhaps targeted "revisits" are the correct and chosen approach. Well done!
- Several workshops were held and others are planned. The project works directly with the local fire departments and the California Fuel Cell Partnership (CaFCP). The web-based information compendium was developed and maintained and the code official training course was successfully deployed. A site visit and case study report on stationary fuel cells for backup power was completed.
- Keep the progress moving. This project is needed on an ongoing basis with the advancement of technology implementation (i.e. as new technologies are developed and the current technologies become more widespread).
- Accomplishments and progress are really hard to measure, but from the information given it appears that the project has been quite successful. I am troubled by the use of the term "electric vehicles", which seems to only apply to battery vehicles. Electric vehicles include fuel cell electric vehicles. Every time we use this term incorrectly (as so many people do), we're teaching the wrong perspective to our target audience. I suggest changing the language to use electric vehicles when actually referring to vehicles powered by fuel cells and/or batteries; and using battery vehicles or fuel cell vehicles when only referring to those individual technologies.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- The initial efforts ran through a gambit of stakeholder groups and individuals. The core team seems to be doing a splendid job.
- Collaboration with local and regional fire departments and the CaFCP is good.
- It is difficult to address the challenge of reaching the exact right people. It is critical to have the input of the collaborators to correctly target the geographical locations (cities) and individuals in those areas.
- It's quite clear that the coordination is far reaching including all the major organizations that are involved in safety, codes and standards development. The only improvement that I can see is incorporating speakers from other organizations in the agenda. The Jefferson Parish workshop agenda, for example, only NREL speakers. I realize there are cost restrictions and the current approach seems to work well, but that would be one place to improve collaboration especially as it's perceived by the audience.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- The project plan appears to be on-track and still targeted to the correct audience in order to bring the most value-added outreach and education where needed. Again, well organized and well done!
- A site visit and report on indoor hydrogen forklift refueling is planned. NREL plans to continue the workshops and safety reviews and collaborate with local and regional organizations. The workshops will be used to help define potential hydrogen and fuel cell codes and standards issues and future R&D needs.
- It can be beneficial to the permitting officials to demonstrate similar features in the permitting process between hydrogen and other alternative fuels, however, the other alternative fuels tend to be more similar to conventional fuels permitting (compressed natural gas, ethanol, natural gas, etc.), where hydrogen is/can be quite different. For this reason, the primary focus should remain on hydrogen.
- Approach and relevance are very good. The current work should continue.

Strengths and weaknesses

Strengths

- Brings "real-time" expertise and information to "real-time" installations and projects. The work supports where it is actually needed.
- The key project strength is getting out into appropriate locations in the field to provide information directly to code officials and project developers on hydrogen and fuel cell technologies. It helps these individuals to then understand the context so they can more efficiently apply the codes and standards in their own projects.
- Good early outreach. Good early outreach is much needed both from an educational perspective and in a practical sense.

- The project gives permitting officials good resources of information, which will ultimately instill confidence in the technology (some evidence of this is in the comments slide).
- A strength of the project is tracking the changing codes and standards and updating AHJs as needed through both the workshops and the online resources. This keeps progress advancing.
- This project represents the main ways that we know work well to develop codes and standards and aid their implementation; workshops and online materials. This bread and butter approach works and simply needs to be executed more often to more people.

Weaknesses

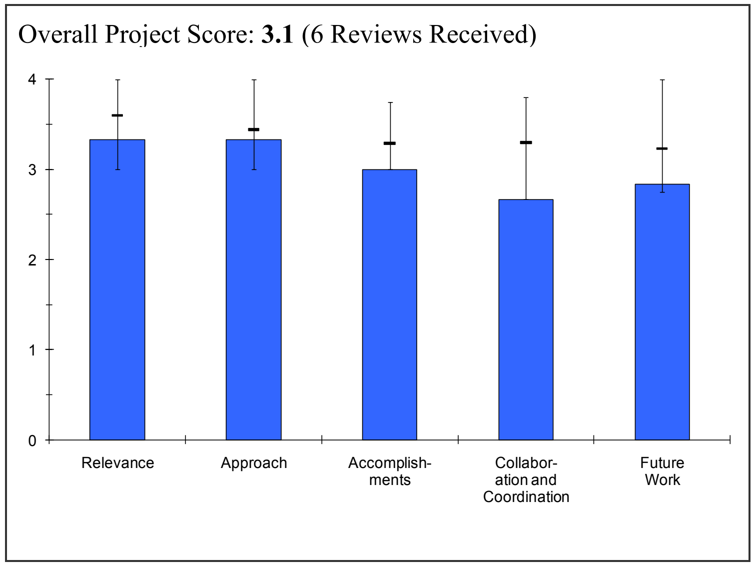
- An additional team would be beneficial.
- Code officials wanted to know how the information might affect their daily lives. Thus, more attention should be focused on this issue during the workshops.
- Moving away from the hydrogen focus and adding other alternative fuels potentially expands the scope to a degree that makes it very difficult to organize and manage.
- There are very few weaknesses. There could be some increased collaboration and volume of outreach to more people and the change in terminology with "electric vehicles”.

Specific recommendations and additions or deletions to the work scope

- Add more funding to allow for return/follow-up sessions.
- Perhaps an occasional outreach to a regional training center that caters to a number of volunteer stations and shops.
- NREL needs to develop materials to publicize the availability of the web-based code official training and the local workshops.
- My preference is for the focus to remain on hydrogen fuel cell vehicles, fueling stations and stationary installations.
- This bread and butter approach works and simply needs to be executed more often to more people.

Project # SCS-04: Hydrogen Safety Sensors
Eric Brosha; Los Alamos National Laboratory

Brief Summary of Project



The objectives of this project are to: 1) develop a low-cost, low-power, durable and reliable hydrogen safety sensor for vehicle and infrastructure applications; 2) demonstrate working technology through application of commercial and reproducible manufacturing methods and rigorous life testing results guided by materials selection, sensor design and electrochemical research and development investigation; 3) recommend sensor technologies and instrumentation approaches for engineering design; and 4) disseminate packaged prototypes to DOE laboratories and commercial parties interested in testing and fielding advanced commercial prototypes while transferring technology to industry.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The development of accurate, low-cost and robust hydrogen gas sensors is important for both stationary and mobile applications.
- There is a critical need for a low-cost, low-power, durable and reliable hydrogen sensor for vehicles and stationary applications to help foster the transition to the hydrogen economy. This project directly addresses several key barriers identified in the DOE Multi-Year Program Plan by developing robust solid-state electrochemical hydrogen sensors.
- This is an issue that may be better served by the efforts of private industry. This project should not be viewed as a critical path in support of the hydrogen program goals.
- Development of sensors that can meet DOE targets is an important part of the Fuel Cell Technologies Program. The project, if successful, will provide robust and low-cost solid-state sensors for vehicular applications. It is not clear how much value the success of developing and commercializing such sensors will have for stationary applications, such as fueling stations, above commercially available safety sensor technology.
- Cost-effective hydrogen sensor technology is an enabler for more robust hydrogen safety practices.
- Sensors appear to be a necessary component of vehicle and stationary and portable fuel cell systems. Some developers are unwilling to include standards due to concerns regarding reliability, false alarms, stability and cost. Having reliable and cost-effective sensors is essential.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The approach taken is sound.
- The project objectives are to develop and demonstrate the sensor and then disseminate it to other national laboratories for testing while simultaneously transferring the technology to industry for commercialization. The approach utilizes two national laboratories with unique and complementary expertise, as well as an industry partner with the ability to engineer commercial prototypes. That appears to be a smart and integrated approach.
- Approach is fine but may be better with more involvement of private industry.

- Integration of commercialization parameters into the RD&D is an approach that should be encouraged, particularly for projects that should lead to widespread deployment in one or more industries. The partnership between Los Alamos National Laboratory (LANL) and Lawrence Livermore National Laboratory (LLNL) is a good example of combining complementary expertise and experience. The presentation could have shown better how the project is addressing not only the identified technical barriers, but also each of the DOE hydrogen sensor targets.
- The project is claiming to be 60% complete, yet there is no manufacturability, packaging or cost information presented. Some assessment of these aspects should have occurred by this point. The durability technical performance target of five years between calibrations is suitable for vehicular applications but is overly stringent for fixed installations.
- Using a DOE workshop to set the goals could be a good method provided that industry was involved.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The accomplishments to date are encouraging.
- One question comes to mind. One example given has a high operating temperature. If the operating temperature is above the autothermal ignition point of the gas being sampled, does the sensor become the ignition source during a catastrophic failure of the gas containment (e.g. hose break)? Is this being considered in the development process?
- In Fiscal Year (FY) 2009 the team completed an early commercial prototype sensor platform which significantly exceeded their 500-hour testing goal and obtained industry input on commercialization potential. Milestones were accomplished for sensor prototype development, achievement of a stable sensor response over time, long-term testing, evaluation of sensor materials and designs to improve long-term stability and characterization of alternative modalities.
- Accomplishments have been impressive but commercialization efforts are the realm of private industry.
- The project has demonstrated progress in meeting objectives specified in its project plan. Although funding issues have impeded progress, it is not clear how 60% of project completion aligns with the key project objective of developing a prototype sensor that can be transferred to industry for commercialization.
- The progress towards technical targets is not presented clearly and there is no cost information available. It is difficult to interpret some of the results, since they are presented as sensor signal output rather than translated into hydrogen concentration readings (and errors). The project claims that mixed potential sensors have exceptional stability but the test results presented do not seem to exhibit even acceptable stability.
- The presentation showed that problems in design have been overcome with good engineering. Design improvements continue.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- The collaboration partners seem to be sufficient. It would have been nice to see collaboration with a potential end user to validate that the device will meet the application needs.
- LANL is working with LLNL and private industry on this effort and effectively utilizing the technical strengths of each collaborator.
- This effort should be passed off to private industry for further development and cost reduction for commercialization. This project has been a two horse show.
- The partnership between LANL and LLNL appears to be effective and has led to good science and research and development. The role, competencies and contributions of the industry partner, ElectroScience Laboratory (ESL), to the project are not clear although it appears that ESL has fabricated at least one prototype. Inclusion of an automotive original equipment manufacturer (OEM) as a partner would be helpful, especially as the sensor is intended for vehicular applications.
- While the partners seem to have appropriate experience with sensor technology development and are cooperating, the project needs more manufacturing, packaging and cost input.

- It appears that LANL and LLNL is actually developing sensors that are going to be used as commercial products. This is in contrast to other DOE programs where competitions and private company offerings are solicited, rather than using the national laboratory as the developer of the device. It is not clear which approach is better, but including additional experienced private companies cannot be detrimental to the program. The presentation only mentioned one private company partner, and they are not a sensor manufacturer. Additional private company partners, including sensor manufacturers, might provide additional benefits.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The future work seems appropriate for developing a prototype. Some thought needs to be spent on goals for commercialization. Additionally, unit cost goals were not mentioned.
- The team will continue to evaluate materials, designs and fabrication processes, fabricate prototypes with non-platinum electrodes, investigate mass production of sensors with long-term stability and assess sensitivity of the sensors to interfering gases and operating temperature variations.
- National labs should not be concerned with commercialization.
- The parameters of the go/no-go decision should be enumerated as should the status of the project in meeting performance measures that would contribute toward a "go" decision.
- Most, if not all, future work identified focuses on technical aspects. More emphasis needs to be placed on manufacturing, packaging and cost.
- New iterative designs are planned. Since LANL and LLNL are essentially inventing this technology, new configurations are planned. It is not clear where these developmental designs will lead.

Strengths and weaknesses

Strengths
- The strengths appear to be the technology and approach.
- The project strength is the previous experience and capabilities of the project team members.
- The project has accomplished the majority of stated goals.
- This is good science that furthers understanding of materials issues involved in solid-state sensor development for hydrogen applications. Incorporation of low-cost, mass manufacturability into project objectives and design is a major strength of the project.
- The innovative design efforts were strong.
- The lab collaboration was strong.

Weaknesses
- The weaknesses appear to be the lack of thought on commercialization, cost goals, performance requirements and calibration requirements. A set of preliminary functional requirements (goals) might be helpful in guiding development.
- There are no weaknesses identified.
- It needs more explicit alignment with targeted end-use since the requirements for vehicular and stationary applications could be very different.
- The progress toward technical targets is not clear and there is no information regarding cost target progress.
- There does not appear to be any active sensor manufacturer involvement.

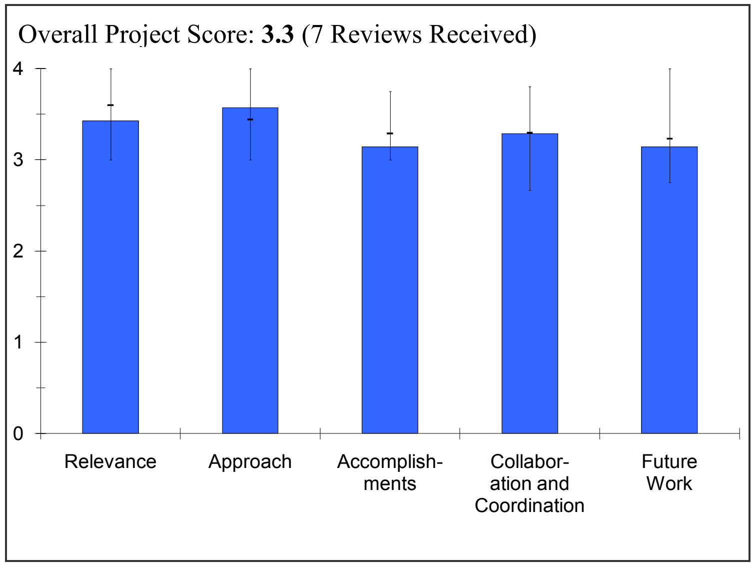
Specific recommendations and additions or deletions to the work scope

- Could the operating range be expanded on the low-end to -50°C, which is the worst case in the populated areas on North America and Europe, and on the high-end to 85°C, which is the upper level limit for non-motor compartment (SAE J1211)?
- Is environmental testing to the U.S. Military Standard (MIL-STD-810) or the American Society for Testing and Materials (ASTM) equivalents envisioned?

- Is a product listing to the Underwriter Laboratories (UL) Standard for Safety Gas and Vapor Detectors and Sensors (UL 2075) under consideration?
- They should continue with planned scope.
- Details of the go/no-go decision parameters should be addressed and requirements for the most feasible applications should be considered in project metrics.
- More collaboration with sensor manufacturers is recommended to add value to the program.

Project # SCS-05: Materials and Components Compatibility
Daniel Dedrick; Sandia National Laboratories

Brief Summary of Project



The overall objective of this project is to enable market transformation through development and application of standards for hydrogen components. Objectives are to: 1) create materials reference guide ("Technical Reference") and identify material property data gaps; 2) execute materials testing following existing standards to meet immediate needs for data in technology deployment, with an emphasis on steel hydrogen storage tanks in FY 09-10; 3) provide data that demonstrates how to improve efficiency and reliability of materials test methods in standards, with an emphasis on fatigue crack growth test methods in FY 09-10; and 4) participate directly in standards development, including component/system design qualification standards such as ASME Article KD-10, CSA Hydrogen Powered Industrial Truck (HPIT)1, SAE J2579 and materials testing standards such as SAE/CSA.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The work carried out in this project provides valuable data which is useful to safety, codes and standards committees as they develop new codes for hydrogen and fuel cell technologies.
- The test methodology is keystone to the development of safety, codes and standards.
- The data collection and evaluation provide an important service to researchers and developers.
- The work conducted by Sandia National Laboratories (SNL) on metallic tank failures is important for manufacturers of current high pressure tanks, but should only be a reference to establish a baseline for advanced material systems and designs.
- Since the current focus of fuel cell powered forklift vehicles seems to be a high priority, using this system as the initial material system seems reasonable. However, based on advanced modeling and analysis techniques, accelerated methods of testing need to be established to better use the single SNL facility.
- Work ties in with development of both United States and international standards. The successful completion of this project will provide a materials qualification guide for bringing new materials into acceptable use.
- The topic treated is relevant for application of hydrogen technologies, but seems to be very specific in terms of testing method, component type and material.
- The work contributes to advancing the development of hydrogen use.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- The approach is focused on certain pressing issues (like the steel hydrogen storage tanks) and is carried out at state-of-the-art testing facilities.
- Direct participation with the SDOs fills not only immediate needs in standards development, but sets the groundwork for future development as well.
- It is a well-integrated approach to identifying needs, filling data gaps and communicating results to stakeholders.

- Interaction with the ASME has led to the development of recommendations using data-supported improvements of the test method, which is an important, cost-effective and technical contribution to the field.
- The current work is focused on dealing with the barriers of existing technology and material systems. However, these are not expected to be the technology needed for large market penetration.
- More work needs to be focused on the use of accelerated methods of testing both to reduce the cost, duration and complexity of this testing. New consensus standards for the ASTM or NIST using these accelerated test methods must be developed.
- Just as we have moved beyond dye penetration methods for crack propagation, we need to focus on other methods to examine and measure changes in material properties.
- The relationships with stakeholders are critical.
- Whereas the overall approach and quality of the work seems appropriate, there may be a deficiency in attempting to explain the observed fatigue crack growth behavior from the point-of-view of underlying materials science. No explanation is given for the non-traditional growth curve. Such understanding is needed for applying the measured crack growth law with some degree of confidence to predict in-service life. Also, extrapolation of crack growth measurements from uniaxial tests on compact tension (CT) specimens to multi-axially stressed components (because of internal pressurization and bending moments associated with presence of defects of considerable size), is not sufficiently clear (at least not from the information provided during the presentation and contained in the slides). The effect of test gas purity on results was not clear either.
- The test conditions must better simulate the real world for the result to be applicable.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **3.1** based on accomplishments.

- There have been significant accomplishments in this year of the project. The method that the presenter used to put the actual accomplishments into perspective, and show specifically how the experimental results have affected code -development is welcome.
- The specific work analyzing forklift applications has had good progress in a relatively short timeframe. This work will be essential as the market moves toward these types of early adoption.
- It has been good and steady progress.
- The proposed changes to the test method are well documented although the observed behavior is not completely understood.
- Additions to the materials database continue.
- The project is providing information in direct support of near-term market transformation applications.
- Although there has been an increase in data provided by SNL, the project still does not appear to be making much progress even with only a single lab focused on the work.
- Even though some of the OEM tank manufacturers have proprietary data and expertise on their designs, SNL must apply methods of material characterization to advanced systems and include them in the technical reference. If the budget cannot support extensive testing on these systems, more modeling of failure modes and safety margins for design consideration should be included until additional funding can be made available.
- Solving the test method issues will allow faster and less expensive evaluations of materials.
- In the absence of information on milestones related to the crack growth experiments, it is difficult to quantitatively assess progress. Procurement of tanks and introduction of artificial defects seems to have taken rather long.
- Good progress has been made.

<u>Question 4: Technology transfer/collaborations with industry, universities and other laboratories</u>

This project was rated **3.3** for technology transfer and collaboration.

- Direct interaction with the codes and standards committees, tank manufacturers, forklift integrators, as well as other DOE working groups ensures the relevant stakeholders receive data and have input into the direction of the project.
- Some international collaboration would be a good addition to the project, especially since the forklift market is growing everywhere.

- It was a good cross-section of SDO/industry/international partners.
- It was good to see collaboration with all important experts, both nationally and internationally.
- There was good collaboration. However, I would suggest involving NASA and their material scientists since the work has applications with their interests and they have developed additional database information on systems such as the National Association of Safety Professionals.
- Close participation between researchers in SDO working groups was very helpful.
- No evidence was given of interaction or collaboration with other materials testing institutes or establishments specializing in measuring fatigue crack growth and elucidating underlying physical mechanisms.
- There has been some collaboration, but more effort to seek out relevant partners should be made including forklift manufacturers and users to improve testing method and protocol.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- The proposed work is good in that it addresses all goals which were established at the beginning of the work. It may be outside the mandate of this project, but given the recent decision of China to ban Type IV vessels, some mention of testing these systems would have been welcome.
- It was a very good proposal for future work, especially collaboration with CSA regarding fuel system component material testing protocol.
- The incorporation of materials characterization data into national and international standards is important for the development of performance-based codes, standards and regulations.
- Per the comments above, more work should be focused on accelerated methods of testing and analysis to reduce the amount of time for system evaluations.
- The planned work dovetails into emerging standards.
- Clear planning seems to exist.

Strengths and weaknesses

Strengths
- They are working to develop quick and cost-effective testing methodology. The development of engineering-based and performance-based testing methodology is a strength.
- SNL has significant expertise in the required subject areas.
- They have proven to be an excellent technical team with outstanding laboratory capabilities.
- The researchers have the facilities and expertise to perform.
- This project supplies a direct feed into standardization.
- Although it was not presented, I found the relevance and usefulness of the materials reference guide very valuable.
- A strength would be their direct involvement in the standard setting process for maximum influence.

Weaknesses
- There is slow progress being made on material systems evaluation.
- It is limited by existing methods and new method development takes time and effort and can detract from the task at hand.
- Application of fatigue crack growth law to predict in-service behavior is based on the hypothesis of "leak before break". It is unclear how the foreseen testing program will allow one to determine whether and under which conditions this is the case.
- There is not a lot of industry collaboration shown.

Specific recommendations and additions or deletions to the work scope

- Two separate projects were presented consecutively but were supposed to be reviewed as a single project (this review is for the material compatibility work only). These projects were different enough and each funded at high-enough levels that they should have been reviewed separately.

- They need to spend more time on advanced material testing methods for accelerated studies and more development of models for predicting failure modes and their probabilities.
- It is more meaningful to have components develop leaks prior to failure by rupture from a safety standpoint.

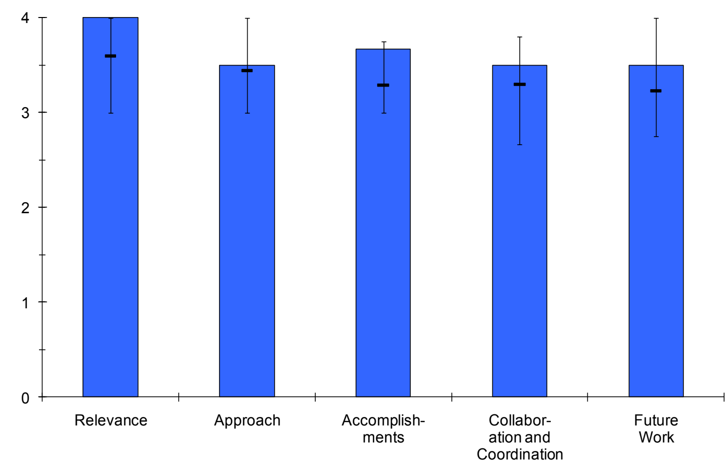
Project # SCS-06: Hydrogen Safety Knowledge Tools

Linda Fassbender; Pacific Northwest National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) capture the vast knowledge base of hydrogen experience and make it publicly available in a living document to provide guidance for ensuring safety in DOE hydrogen projects, while serving as a model for all hydrogen projects and applications; and 2) collect information and share lessons learned from hydrogen incidents and near-misses with a goal of preventing similar incidents from occurring in the future. Goals for this year are to: 1) update the Hydrogen Safety Best Practices Online Manual improving existing content and adding new content; 2) achieve a target of 200 records in the H2 Incident Reporting and Lessons Learned Database; and 3) analyze the lessons learned from incidents.

Overall Project Score: **3.7** (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The need for such tools is obvious. Most aspects of this initiative appear to be in-line with current emerging technology needs and critical information access.
- This is an excellent resource for all those involved in hydrogen. I have referred many fire professionals and project planners to these databases, as it is very important to have this type of resource.
- This is the only resource I know about that has this information. As such, it's essential to answer questions about the safety of the hydrogen industry.
- Best practices and incident reporting are important to furthering the goals of the hydrogen industry.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- It was not clear to me if non-disclosure agreement-type relationships were solicited to major hydrogen stakeholders and essential bulk or high volume users, except for NASA, in order to gain more information. Perhaps this might be an opportunity to establish "hold harmless"-type relationships that allow for a freer flow of incident reporting and sanitized details (in terms of liability issues and vulnerability).
- The project incorporates a straight-forward approach to creating best practices and cataloging incidents to answer questions about the safety of hydrogen. No changes suggested.
- The Hydrogen Safety Panel does a good job of corralling the hydrogen user community for relevant information.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.7** based on accomplishments.

- I am concerned with dissemination regarding access to "novice users" as potential training aids. Perhaps a focused campaign to the various SDOs/CDOs/non-governmental organizations stakeholders as well as national, independent and academic research facilities would help the process.
- It was a good population of events. Although new events are added, it is not to the extent that it conveys a message of 'not safe', which is an important nuance.
- I expected to see a report on the amount of new data entered into the database, but to see all the different features added shows significant progress and accomplishment. It's good to hear about some of these new features which I'd like to use in other projects we have.
- The expansion of the incident database is a good thing. Contributors need to understand that this is not a "hit list", but a learning tool. Progress has been made since the last review in this area.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- It might be advantageous to explore alternate avenues to information sharing and dissemination. This issue has previously been addressed and the team appears to be pursuing various paths. There is still a fair amount of progress needed.
- The communication between national labs and other organizations (NASA) seems to have expanded which is excellent.
- The project's coordination with organizations providing data is essential and obviously working well to collect the data needed for this project. If summary data/messages are developed, those could be shared with many additional groups.
- I would like to see more industry members (i.e. energy companies) on the safety panel.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- Pursuing indoor users, as well as bulk and sectional suppliers, is critical!
- Greater efforts should be focused in this area.
- Enhancing the web site utility may help to increase visits. This can be accomplished by making the site more 'friendly' and easy to navigate and also adding photos, graphics and videos. This will likely make it a more valuable resource.
- It was good to hear about future work on indoor forklift fueling. Addition of graphics and usability of the incidents data is also good. As a user, it would be nice to be able to pull out sound bites or summary points of the entire database, such as the number of hydrogen incidents represent 0.000X% of all hydrogen handling in a specific category. Data like this would allow one to compare hydrogen's safety record to that of other fuels. It would also provide more relevance to people who want to understand high-level data about hydrogen incidents, but don't want to get into a deep level of detail on individual projects.
- Continuing on with an increased emphasis on getting more energy companies involved is preferred.

Strengths and weaknesses

Strengths
- This may be an outstanding tool as sections "fill out" and collaborative efforts gain traction.
- This is an excellent resource and critical for those involved in the implementation of hydrogen and infrastructure as a 'go to' information source. The DOE and also PNNL are the most logical and appropriate organizations to do this.
- It is a unique and comprehensive collection of data, with a user friendly site, and the search features work well!
- The expert-based panels proved to be a strength.

Weaknesses
- Some weaknesses are the need for greater dissemination, a solution to "sanitizing protocols" and liability and identification vulnerability.

- The potential inclusion of compressed natural gas information (the focus needs to stay on hydrogen) and the addition of other alternative fuels expands the scope such that it may very well become unmanageable.
- Tough to compare such different incidents and pull out high-level summary data. This project does a great job of doing so, but it's also the area where improvement could be made.
- There needs to be more energy company involvement.

Specific recommendations and additions or deletions to the work scope

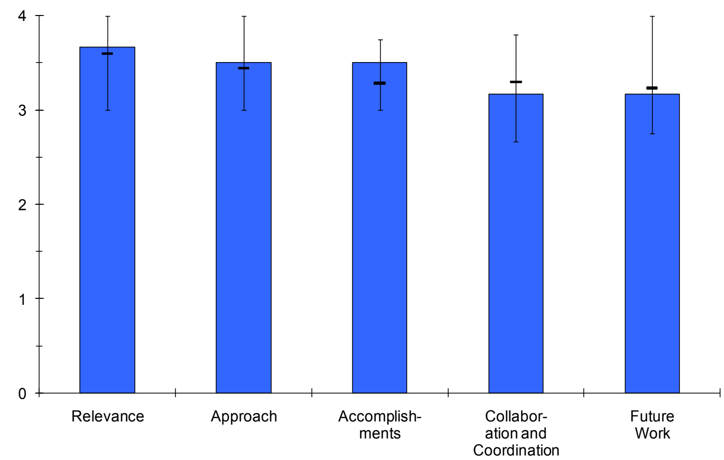
- Prioritize the establishment of workable agreements for the safe forward progress of the technology.
- Work with a graphics specialist on the overall layout to make it easier to navigate (improvements are good but it could use more).
- Add ability to generate reports of failure by component and cause (i.e. PRD, regulator, human error) to feedback about the SDOs on a bi-annual basis or coordinate with the meetings for revisions.
- See notes about pulling out high-level summary data on hydrogen incidents. For example, XX% of hydrogen incidents in the database had no resulting injury or XX% of incidents resulted in no loss of product.
- It would be good to know how much the website is being utilized (hits, visits, etc.) and whether the use is steady or growing (trends).

Project # SCS-07: Hydrogen Fuel Quality
Tommy Rockward; Los Alamos National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) help determine levels of constituents for the development of an international standard for hydrogen fuel quality; 2) test the critical constituents (NH3, CO and H2S); and 3) present data at the Working Group 12 meetings.

Overall Project Score: **3.5** (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- The purpose of this exercise is to generate data in support of an ANSI and an international hydrogen fuel standard for polymer electrolyte membrane fuel cell (PEMFC) vehicles. The ANSI document takes priority due to the immediate need to support the State of California Department of Weights and Measures as they need to comply with California statutes.
- The fuel quality standard is the lynch pin of the hydrogen economy. It will stipulate what non-hydrogen constituents the polymer electrolyte membrane (PEM) systems need to tolerate, and stipulate what non-hydrogen constituents the infrastructure is not to allow into the fuel.
- Setting and harmonizing standards for hydrogen fuel quality is extremely important to ensuring fuel cell life and durability.
- Since fuel cell technology is critical for the application of hydrogen as a vehicular fuel, understanding the relevance of hydrogen quality is of critical importance.
- This work tries to clarify levels and types of contaminants that could impact current PEM fuel cell technology. This foundation will be needed as the technology and membrane electrode assembly (MEA) materials change with continued development.
- This project is critical to the development of hydrogen fuel as a commerce item.
- The DOE objective to support and facilitate the completion of technical specifications by ISO for gaseous hydrogen is met. This is needed in order to complete the codes and standards needed for the early commercialization and market entry of hydrogen energy technologies by 2012.
- Hydrogen quality specifications are necessary to allow fuel cell developers to design their systems to cope with the expected level of impurities.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- The approach taken is valid. The data is currently limited to the capabilities of subscale testing. The results are based on quasi-steady state testing.
- There is more value in improving the detection limits for contaminants than in using data to set fuel quality standards that are more restrictive than previously set standards.
- It is clearly focused on the three major contaminants that affect fuel cell/MEA stability.
- The work resulted in an improved ISO fuel specification.
- Data indicates breadboard test modules are providing reproducible data that can be modeled for longer-term performance.
- There was limited feedback from OEMs.

- The technical barriers (i.e., conflicts between domestic and international standards and insufficient technical data to revise standards) have been addressed. Also, integration with other efforts through collaboration appears to be the case.
- The presentation shows that the work has been well organized over time and addresses the technical barriers provided by the industry panels to support the national and international standards development work.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- The data generated and more importantly the collaboration between investigators should be considered the model on how to approach tasks like this.
- Improving detection limits supports standards tied to system and fuel cell performance.
- The project had good experimental and modeling results that were adopted by the international community.
- Provided major emphasis to improve the detection limits of these impurities for improved durability.
- Excellent work that needs to be fed into the modeling group to check their models.
- Significant progress has been made on the establishment of cross contaminant effects, according to project plan.
- The presentation shows that the work has provided data that can be used to set limits for contaminants. However, physical upscaling to stack and system-level experience has not been done.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The collaboration on this project is fantastic. It is surprising that credit wasn't taken for the industry support and guidance on this task in regards to Air Products & Chemicals, Inc., Ballard, Chrysler, General Motors, Linde, Praxair, etc.
- Partners/collaborations were identified, but it was not clear what role each plays in the effort.
- There was excellent collaboration with universities, national labs and the international community.
- More active feedback from gas production and OEMs would move things along more swiftly.
- Collaborations with U.S. academia and national laboratories are clearly mentioned, but international collaborations are missing or not mentioned.
- There has been good collaboration with other labs and other stakeholders. Collaboration with fuel providers has been difficult. More collaboration with fuel providers could be an improvement.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The suggested future work is appropriate and should be seriously considered. Additionally, the collaboration effort should not be abandoned but rather expanded.
- Continuing research to develop the tools to assess the effects of reduced contaminant levels and harmonization of fuel quality standards will facilitate standards for hydrogen production and quality.
- There was not much provided on future directions except to communicate results and discuss them with regulators and manufacturers.
- Start/stop and some durability testing should be incorporated.
- The R&D is planned and performed systematically and conforms to common practices. Awareness of milestones and risks is evident.
- The future work seems to be well planned in order to complete the testing necessary to provide input to the hydrogen quality standards efforts, but system-level tests have not been planned and validation of model predictions have not been planned either. Depending entirely upon model predictions for stack performance under contaminant conditions may be a risky approach. Stack performance may be better or worse than predicted by the single-cell testing and the modeling.

Strengths and weaknesses

Strengths

- The strengths are the collaboration and the testing methodology.
- A strength was the excellent technical team and the R&D approach.
- There was good research and relevant data.
- There was systematic planning and execution of R&D.
- There appeared to be good coordination with other laboratories.
- A huge benefit was the good implementation of testing techniques to predict cell response to contaminants.
- There was a good work plan using an iterative approach to refine results.

Weaknesses

- The weaknesses are only those limits inherent to sub-scale testing. Full-scale testing to validate results might be appropriate.
- There were no weaknesses.
- They need to go further with durability testing at the cell level.
- There was information missing or not mentioned concerning international collaborations with actors performing similar R&D elsewhere.
- The presentation did not show a high degree of collaboration with fuel providers. Additional collaboration with fuel providers might help to get their buy-in and get their input to the program.
- During the presentation, it was mentioned that some vehicle manufacturers have implemented shutdown sequences that might mitigate degradation. It would have been more expedient to have this input earlier. Additional collaboration might have been helpful and perhaps that information could have obtained earlier.

Specific recommendations and additions or deletions to the work scope

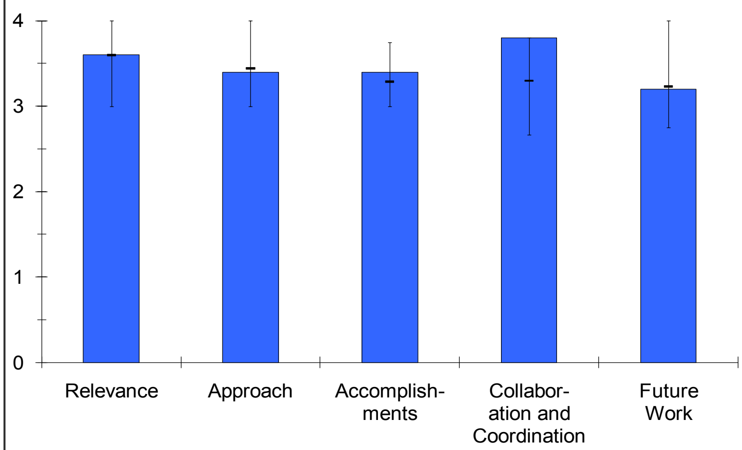
- This activity should be continued.
- Perform similar testing but at lower platinum levels.
- They should incorporate durability testing at the cell level.
- Additional collaboration with fuel providers and vehicle fuel cell stack developers could be an improvement.
- Stack-level testing could be added as an enhancement. System-level validation could also be added as an enhancement.

Project # SCS-08: Hydrogen Safety Panel
Steven Weiner; Pacific Northwest National Laboratory

Brief Summary of Project

The objectives of this project are to: 1) provide expertise and guidance to DOE and assist with identifying safety-related technical data gaps, best practices and lessons learned; and 2) help DOE integrate safety planning into funded projects to ensure that all projects address and incorporate hydrogen and related safety practices.

Overall Project Score: **3.5** (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The overall importance of a cast of "go-to" practitioners (not simply academic or theoretical experts) cannot be overstated. This type of group is critical to the responsible forward progress of this emerging technology.
- This fits very well into the overall scope of Pacific Northwest National Laboratory's (PNNL) responsibilities for hydrogen safety and education.
- The Hydrogen Safety Panel is obviously necessary to provide oversight to all DOE-funded hydrogen projects. The panel of experts is fairly large and diverse, which is beneficial, as they each bring unique and different areas of expertise to the table to provide a comprehensive assessment of safety reviews.
- The Hydrogen Safety Panel is a critical component of the overall DOE safety, codes and standards sub-program. The Panel ensures that the standard operating procedures of DOE-funded projects conform to the best practices in safety. Over the past six years, the practices, procedures and priorities of the Panel have evolved to become a more integral part of the sub-program. The Panel provides a unique venue where critical safety issues, from assessing recent safety incidents to identifying impending needs, can be addressed by an expert group. The Panel has made important contributions to the sub-program.
- The project is relevant to the DOE RD&D objectives in that it identifies project leaders where hydrogen safety must remain a priority.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- It appears that many feel a more formalized entity (that also is utilized to a greater investigative and perhaps "oversight" capacity) would benefit this highly compartmentalized industry.
- The approach offers the ability to tap into the resources of the Panel (for example NASA).
- The approach to safety assessment is continuous, iterative and appears to be more rigorous each year with improved data collection and dissemination strategies.
- The Panel has kept its focus on safety, and its work continues to address a critical barrier to deployment of hydrogen and fuel cell technologies. The work of the Panel is better integrated with other efforts, particularly with work on the incidents database and best practices manual, as recommended in the AMR last year. There is still room for improving this integration. The presentation showed how the Panel is well integrated with PNNL's Hydrogen Safety and Education Program, but did not adequately address how it is integrated with the sub-program. This integration should be better addressed in the 2011 AMR.
- The Project makes good use of notable experts in the field. The project seems to be doing a lot of work, but it's confusing to me why the Panel is needed at almost $1 million per year. Is there any evidence that the safety of

projects is so unpredictable and risk of incident so high that this extra check is required? If so, that should be better articulated. If not, what is the best use for this great group of experts? This role of the Panel was not clear to me.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- More investigative work as well as closer oversight and better established and organized feedback is needed. It is particularly valuable to see the group focusing on defined and specific application projects (like the use of indoor specialty vehicle fueling situations).
- There was some concern with the white paper on supplemental safety for 70 MPa fueling—this is a sensitive subject with the auto manufacturers.
- The Project has very good communications and outreach. The only reason I do not say outstanding is that I think that would imply that all bases are covered and the job is done. The accomplishments to date, including many recommendations and reviews coupled with very few incidents to investigate, indicate that the Panel is successful in achieving its goals.
- The preliminary metrics presented at the AMR to measure the accomplishments and progress of the Panel is a good start. The Panel should address these metrics more carefully and develop a set of metrics that it will use for self-assessment. A Panel self-assessment of its accomplishments and effectiveness in meeting the terms of its charter should be reported at the 2011 AMR.
- The statistics were very helpful, as were the examples of specific projects with which the Panel was engaged.
- Related to the comments in the approach, besides reports and suggestions that were given to DOE and project managers (which does have a value), I wonder what beyond that was really accomplished in terms of reducing risk or improving the execution of projects for the betterment of the industry.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- Collaboration appears to be moving in the right direction with the right stakeholders. It was not clear to me what off-shore agents or organizations were being utilized to their optimum capacities, so I see opportunity for further cooperation and inclusion in joint activities.
- The panel itself has a good representation and they engage others well outside the panel.
- Well-coordinated interaction with project teams is the point of the effort.
- Collaboration with other institutions is inherent whenever the Panel conducts a review of standard operating procedures for safety at research facilities and industrial organizations. The Panel has done an outstanding job of reviewing such procedures and following up on its reviews. The Panel approaches safety reviews as a collaborative effort with the institution being reviewed and is to be commended for this. The Panel's reviews are appreciated by the institutions being reviewed, as an independent third party review by safety experts is invaluable for the safe operation of research and development facilities.
- The Panel structure itself is collaborative with different organizations. Given the difficulty in getting safety information from companies, the collaboration seems to be quite good.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The target summary slide was generic, but we assume it includes all of the applicable fiscal year DOE projects.
- The work with the American Recovery and Reinvestment Act of 2009 fuel cell deployment projects is in line with the purpose and goals of the Panel. Conducting safety plan analysis for six fuel cell forklift installations and two backup power installations will instill confidence within the industry as a whole (reducing incidents and risk).
- The plans build on past progress.

- Meetings are generally planned one at a time and held twice a year—an approach that encompasses an annual (or even broader) perspective based on key safety concerns that need immediate attention. Safety as an enabler of hydrogen and fuel cell technologies should also be considered.
- I have the utmost respect for the people involved and their expertise, but question what is really needed by the industry from this panel. If there is a real need, there's no doubt that the Panel could accomplish it, but maybe that needs to be better identified and the purpose of the Panel re-examined to determine maximum effectiveness from dollars spent.

Strengths and weaknesses

Strengths
- There were varied backgrounds of experienced, seasoned and professional practitioners.
- Recommendations based on their review of incidents and the publication of the safety documents is excellent resources.
- The Panel is made up of a diverse group of highly qualified experts with excellent administrative management provided by PNNL. The Panel has conducted a number of site visits with follow-up and published relevant guidance documents. Its meeting provides an excellent forum to address both specific safety incidents and general safety issues.
- There was a very impressive collection of experts who are widely respected for their expertise in the area of safety.

Weaknesses
- The project should strongly consider "formalizing" this group in a more "established" organizational model, as well as allowing more capacity for greater access and oversight.
- It is very difficult to develop and implement metrics of effectiveness for an activity such as that undertaken by the Panel. The Panel has attempted to do this by accounting for the number of safety plans reviewed, site visits, etc., but more should be done to evaluate effectiveness if possible.
- There is a lack of clarity on the need for this panel. It's not completely clear to me why this panel is essential, and especially at the price tag stated (which I realize doesn't include the in-kind contributions). For example, when the Benning Road hydrogen station was put in, the companies involved were so concerned about safety a panel like this wasn't needed, in my opinion. That amount of oversight from many companies seems common. But are other companies much less reliable? And therefore is the Panel needed to help protect DOE and to reduce the risk of incidents with DOE projects, or projects where companies don't have the resources to conduct adequate safety reviews? The main need being addressed could have been clearer.

Specific recommendations and additions or deletions to the work scope

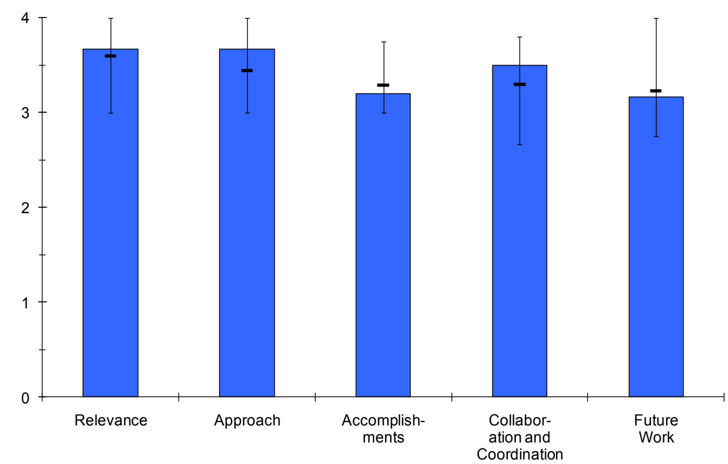
- Other unique fueling scenarios should be visited. Similar approaches that were addressed concerning specialty vehicles have value. Portable and remote applications may be candidates, particularly those within the aerospace and aerospace support arenas.
- Perhaps an annual report on the status of hydrogen safety in the United States, key lessons learned during the year and critical issues to be addressed in the upcoming year would be helpful additions to the Panel's activities. Such an evaluation could be an agenda item during one Panel meeting. What has been the key value-added of the Panel's efforts during the current fiscal year?
- I feel uncomfortable making these somewhat harsh suggestions about a project run by people whose work I really respect, however, I think it would be worth looking into whether the purpose and function of the Panel needs to be reinvented. It's clear that this group could accomplish much, but I'm not sure that the current purpose is meeting an essential need. Maybe I'm missing something and if so, those missing elements or statements of critical need really need to be stated more clearly.

Project # SCS-10: Hydrogen Release Behavior
Daniel Dedrick; Sandia National Laboratories

Brief Summary of Project

Hydrogen codes and standards need a defensible and traceable basis. Objectives for this project are to: 1) use quantitative risk assessment for risk-informed decision making and identification of risk mitigation strategies; 2) perform physical and numerical experiments to quantify fluid mechanics, combustion, heat transfer and cloud dispersion behavior; 3) develop validated engineering models and computational fluid dynamics models for consequence analysis; and 4) provide advocacy and technical support for the codes and standards change process.

Overall Project Score: **3.4** (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This project is focused on tunnel and indoor fueling accidents. The need for this research is self-evident.
- This project provides valuable real world data which feeds into the development of codes and standards for hydrogen. As such it aligns well with the RD&D goals of the Fuel Cell Technologies Program.
- The science-based defensible and traceable development of codes and standards are key to overall DOE objectives.
- Providing engineering data for development of hydrogen codes and standards is critical to establish appropriate requirements for hydrogen technology deployments.
- The project is providing excellent support to both domestic and international standards with the separation distance work.
- The project provides vital information for the development of DOE R&D efforts in the hydrogen program.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The approach on the tunnel testing is appropriate. It isn't as clear what the game plan is for the indoor fueling question. Are the warehouse fire safety facilities of Underwriters Laboratories (UL) in Illinois or Factory Mutual in Massachusetts being considered as test sites?
- The technical barriers addressed in this project are well planned and well executed. The approach taken, using models and then validating those models with real world experiments, is the way that hydrogen safety R&D should be done (as opposed to just doing the modeling compartment).
- The effort uses a well thought-out approach using a combination of modeling and physical validation activities to characterize hydrogen release behavior.
- The scenarios investigated are relevant to industry. The separation distance studies are critical to infrastructure development.
- Caution must be used in relying too much on risk-based decisions using probability alone without sufficient attention paid to severe consequences. Some effort should be spent on mitigation of low-probability high-consequence events, such as 300-year floods. Recent events such as Hurricane Katrina and the British Petroleum oil spill have led regulators and others to rethink the approach to manage risk and accidents. It would

not have prevented the accident from happening, but at least there may have been efforts devoted to a "Plan B" in case such events happen.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The accomplishments on the tunnel issues are impressive. The progress on the indoor fueling is less clear.
- The spontaneous ignition results are interesting.
- The tunnel and semi-enclosed spaces work has resulted in significant advances. Relating the experimental work to actual risk statistics, to show that the frequency is less than that for everyday life, was a very good way of putting the issue into perspective.
- The work on "spontaneous ignition" has made significant progress as well. This is a bit of a controversial area in the hydrogen safety world. The results of this work, which highlight the effects of entrained particles, are therefore a welcomed advancement.
- The results appear to have been slow but steady.
- Overall, the progress of the work and the dissemination of results to the codes and standards community are excellent. The criticality of material entrainment and its influence on release ignition needs to be clarified. Indoor refueling characterizations need to be accelerated as early market deployments are well underway.
- The tunnel work is important. The results need to be distributed to the code official community.
- A model was completed and some testing was done.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- The collaboration for the tunnel issues appears to be appropriate. It is suggested that UL and Factory Mutual be approached on the indoor fueling issues for several reasons: test facilities, test methods, indoor modeling and acceptance of results by AHJs and underwriters.
- There is reasonable collaboration of this project both within the United States and also internationally through the International Energy Agency Hydrogen Implementing Agreement (IEA HIA).
- It was a good and collaborative effort involving SDOs and international agencies.
- Collaboration is largely relative to dissemination, with the most important aspect being dissemination to the codes and standards community.
- Collaboration with code organizations is critical to success.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The proposed future work is appropriate.
- The proposed future work is well in line with the milestones that were originally set out in the project. The inclusion of indoor refueling into the project is a very good direction.
- The low-temperature work is similar to work being carried out at the Health and Safety Laboratory (HSL) in the United Kingdom and it would have been good to see some mention of possible interaction with those researchers (data sharing, etc).
- It could be clearer how the work scope is evaluated, but overall future work identified is appropriately structured and important to the development of hydrogen safety, codes and standards.
- Indoor fueling work will be critical in 2011.

Strengths and weaknesses

Strengths
- The strengths of this project are self-evident.
- There is an excellent translation of scientific study and analysis to concrete safety guidance.

- The expertise of the panel was a strength.
- The project provided some good data and a model for use.

Weaknesses
- The lack of accepted industry collaboration mentioned for the indoor fueling question is a weakness. Support and concurrence from UL and/or Factory Mutual will help with AHJ and underwriter acceptance of results and conclusions.
- It could use some additional clarity as to the direction of the tunnel release work.
- Time consuming studies are a weakness.

Specific recommendations and additions or deletions to the work scope

- The PIs should discuss the indoor fueling activity with the building fire experts at either UL and/or Factory Mutual. This should facilitate the acceptance of the testing results (instrumentation techniques) and may improve the applicability and fidelity. This is their bread and butter.
- It is not clear how significant an issue particle entrainment is.

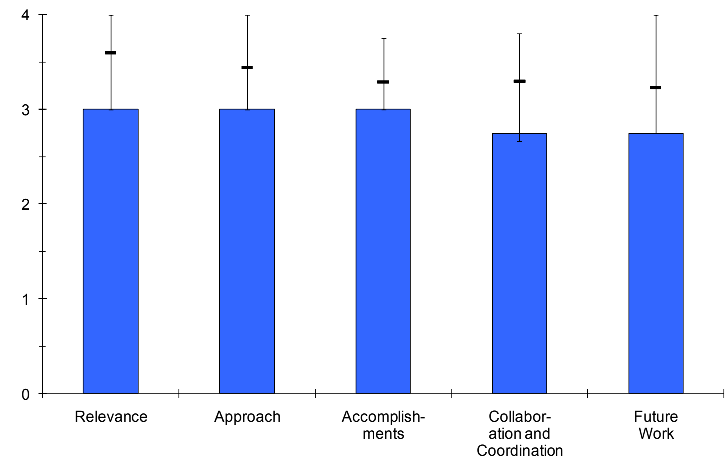
Project # SCS-13: International Energy Agency-Hydrogen Implementing Agreement: Task 19 Hydrogen Safety

William Hoagland; Pacific Northwest National Laboratory

Brief Summary of Project

The goal of this project is to conduct a collaborative program to develop predictive methods, data and other information that will facilitate the accelerated adoption of hydrogen systems. Specific objectives are to: 1) characterize and assess risks and hazards and quantitative risk assessment methodologies, including risk informed criteria for permitting approval and simplified methods; 2) conduct collaborative testing program to validate the models that have been developed and to further refine those tools for use in real-life scenarios; and 3) document and convey results and data to reduce the barriers that inhibit commercial introduction of hydrogen systems.

Overall Project Score: **3.0** (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- It appears that the collaborative efforts of the program are worthy as are most collaborative efforts and instruments, but it appears a bit vague as to the specific what, why and how.
- The mission to accelerate hydrogen implementation and widespread utilization (overall) is exactly in line with the DOE Fuel Cell Technologies Program on safety, codes and standards and that of the industry.
- The project is aligned with DOE goals.
- International collaboration is important to allow U.S. manufacturers to sell into international markets. International collaboration is also important to allow U.S. citizens to gain the advantages of technologies developed elsewhere.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Again, I have had some difficulty aligning what I am aware of as critical "needs" with regard to international coordination, cooperation and voice (particularly specific to the International Organization for Standardization (ISO), International Electrotechnical Commission (IEC) and Global Technical Regulation (GTR) and this program's specifics.
- Providing input to the risk-informed codes & standards development with input from experts around the world helps to ensure that the results are also accepted on an international basis. This is so very critical to hydrogen technology adoption.
- Analyzing how countries interpret risk and what they accept as such demonstrates international leadership.
- International experts share information and data. It was a good plan to disseminate results at the end of the task.
- It appears that the "risk-informed" methodology mirrors and parallels the SNL/DOE approach to risk assessment for hydrogen and fuel cell systems.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- It appears that meeting support or expertise identification and access, which can at times be daunting, was the prime deliverable for this reporting period.
- The importance of the projects and accomplishments of the Task 19 Panel (vetted database, position papers that are a consensus of experts on major issues and a database of suggested models validated against the unintended release database) are indescribable.
- Fundamental data, modeling, component testing and mitigation resulted in engineering models, dispersion models for releases and a more detailed thermal radiation model that accounts for cross winds. All of this personally created a higher confidence level in these areas. Plus it's on an international scale!
- It was a logical subtask structure covering technical and communication activities.
- A bibliography has been developed and modeling has been done.
- A database has been developed.
- Hydrogen incidents have been provided to www.hydrogenincidents.org. This is a strength.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Perhaps a comprehensive listing of collaborative entities, institutions and organizations would better facilitate the understanding of this project's deliverables.
- This is an international effort which includes 11 countries participating, assuming that all are actively providing input, and is an excellent collaboration.
- There was excellent international participation and collaboration.
- If this work is international, it needs to feed into ISO, IEC, and IEC System for Certification to Standards Relating to Equipment for use in Explosive Atmospheres (IECEx), and other standards systems, as well as the European Norms (ENs) that are typically derived from the IEC and ISO standards in order to meet the European directives. It is not clear that this vital link will be made. In addition, if Japan is to be involved, the Japanese industrial standards (JIS) need to be linked to the program as well as the Japanese laws.
- Collaboration appears to be in place between the countries, but the collaborative goal needs to be better defined in order to support the goals of removing barriers to codes and standards.
- Collaboration with the U.S. labs seems to be in place.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- It ends in October 2010 and will have an end-of-task forum/workshop to disseminate results (which will be kept in a database that is closed for now but will be made public).
- The work will be completed in December 2010. The communication of final results of the activities will be critical.
- More information sharing appears to be the most robust function. Additional linkages to the actual standards, norms and directives would be helpful.

Strengths and weaknesses

Strengths

- The potential to link off-shore efforts and support to a unified task in harmonization is a strength.
- Incredible collaboration and data that delivers results is due to the level of experts involved.
- The strong international collaborative effort is a strength.
- Good collaboration between the countries is a strength.
- There appears to be good collaboration between the U.S. labs and the project.

- Input to the www.hydrogenincidents.org project is a strength.

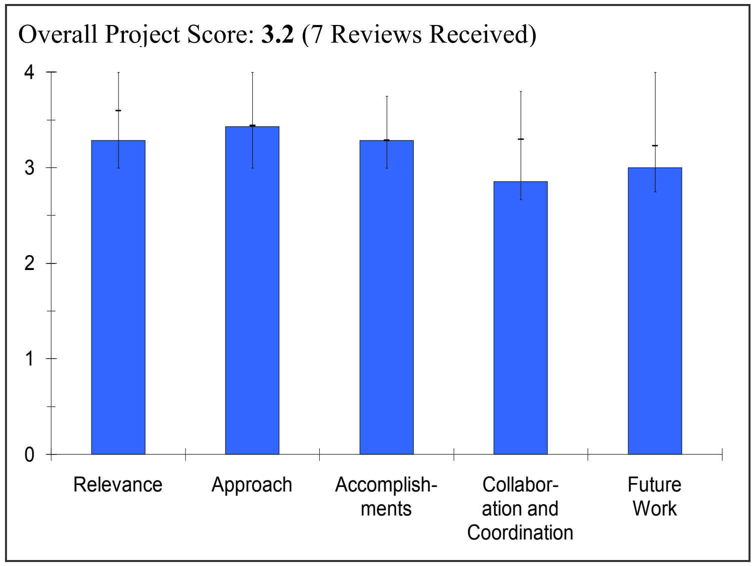
Weaknesses
- It appears to be vague and essential elements were not identified.
- The project is ending due to lack of funding.
- There was a lack of well-defined linkage to ISO, IEC, IECEx, ENs, European directives, Japanese laws, JIS standards and Consumer Electronics (CE) certification.

Specific recommendations and additions or deletions to the work scope

- I recommend that specific targeted collaborative partners are identified and relationships are somewhat formalized (i.e., through Memorandums of Understanding).
- Increase the access to the Hydrogen Technical Experimental Database (not the videos), so that the data can be incorporated into other safety-related activities supported by the DOE program.
- It would be valuable to establish a solid linkage to the IEC, ISO, IECEx, European Norms, Japanese laws, European directives, JIS standards and CE certification.

Project # SCS-14: Safe Detector System for Hydrogen Leaks
Robert Lieberman; Intelligent Optical Systems (IOS), Inc.

Brief Summary of Project



The project goal is to select and finalize hydrogen sensor technology, design and fabricate scalable prototype sensors and investigate and establish end-user market size and cost analysis. The overall objectives are to: 1) integrate IOS's proprietary hydrogen indicator chemistry into a complete optoelectronics package with well-defined sensing characteristics and a known end-use market; and 2) identify different formulations and physical embodiments to meet specific market requirements. Technical objectives for 2009-2010 are to: 1) select and finalize hydrogen sensor chemistry that possess the optimum sensitivity, reliability, reproducibility and aging performance; 2) finalize and fabricate optoelectronic board for hydrogen leak sensor; 3) assemble packaged prototype hydrogen point sensor that meets DOE specifications; and 4) test and validate the full packaged prototype performance at the NREL testing laboratories.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The idea to put hydrogen sensors in residential garages is not in-line with the industry. There is no precedent, for example, no natural gas sensors for hot water heaters. The National Fire Protection Act does call for sensors in repair facilities, which this could very well fit into, but I would like to see them move away from the residential garage/home sensor. It sends a negative message to the public. Perhaps the panel needs to read up on residential garage modeling studies?
- In regard to the mentioned vehicles, vehicles do have sensors (now) and all of the OEMs have their vehicle sensors 'worked out' (either they make them themselves or have a supplier they are satisfied with make them).
- The development and deployment of safety sensors that meet DOE targets is an important part of the Fuel Cell Technologies Program. The relationship of performance targets (sensor product specifications) to the DOE targets should be discussed.
- Cost-effective hydrogen sensor technology is an enabler for more robust hydrogen safety practices.
- Flexible sensor technology is important for hydrogen safety. I'm not sure how this technology itself applies to overall programmatic goals, but it's good to see DOE helping technologies like these. These technologies are important to reach commercialization.
- Quality sensors are critical for the widespread use of hydrogen.
- Hydrogen sensors are essential for the safe use of hydrogen. As such, this project provides critical support to the safe deployment of hydrogen (from production to end use) by designing a sensor to meet the requirements of the application.
- Leak sensors appear to be a necessary component of vehicle, stationary and portable fuel cell systems, at least in most applications. Some developers are unwilling to include standards due to concerns regarding reliability, false alarms, stability and cost. Having reliable and cost-effective sensors is essential.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- The project is investigating various sensing platforms that could lead to a new technology, such as fiber optics for very small sensors (working with them now).
- The approach as shown in slides 6-11 shows a number of things (e.g., different application platform requirements, project plan, testing station, etc.), but does not add up to a coherent statement of technical approach.
- The project addresses technical barriers adequately.
- The approach incorporates an appropriate balance of technical, manufacturing and packaging R&D.
- The approach is systematic and well-developed. The technology development manager's suggestion to push towards a project in this area last year was good advice to make sure that a product, and hopefully testing, could result from funding received by this project.
- This was very focused work. This is a good example of a project run by industry and not academia.
- In general, it is clear how this project addresses the barriers identified with delivery, manufacturing and technology validation. The relevance to storage balance-of-plant components is less clear.
- The cost and durability of the optical sensor are not mentioned even though these two factors are explicitly mentioned in the relevant DOE Multi-Year Program Plan list of barriers.
- This project is using the goal targets from the 2007 workshop. This is a good approach, provided that industry was involved.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- A ten-hour battery life would be an accomplishment.
- The presentation states that Tasks 1-12 have been 100% accomplished (slide 7), but did not report on performance of a hydrogen sensor market study (Task 7). The requirements of targeted markets and an assessment of how objectives and accomplishments of the project align with these requirements would be helpful. More explanation of the cross-contamination testing (slides 17 and 18) and NREL's test results would also be helpful in determining the status of Milestone 1: "Complete cross contamination testing of sensor elements". According to the project plan (slide 8), the activity to "Establish commercial market and partnerships" started at the end of year one and should be about 90% complete. However, there was not enough information presented about progress on this activity.
- The nearly completed project has met most technical targets although it was limited to only 10% hydrogen concentrations. Issues with temperature and humidity are still being investigated but do not seem insurmountable. CO and H2S contamination issues still need to be resolved.
- The presenter was very clear about what has been accomplished and what remains to be done. It appears that of the challenges remaining, they all will be overcome. The cost and size reduction opportunities from potential volume manufacturing are encouraging.
- The segue from performance to goals is excellent. The end of the project will produce a usable device.
- Good progress has been made with respect to the project objectives. The sensing material has been successfully integrated with an optoelectronic interface and into a prototype unit that has been tested by collaborators. Further developments (e.g. polymer) will be necessary to extend the operating specifications to meet the targets. While problems of humidity and temperature influence on sensor performance have been addressed, this is only obvious for a very limited range. The sensor does not appear to be resistant to CO. In fact, the response appears to be permanently changed (poisoned) by CO.
- The presentation showed good progress towards a commercial sensor.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.9** for technology transfer and collaboration.

- Collaboration does not seem to apply in this project (private company with a product they want to bring to market).
- The collaboration with NREL is commendable, but it is not clear what role NREL's sensor laboratory played in the testing to date of the prototypes. My understanding is that the project benefitted from leveraging and

extending RD&D conducted under other federal programs. If so this should be noted, as it shows how DOE funding has built upon previous work funded by another federal agency. The extension and substance of collaboration with Intelligent Energy and Jadoo Power as "commercialization partner(s)" should be explained.
- The project has some experienced partners, but more clarity could have been provided regarding their contributions.
- Coordination is good. The PI seems to have coordinated with the appropriate kinds of groups, including a customer.
- It is a small group that is easily managed and has short communications lines.
- The collaboration with NREL as a testing and validation provider is visible and such independent testing by a national laboratory is very important and commended.
- The collaboration with and input from other named collaborators is less visible. The degree of coordination is also unclear.
- There is good collaboration with NREL and potential customers.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- They have essentially completed the program (doing marketing projects), and the next step is potentially a multi-site field test.
- As the project is 95% complete (slide 2), there was no explicit discussion of future work. The technical objectives to "assemble packaged prototype point sensor that meets DOE specifications" and to "test and validate the full packaged prototype performance at NREL testing laboratories" (slide 3) seem to be the most important work that remains to be conducted in FY 09 –10 and should be discussed in more detail.
- Project is effectively complete so this rating is effectively the average of the other category ratings.
- Plans to do testing of the product and to address the challenges remaining with regard to contaminants were clear and seem to be what's needed next in the development of this technology.
- Commercialization of this device will be good for infrastructure development, especially indoor refueling.
- The project scheduling is rather unclear (timeline 2007-2011 and project plan 2008-2011), so it is difficult to comment on. Future developments should lead to operating specifications that meet the targets given (slide 5) in terms of measuring range, temperature range and cross-sensitivity. It is not clear if and how this is planned. It is also unclear whether a prototype wide-area hydrogen sensor has actually been made or if only the point sensor has been developed.
- Continued efforts to perfect the technology are planned. The final outcome is not known at this time.

Strengths and weaknesses

Strengths
- It is focused R&D and engineering on applying fiber optic technology to address hydrogen safety sensor needs and targets. The sensor technology developed by the project has the potential to address safety sensor needs in a variety of applications and markets.
- A well executed project that is very close to delivering a commercial sensor.
- The product seems to have developed systematically and successfully through the R&D process. The end result so far works well, although some challenges remain.
- This is critical technology; an optical sensor with the potential to be intrinsically safe with wide-area monitoring is a strength.
- The development from basic material to sensor prototype is commended, and the independent testing of the unit (with NREL) is also commended.
- This is innovative technology.
- There was good collaboration with potential customers.
- There is good collaboration with NREL.

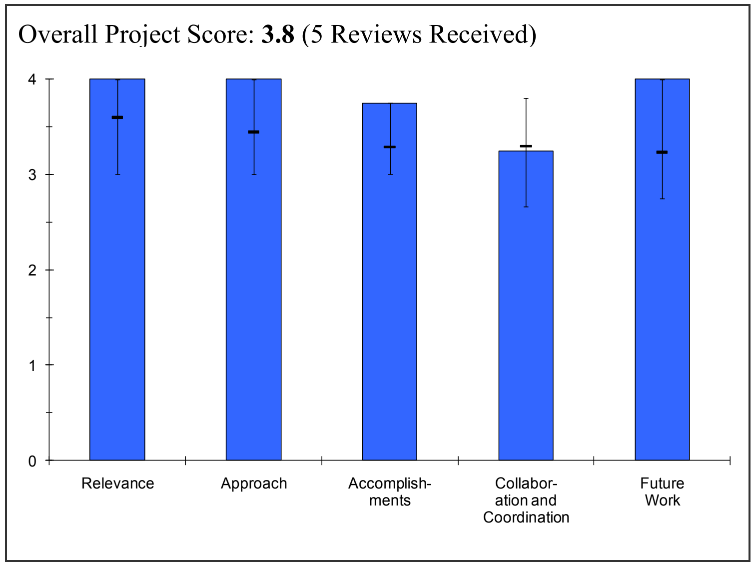
Weaknesses

- While the technology appears to be unique and advanced, the push to get these installed in residential garages is not the message that should be propagated in getting hydrogen vehicles to the general public as it presents a negative and fear-based connotation towards hydrogen. The question may be raised, "Why would I buy a hydrogen car if it's dangerous enough that I have to put a special sensor in my garage?"
- A key project goal to "investigate and establish end-user market size and cost analysis" was not addressed.
- The presentation was weak on information regarding how this product might be used and coordination with additional potential partners. Since vehicles and garages are listed as potential users, is there any evidence that the automakers would be interested in such a product? How about people who construct commercial or residential garages? I'm not sure the customers identified would be the most likely to be interested in the product and, if the customer is different, would the product need to be developed or packaged differently?
- I see no weaknesses at this time.
- Their cost targets are a weakness. It is unclear how competitive they will be when compared with commercially available sensors.
- Methods to reduce cost should be addressed.
- Poisoning by carbon monoxide is an issue.
- Cross interference has not been solved yet.
- The sensors will not work in 100% relative humidity (condensing atmospheres). This could make them unsuitable for some scenarios.

Specific recommendations and additions or deletions to the work scope

- See weaknesses listed above.
- I suggest looking more into how this product might be used. Since vehicles and garages are listed as potential users, is there any evidence that the automakers would be interested in such a product? How about people who construct commercial or residential garages? I'm not sure the customers identified would be the most likely to be interested in the product. If the customer is different, would the product need to be developed or packaged differently?
- The project needs the identification of a niche market for the sensor and a favorable comparison with available commercial sensors.
- Hold a demonstration of their distributed sensor as a potential improvement on current technologies.
- Sensor durability and lifetime needs to be demonstrated.
- Additional collaboration with laboratories and other manufacturers could be beneficial.

Project # SCS-15: Hydrogen Safety Training for First Responders
Linda Fassbender; Pacific Northwest National Laboratory



Brief Summary of Project

The long-term goal of this project is to support the successful implementation of hydrogen and fuel cell technologies by providing technically accurate hydrogen safety and emergency response information to first responders; including fire, law enforcement and emergency medical personnel. The objectives for FY 10 are to: 1) continue to provide a one-day first responder training course utilizing DOE's fuel cell vehicle (FCV) prop at the Hazardous Materials Management and Emergency Response (HAMMER) facility; 2) offer the FCV prop course at training centers in California for approximately 300 first responders; and 3) continue to support the web-based awareness-level course; and 4) disseminate first responder hydrogen safety educational materials at appropriate conferences to raise awareness.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- This was highly relevant, timely and essential!
- Providing safety training to first responders is critical to advance the safe deployment of the hydrogen economy.
- It is critical to move emerging knowledge generated by the scientific and codes and standards communities to those responsible for responding to potential hazards in real-world situations.
- Hands-on training is an essential part of building hydrogen awareness and preparing fire service personnel for greater commercialization of hydrogen technologies.

Question 2: Approach to performing the research and development

This project was rated **4.0** on its approach.

- Comprehensive efforts to provide this activity should be applauded!
- The approach to reaching the target audience is outstanding. Online training materials are designed to be engaging and provide training to the entire emergency medical service community. Outreach through conferences and hands-on training with the prop are other ways to engage people and let them really experience some of the unique properties of hydrogen and associated hazards.
- Using the TrainingFinder Realtime Affiliate Integrated Network (TRAIN) Website is also a great way to get the materials out there to the target audience. I was glad to hear about the TRAIN link.
- The multifaceted approach incorporating outreach, self-directed training and hands-on simulated training is designed to reach a wide audience.
- Use of the prop and procedure to teach fire personnel are appropriate, and from the quotes you can tell that it's just what they need.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.8** based on accomplishments.

- The project is well on the way to providing a much needed service for "initial training" as well as "recurring training" where the technology is in service.
- It is not clear how one measures success in this sort of effort (e.g., number/percentage/by region of responders or trainers trained) but given the feedback received by attendees, the program is having a positive impact.
- From the responses of the students, it seems that this project is accomplishing everything it needs to accomplish. It's good to see steps being taken to get the prop to many more people. The instruction seems to be working well, so steps should be taken to get the course to more students and locations.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- To the novice and outsider this program is still "hard to find", but is getting "louder" on the radar screen. Perhaps renewed efforts to identify other stakeholders would be helpful. Groups like the National Training Institute (NTI, formally the National Training Institute of the Electrical Industry), the National Joint Apprenticeship Training Committee (NJATC), as well as individual regional "union shops and houses" (fire and other emergency response teams), would be good collaborative options.
- Their collaboration and cooperation with other institutions is very good. There is probably even more feedback from other portions of the PNNL programs (incidents database, safety panel, etc.,) that are not listed here since they are providing an integrated approach to safety and education.
- The partners are very appropriate, but some additional information regarding their contributions would be helpful.
- There was good collaboration with organizations like the California Fuel Cell Partnership (CaFCP) to assist with training and development of modules and with fire service personnel for course content.

Question 5: Approach to and relevance of proposed future research

This project was rated **4.0** for proposed future work.

- Full support must be given to this outstanding effort. The identification of training sessions should start to focus on a greater emphasis of "recurring training" on an ongoing basis.
- The project approach and relevance are both outstanding. Enhancing the online training, taking the prop-based course on the road and continuing collaborations through CaFCP are all good.
- Off-site training opportunities and extending channels through Department of Transportation (DOT) and the International Association of Fire Chiefs will provide wider training opportunities. Forklift training variants will be useful as well.
- It is excellent to see plans that take the prop and course to other sites and also visits to other cities.

Strengths and weaknesses

Strengths
- The project focuses real-time training for essential parties.
- This was a well-designed program showing very good results.
- Using a prop to give hands-on training and the ability to take it to other locations is clearly the strongest part of this project. The second most important part would be the awareness-level online course that is essential for the folks who can't come to experience the prop in-person.

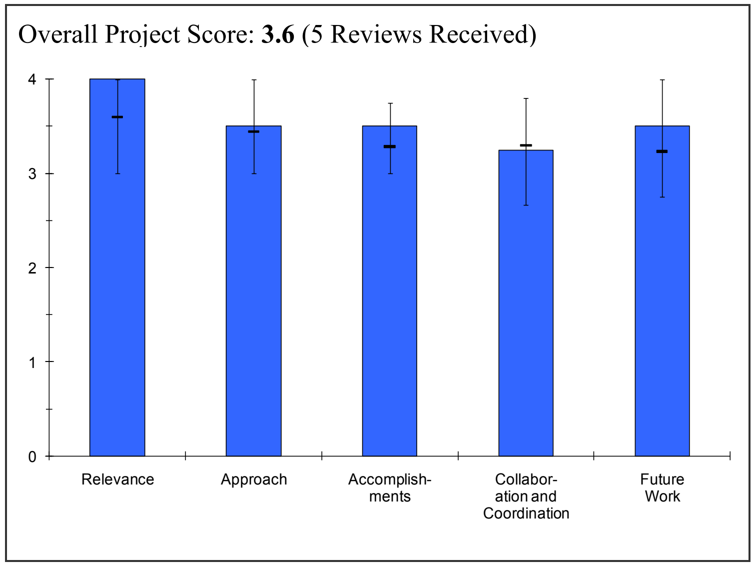
Weaknesses
- Perhaps a site on the east coast will soon be in order. Cooperation and collaboration with the Department of Defense (DOD) would help.
- A forklift training variant should be given priority over other options (stationary power, portable power and auxiliary power).
- The project needs to get the prop to more people and more locations.

Specific recommendations and additions or deletions to the work scope

- They need to develop a "full up" simulator as well as a real-life training device that is the next generation of this already outstanding tool. They also should start to integrate the growing high-voltage drive train electrical intricacies, as well as perhaps the numerous electronic control devices for unintended active restraint devices. This is the real deal! Bravo so far!
- This was already suggested, but I cannot emphasize enough developing a cooperative and even perhaps collaborative relationship with prime stakeholders from the various service branches within the DOD!
- Non-vehicular responses should be integrated with personnel certification while providing feedback to the regulators. Public release for the emergency response guidebook and how they provide the right information in the right format is important. They need to be talking proactively with the DOT.
- It would be good to get a sense from the fire service personnel about how concerned they are with non-vehicular incidents. If they are aware, but not concerned, then the current focus on vehicles would seem the most appropriate. If they are concerned perhaps scope needs to be added.
- Data on the website usage of the awareness-level course would be good in order to show how strong interest is in that course and whether it's growing or not.

Project # SCS-17: Hydrogen Safety Training for Researchers
Salvador Aceves; Lawrence Livermore National Laboratory

Brief Summary of Project



Appropriate hydrogen safety instruction is key to avoiding accidents. Laboratory researchers handling small amounts of hydrogen need basic information on pressure, cryogenics, flammability, asphyxiation and other risks and precautions for using hydrogen. Technical personnel in charge of operations need comprehensive instruction on components, system design, assembly and leak testing. This project seeks to minimize risk of accidents and maximize productivity through improved knowledge of hydrogen properties and procedures. Objectives are to develop: 1) a four-hour web-based class for laboratory researchers handling hydrogen; and 2) a three-day hands-on safety class for technical personnel in charge of designing, assembling and testing hydrogen systems.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- I think training is critical to the advancement of the hydrogen industry.
- Appropriate hydrogen safety training is critical to avoiding accidents. The DOE Fuel Cell Technologies Program sponsors many laboratory research projects that must maintain good safety records. There are two audiences for the LLNL hydrogen safety training: 1) laboratory researchers who are new to hydrogen; and 2) technical personnel who are in charge of laboratory operations and need more detailed information on component design, assembly and leak testing.
- Safety training (online and in-person) is essential for the safe operation of hydrogen research operations.
- The project has a simple yet important premise, to give inexperienced technical people at universities, companies and even government agencies a web-based tool to learn about hydrogen and related technologies.
- The project contributes to the safety in handling hydrogen.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- Two courses were developed to serve the two target audiences: a four-hour web-based class developed for laboratory researchers who work with hydrogen and a three-day hands-on class developed for technical staff in charge of designing, assembling and testing hydrogen systems that will be used by researchers.
- The activity provides both online and in-person (which is being developed) training. This approach is important to impact a large number of people requiring general training through the online course, and also offers a more intense in-person class for those working more closely with high-pressure systems.
- The class materials and approach are easy to replicate for other organizations.
- This is a simple, yet technically sound, approach to presenting complex materials.
- It provides good feedback on knowledge retention with testing after each module.
- The module for laboratory personnel should be divided into two, because the one presented may be too basic for many personnel. Many technicians have some basic chemistry training and a more advanced module specific to hydrogen would be more rewarding

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- LLNL completed the six modules of the four-hour web-based course and they were peer reviewed twice by the Hydrogen Safety Panel. This course is now ready for use. Development of the three-day course is just getting started. A variety of components have been procured for the hands-on course (e.g., pressure vessels and regulators) in LLNL's high-pressure laboratory.
- Given the extremely low level of funding, this project is showing amazing progress, especially in comparison to other projects with much higher funding.
- Based on the budget, this project has done an extremely good job developing the web-based tool.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Hydrogen Safety Panel peer reviews of the web-based course resulted in valuable improvements to the modules.
- Peer review of the on-line material is excellent. LLNL has much experience in pressure safety training.
- There was good collaboration with Hydrogen Safety Panel and laboratory managers during the peer review.
- I would collaborate in the future with NASA technology managers and see if they can use it to train contractors.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- The project should maintain and improve the web-based class based on user comments and complete the hands-on class.
- The completion of the development of the in-person class is an important activity and should be fully supported.
- This work should become more of an educational activity with continued peer review by educators.
- Keep the same group to develop these expanded classes and fully fund the activity.

Strengths and weaknesses

Strengths
- The Web-based course makes good use of graphics and provides opportunities for students to test themselves on what they have learned.
- The technical expertise and facilities exist at LLNL to develop and provide the training.
- This was a great approach and produced sound technical material.
- It provided good materials and setting for training personnel in the safe handling of hydrogen and reached out to the right audience.

Weaknesses
- Do students have to come to LLNL for the three-day course? It would be more useful to have it tailored specifically to their own laboratory facilities.
- I found no weaknesses.
- The web-based course may need to be a multi-pronged approach providing introductory modules and more advanced modules for laboratory personnel who have associate or college degrees.

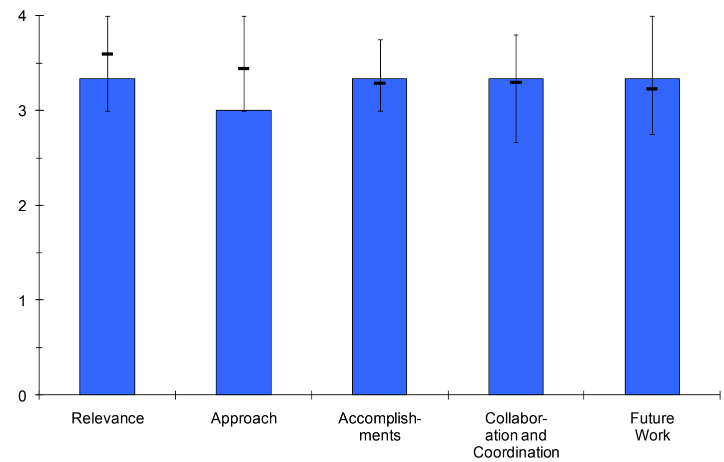
Specific recommendations and additions or deletions to the work scope

- In the testing module, I understand that if you mis-number one item you fail the test. It may be helpful to offer immediate feedback on the questions in the test, whether or not this would occur during the actual test and be recorded.

- The hands-on course should be thoroughly peer reviewed by the Hydrogen Safety Panel and other subject matter experts prior to offering it to prospective students.
- It is recommended to fully fund this activity, as it addresses many of the issues of safety for an expanded national effort to use hydrogen as an energy carrier.

Project # SCS-18: Optically Read MEMS Hydrogen Sensor
Barton Smith; Oak Ridge National Laboratory

Overall Project Score: **3.3** (3 Reviews Received)



Brief Summary of Project

The goal of this project is to develop optics-based sensing technology that achieves DOE research and development targets for hydrogen safety sensors. Milestones are to: 1) complete characterization of response time, recovery time, sensitivity and accuracy within the operating temperature range; 2) establish commercialization partnerships; and 3) demonstrate sensor performance and compliance with safety goals.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The project addresses the standard practice of safety variations.
- The cost-effective hydrogen sensor technology is an enabler for more robust hydrogen safety practices.
- Sensors are critical for infrastructure development.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The operational temperature range should be expanded to -40°C on the low end. This would address applications in both vehicles and appliances.
- More detail could be provided regarding the work flow (not just milestone dates). Some discussion on efforts related to manufacturing and packaging would be useful.
- This was an excellent presentation of the approaches selected. The presentation does not say which, or how many, approaches were discussed and discarded.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The overall technical accomplishments and achievements are good. The improvements in instrument response and size are noted. The identification of commercialization partners is also noted.
- The technology is very promising showing very acceptable performance across the performance target metrics. Examination of potential poisoning agents (hydrogen sulfide, ammonia, etc.) should be investigated.
- Working with industry partners should accelerate the market penetration time.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- The transfer of technology is noted.
- The project has a good mix of academic and industry partners with appropriate experience that are materially contributing.

- This is credible data in collaboration with private industry. Perhaps you could have some code people aware of the sensor work.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- This is prudent in scope given the commercialization potential.
- The future work is well thought-out and structured, but more detail should be provided regarding potential decision points.
- The proposed work seems more academic than commercial, but that's the process. I suggest revisiting the number three and four technologies to see if there are chances for improvement.

Strengths and weaknesses

Strengths
- Collaboration and technology transfer stand out as strengths in this project.
- A well-designed project that is delivering promising results.
- The cooperation between government and industry is encouraging.

Weaknesses
- I would have liked to see a nationally recognized testing laboratory like Underwriters Laboratories (UL) involved.

Specific recommendations and additions or deletions to the work scope

- One of the biggest code hurdles to acceptance of indoor fueling, specifically home fueling, is the lack of an odorant in hydrogen. Code officials should be aware of the advances in hydrogen sensor technology and its relevance to their understanding of hydrogen as a fuel. Also, an organization like UL should be involved in evaluating their technical progress with an eye on the device commercialization process. UL, or any other Nationally Recognized Test Lab (NRTL), will eventually be responsible for certifying the safe operation of the sensor as well as its performance to established requirements for Code officials.

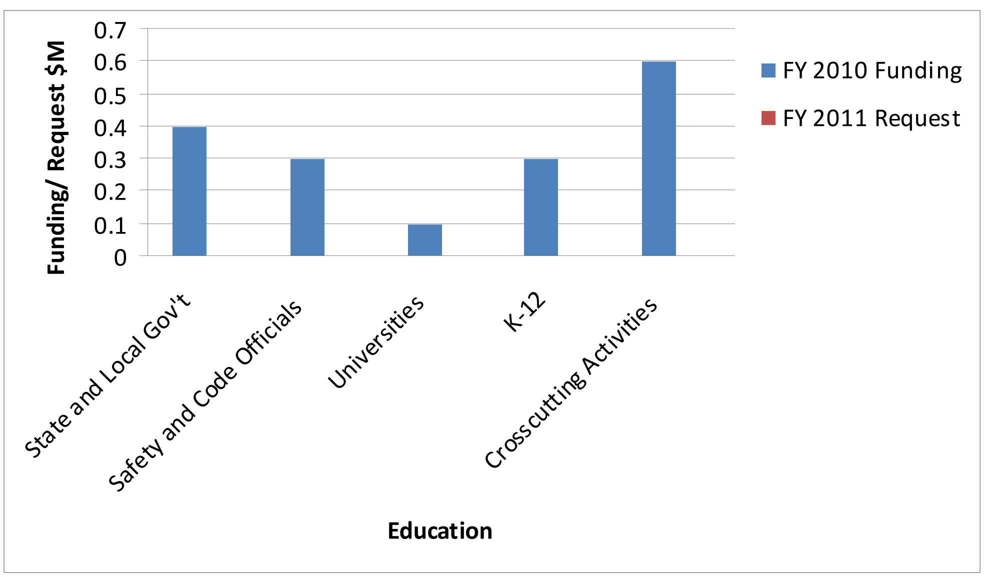
2010
Education
Summary of Annual Merit Review of the Education Sub-program

Summary of Reviewer Comments on the Education Sub-program:

Reviewers consider the Education sub-program to be focused, well-managed, and effective in meeting clearly delineated goals and objectives. They commended the broad project portfolio for its diversity of target audiences, and the reach of the projects was viewed as significant, with no evident gaps. However, in general, progress was not explicitly communicated and reviewers recommended data and quotes to measure the impact of education efforts on perception and knowedge. Reviewers noted the potential opportunity to strengthen end-user outreach efforts using performance data collected from ARRA and other deployment projects to build the business case for the use of fuel cells and hydrogen technologies. Concerns about limited funding appeared repeatedly in the overall sub-program review, and the individual project comments encouraged continuation of efforts across all target audience projects. Reviewers suggested increased and integrated education activities across EERE—they noted that incorporating messaging for hydrogen and fuel cells with other energy efficiency and renewable energy technologies, while emphasizing the complementary nature of the technologies, is key to an efficient and effective public education campaign.

Education Funding by Technology:

The Education sub-program efforts are prioritized to focus on the target audiences involved in facilitating the use of hydrogen and fuel cell technologies for near-term applications as well as for longer-term light-duty transportation applications. The FY 2010 appropriation allowed for support of mortgage obligations for projects across the education portfolio, including outreach to state and local government officials, university hydrogen and fuel cell education programs, in addition to ongoing efforts to educate first responders and code officials, and teachers and students at the middle and high school levels.



Majority of Reviewer Comments and Recommendations:

Education projects scored 3.6, 3.2, and 2.6 for the highest, average, and lowest scores, respectively. Scores reflect progress made over the last year and reported plans for future activity. Key comments and recommendations are summarized below. DOE will act on reviewer recommendations as appropriate to the overall scope, direction, and coherency of the Education effort.

Universities: Reviewers recognized the need for "a range of hydrogen and fuel cell educational opportunities for university students with the objectives of both general and specific training. There is an acknowledged need for educated trainers, engineers, and scientists to work in hydrogen and fuel cell technology areas." The projects were commended for their efforts to address these issues through a multi-element, hands-on approach, which allowed the project to reach a large group of general and engineering students in a meaningful and relevant way through lab kits, internships, textbook modules, seminars, general engineering curriculum modules, and specialized engineering classes. Reviewers acknowledged the strong contributions of each of the projects toward Education sub-program goals and viewed the education materials to be comprehensive, well-designed, and well-reviewed. In general, collaboration with industry and other stakeholder groups was viewed as well-developed, but reviewers strongly encouraged projects to engage with other educational institutions as a means for material distribution. Although reviewers thought the projects were successfully executed at each respective university, they noticed a lack of outreach to other universities, and recommended a more proactive effort toward disseminating the curriculum materials to other educational institutions and through remote teaching to optimize national impact. Additionally, reviewers wanted to see performance metrics measured through student information retention and opinion feedback on coursework.

End Users: Reviewers commended this project for providing an invaluable combination of education and direct hands-on experience through demonstrations with well-chosen lift truck users from a range of industries. They thought that the market sector, material handling equipment, was well chosen as a promising area for commercial growth and ideal for targeted outreach. The educators, from an established forklift distributor, were situated to reach out to potential buyers as trusted spokespeople in the industry. Furthermore, reviewers noted the broad target audience, which included forklift operators, end-users, and the first responders and fire marshals who would be involved in permitting potential installations. Reviewers would have liked to see more interaction with material-handling trade associations and workshops at lift truck conferences rather than at hydrogen and fuel cell venues. They suggested a better developed performance data collection effort and recommended tracking education impact by measuring changes in opinion and knowledge. Reviewers encouraged similar follow-on projects.

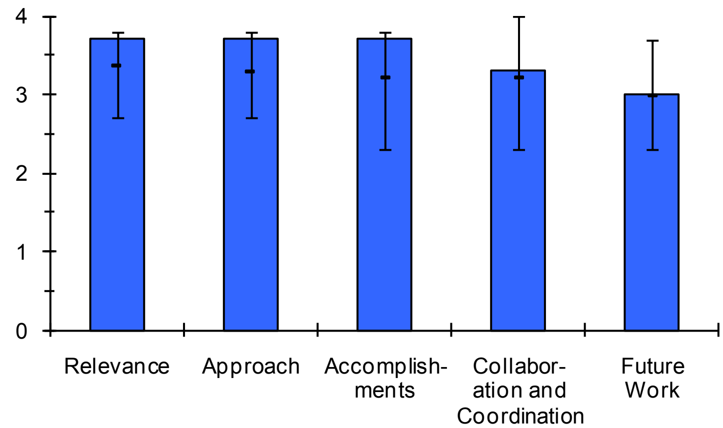
State and Local Government Officials: Since state and local leaders are potential technology deployment facilitators, their education was seen as essential to the future success of hydrogen and fuel cells. The locales of the projects were well-chosen, in states with an existing hydrogen and fuel cell presence, with the addition of complementary national efforts to reach the broadest audience possible. Reviewers commended the multi-pronged approach of all the projects implementing outreach through face-to-face meetings, webinars, conference presentations, fact sheets, case studies, best practices, financial and feasibility models, and traditional and new media. Real-world experience—both actual and virtual, through ride-and-drives and nationally broadcast video segments—was viewed as especially effective in educating state and local leaders. Reviewers valued the honest communication imparted through these hands-on experiences and the financial and feasibility models—these elements provided a realistic basis for recommending fuel cells without overselling the technology. Collaboration was considered to be comprehensive within the state, but many suggested increased cooperation with other neighboring states. Reviewers encouraged the individual state projects to serve as a model for other states by sharing best practices and lessons learned. Some activities were considered to be too focused within the hydrogen, fuel cell, and clean energy community; reviewers suggested moving beyond this already educated group to reach out to state and local leaders through established local govement meetings and organizations.

First Responders and Code Officials: These co-funded projects were reviewed under the Safety, Codes & Standards sub-program at the 2010 AMR. See the Safety, Codes & Standards section of this report for reviews and comment summaries.

Project # ED-03: Hydrogen and Fuel Cell Education at California State University, Los Angeles
David Blekhman; California State University-Los Angeles

Brief Summary of Project

Overall Project Score: 3.6 (3 Reviews Received)



The overall objective of this project is to implement a comprehensive set of curriculum development and training activities. Objectives include: 1) developing and offering courses in fuel cell technologies, hydrogen and alternative fuels production, alternative and renewable energy technologies as a means of zero-carbon emissions economy and sustainable environment; 2) establishing a zero-emissions proton exchange membrane (PEM) fuel cell and hydrogen laboratory supporting curriculum and graduate students' teaching and research experiences; 3) providing engaging capstone projects for multi-disciplinary teams of senior undergraduate students; and 4) fostering partnerships with automotive original equipment manufacturers (OEMs), energy providers, community colleges, government agencies and other stakeholders.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This is a good project for fuel cell and hydrogen education, but it is limited to California State University (Cal State), Los Angeles, and not much else.
- It is an outstanding educational program with good attendance and curriculum.
- Ensuring that universities have courses in hydrogen and fuel cells is critically important to meeting DOE RD&D objectives.

Question 2: Approach to performing the work

This project was rated **3.7** on its approach.

- The PI is working with the College of Engineering and Department of Technology and weaving course curriculum into a variety of lower and upper division course work. Three different courses are offered. GM Corporation, Honda, Southern California Edison, and the California Fuel Cell Partnership are good partners. The location in the center of a future hydrogen fuel cell vehicle rollout site is ideal for an educational thrust.
- This is absolutely appropriate for an educational institution. The course work combined with laboratories is right on.
- The approach addresses several DOE barriers in a logical, effective manner.

Question 3: Technical accomplishments and progress

This project was rated **3.7** based on accomplishments.

- The PI met the objectives that were in the proposal. There is a question as to the relevance of writing an operating manual for an electrolyzer when that is usually the company's responsibility. But it was educational for the graduate student who did it. Out of 800 engineering students, 150 took the courses. There are 22,000 students total at Cal State LA. The focus was mostly on light-duty vehicles, not other fuel cell applications.

- The project showed outstanding progress in the development of these curriculum elements. While there is some concern about sending the correct message around hydrogen safety, this program at least attempts to address the issues in a balanced manner. It is strongly suggested that the PIs familiarize themselves with the unintended release work done under the codes and standards Fuel Cell Technologies sub-program element. While the basic flame properties, buoyancy, and diffusivity are important pieces of information to impart, they can be very misleading with respect to predicted behavior. Predicted flame and/or hydrogen diffusion behavior is almost always dominated by the combustion fluid dynamics of the particular scenario, which could differ markedly from the behavior expected when considering only the molecular properties.
- The project team is doing great work reaching the maximum number of students that can be accommodated with the course and laboratory setup. It is good to know what graduates have decided to do in the workforce. It would be good to share more data on that in future presentations.

<u>Question 4: Collaboration and coordination with other institutions</u>

This project was rated **3.3** for collaboration and coordination with other institutions.

- Collaboration with partners was good to ensure accuracy of curriculum and displays. Demonstrations of hydrogen fuel cell vehicles help drive the education point home.
- The project has developed good collaborations where appropriate, such as the hydrogen fueling station and the hydrogen internal combustion engine program. The team is commended particularly on the collaboration with the National Fuel Cell Research Center at the University of California-Irvine.
- There is a lot of coordination. It was good to see that the PI took advantage of events that came to the southern California region for his coursework and expanded education by exposing students to hydrogen and fuel cell professionals at the National Hydrogen Association Conference and the Huntington Beach event.
- It would be good to see coordination with Humboldt State University (HSU). They did not seem to be included among the collaborators listed. In the presentation by HSU's Schatz Energy Research Center (Schatz/HSU), future work with other CSU campuses was mentioned, so it seems like there is an opportunity here to build on the successes. Perhaps the group will present all courses together to other campuses that may wish to replicate part or all of the different courses developed by this group and Schatz/HSU.

<u>Question 5: Proposed future work</u>

This project was rated **3.0** for proposed future work.

- This is a point solution that will serve only Cal State L.A. Federal hydrogen and fuel cell education funding should be used to maximize the breadth of outreach that is accomplished. This is not the normal way universities operate because course curriculums are tightly held and not readily shared.
- This project is scheduled to end September 2010. The list of future work might be a bit aggressive for three months of work. It would be good to see them thoroughly complete a smaller list rather than leave some items partially done at the end of this project. On the other hand, it appears that this work may continue beyond the current project life, which is excellent.
- It looks like future work is a continuation of current work. That's good because more outreach is needed. However, there's an opportunity to take the courses developed at this school to other campuses. See the Collaboration section for more specific ideas.

<u>Strengths and weaknesses</u>

<u>Strengths</u>

- The education effort is in the center of a prime region for initial commercialization of hydrogen fuel cell vehicles.
- The project is teamed with GM Corporation, Honda, Southern California Edison, and the California Fuel Cell Partnership to ensure good curriculum and educational events.
- This is a much-needed, well-thought-out undergraduate curriculum for hydrogen technologies. The coursework is combined with substantial laboratory work, which is excellent.
- There is a lot of coursework; students seem to enjoy the courses.

Weaknesses

- This is a point solution not readily transferrable beyond Cal State L.A.
- There is a previously mentioned concern about making sure that they do not send a misleading message with respect to hydrogen behavior in the real world. The reviewer is not penalizing the project for this concern; however, he/she does want to see real-world behavior embraced in the teaching of hydrogen behavior. For example, it can be very misleading, and dangerous, to compare hydrogen releases to methane releases. Under the same release conditions, a hydrogen flame extent is significantly larger than methane.
- None noted. There are opportunities to gather more information on the reception of the students to this coursework, including what they are learning from the courses, and coordination with the Schatz/HSU project.

Specific recommendations and additions or deletions to the work scope

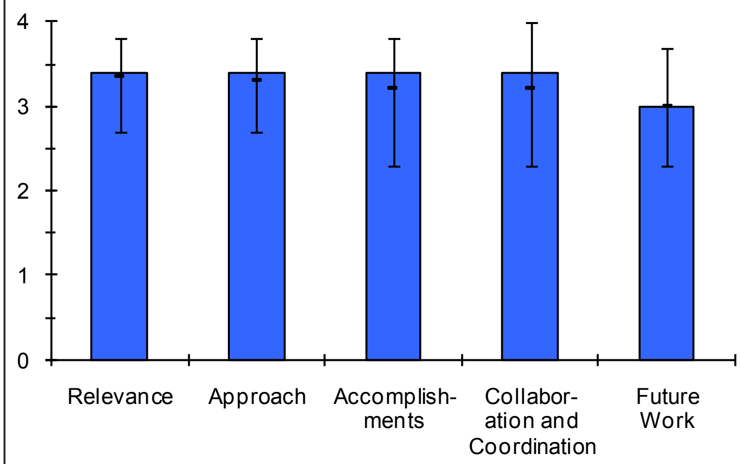
- It would be good to see coordination with HSU. HSU didn't appear to be listed among the collaborators. In the Schatz/HSU presentation, they mentioned future work with other California State University campuses, so it seems like there's an opportunity here to build on the successes. Perhaps they should present all courses together to other campuses who may wish to replicate part or all of the different courses developed by this group and Schatz/HSU.
- It would be good for students to be surveyed on what concepts they retained from the course and whether they would recommend the course to other students.

Project # ED-04: Hydrogen Energy in Engineering Education (H2E3)
Peter Lehman; Humboldt State University Sponsored Programs Foundation

Brief Summary of Project

The objectives of this project are to: 1) deliver effective, hands-on hydrogen energy and fuel cell learning experiences to a large number of undergraduate engineering students at multiple campuses of the California State University (Cal State) and the University of California (UC); 2) provide follow-on internship opportunities for students at hydrogen and fuel cell companies; and 3) develop hydrogen teaching tools to be used commercially, including a basic fuel cell test station and a fuel cell/electrolyzer experiment kit suitable for use in university engineering laboratory classes.

Overall Project Score: 3.4 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project is relevant because it is directed toward providing a range of hydrogen and fuel cell educational opportunities for university students with the objectives of both general and specific training. There is an acknowledged need for educated trainers, engineers, and scientists to work in hydrogen and fuel cell technology areas. This project attempts to address both of these recognized needs.
- The goals are aligned with producing an informed set of engineers and citizens.
- Given the short nature of the units, the actual progress toward the objectives would be slow.
- California is a leading state in hydrogen technology, and Cal State's program provides good reach for education curricula within the state. However, the level of reach outside of California is unclear, even if the project is successful in penetrating the state university system.
- This project is highly relevant to the DOE Hydrogen Program education objectives.
- The PI's ability to connect with engineering students is unmatched. It's their "bread and butter," and they seem to be doing a great job at execution. It's important for DOE to make sure that outreach is conducted with this important target audience.

Question 2: Approach to performing the work

This project was rated **3.4** on its approach.

- This project has adopted a multi-element approach to provide educational opportunities for students. The approach effectively provides classroom instruction, laboratory experimentation, hands-on experience in fueling station analysis, and potential industry internships.
- They intend to reach their goals by delivering fuel cell test stations and electrolyzer/fuel cell kits and by providing training to maintain them.
- The project is available to many students at UC Berkeley, and Humboldt State University (HSU), but it not as valuable anywhere else. That means there is a limited impact on the need for educated workers. The real value is probably in acting as a model that others might imitate, but that would require actual dissemination of the program and instructions on assembling the equipment it takes to implement the modules.
- It is good to lead with hands-on tools like kits and test stations.
- The focus is hands-on, fuel cell-related activities with students.

- The curriculum module approach is an effective one.
- Part of what makes this approach outstanding is that hydrogen education has been built into all engineering disciplines, not just as a stand-alone course. It makes the education more effective and reaches more students in a way that's relevant to them. Another outstanding element is the electrolysis and fuel cell demonstration unit.

Question 3: Technical accomplishments and progress

This project was rated **3.4** based on accomplishments.

- The project has made substantial progress in its instructional activities. Several hundred students have taken coursework and laboratories that are developed and integrated by the project as part of the existing engineering program. In parallel, the project has developed and fabricated hydrogen production/fuel cell demonstration kits. The project has also made information regarding its courses and activities available to the educational community and general public through online sources.
- The project made and delivered the kits, then informed a significant number of staff members.
- The curriculum was well received by its target audience.
- The video may be used for general information.
- The project was evaluated on its effectiveness, and the results included a 25% increase in the ability to answer basic questions.
- Students are educated on the basics of fuel cell technology.
- There is concern that the team has made little penetration beyond two universities.
- There are significant accomplishments and progress. The teaching kits are very good for hands-on student experience.
- The electrolyzer and fuel cell kits are top notch. There is great outreach to more than 250 students. They have made great progress since last year, and this should be a sustainable program reaching many more HSU students and students in other universities. The connection between the lab program and the station and vehicles is vital.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for collaboration and coordination with other institutions.

- Collaboration with other California universities and with industry partners was reported.
- There is reasonable collaboration with good value.
- The difficult economy has probably hurt the efforts here. At least one of the industry partners has been experiencing financial difficulty and downsizing. It appears that the project team's progress is mostly at two universities, so many more active university partners need to be recruited.
- There are excellent collaborations with other institutions.
- There is great collaboration with a variety of different organizations.

Question 5: Proposed future work

This project was rated **3.0** for proposed future work.

- The plans for future work focus on continuing the effort and activities already underway. This seems reasonable and appropriate to complete the project. The plans to widen the dissemination of project information at educational conferences and on the Internet are especially pertinent.
- The project is going to expand the implementation at allied campuses.
- This is probably the best they can do with funds available. Dissemination to others is probably the most valuable and should be favored.
- The project really needs to penetrate into many other universities and colleges in the Cal State system. It has impacted fewer than 300 people so far.
- The future work looks good.
- It's good to hear that outreach to other California universities is planned for next year, as is outreach to additional faculty to teach these courses. The reviewer hopes the internship program situation can improve.

Strengths and weaknesses

Strengths

- This project has dedicated and experienced staff with demonstrated knowledge and expertise.
- There is hands-on impact.
- The project involves developing hands-on tools such as fuel cell/electrolyzer kits.
- There is an excellent focus on activities that make an impression on students.
- The hands-on experience is critical to retaining information about hydrogen and fuel cells and encouraging students to share it.

Weaknesses

- Financial support for the industry internships is needed.
- There is a limited area of impact geographically.
- There is limited penetration into college campuses within the state's university systems.
- The poor economy has hindered the potential for internships.
- None.
- None noted.

Specific recommendations and additions or deletions to the work scope

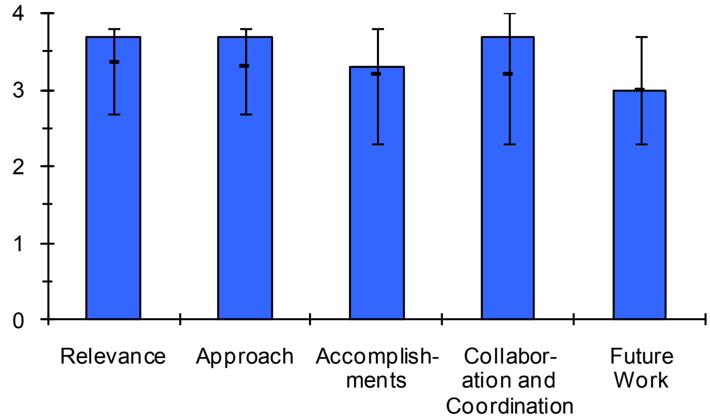
- The project team should strongly encourage DOE to consider earmarking funding for sponsoring industrial internships. Relatively little funding would be required to support several internships, and industrial sponsors could be required to provide like-minded support.
- The project should be encouraged (allowed) to broaden its work to include educational institutions outside of California.
- There is a need to recruit other industry partners. For example, Altergy Systems is located near Jadoo Power, and they have made good progress in the backup power market. Oorja Protonics is another possibility. Bloom Energy could be recruited to be an industry partner, because it is focusing its sales effort in California for the time being. The national laboratories in California may be candidates for collaboration or for placement opportunities for interns.
- It is recommended that the Schatz Energy Research Center looks in more detail at the results of the National Hydrogen Association's U.S. Market Report, specifically the education section that shows the states that have the most university-level hydrogen or fuel cell courses. Perhaps there's an opportunity to use these schools as way to reach further with the curricula and other parts of the Hydrogen Energy in Engineering Education (H_2E^3) program.
- The quotes from students are great, but it would be better for students to be surveyed on what concepts they retained from the course and whether they would recommend the course to other students. Feedback would tell all of us what's being learned and also provide valuable information to the instructors.
- It is suggested that coordination with other southern California university campuses, as outlined in future work, to include the Cal State coursework by David Blekhman specifically, which seems to be complementary. Pitching the combined coursework that has been developed to other schools could really help spread these successful projects more effectively.

Project # ED-05: Hydrogen Education Curriculum Path at Michigan Technological University
Jason Keith; Michigan Technological University

Brief Summary of Project

The overall objective of this project is to expand existing university programs in fuel cell and hydrogen technologies. Objectives are to develop: 1) courses in hydrogen technology, 2) curriculum programs in hydrogen technology, 3) hydrogen technology-related modules for core and elective engineering courses, and 4) hydrogen technology modules to supplement commonly used chemical engineering texts.

Overall Project Score: 3.5 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This project is relevant in that it is developing university course materials in hydrogen and fuel cell technologies for use at both the undergraduate and graduate levels. There is a recognized need for training in hydrogen and fuel cells for future engineers.
- The project is well aligned for the school.
- There is good alignment looking at a national perspective.
- The project is highly relevant to DOE Hydrogen Program education objectives. There is potential for a widespread impact.

Question 2: Approach to performing the work

This project was rated **3.7** on its approach.

- The approach adopted by this project has been to develop course modules that can be used at the university level to inform and educate students on hydrogen and fuel cells. This is in an effort to help prepare for a future need for technologists in these areas. The modules have been used and tested in courses offered at Michigan Technological University (Michigan Tech). The modules they developed incorporate hands-on hardware experiments and demonstrations in partnership with fuel cell suppliers.
- This project directly attacks the problems targeted in Michigan, but there is also substantial outreach, that is not dependent on hardware, to the United States as a whole.
- This is a full curriculum, not a one-day or one-week module.
- The module approach is excellent. It makes the hydrogen information convenient to incorporate into a variety of education courses and allows for widespread dissemination.

Question 3: Technical accomplishments and progress

This project was rated **3.3** based on accomplishments.

- The progress in this project appears to be consistent with the approved project schedule. Approximately half of the project is completed (56% specifically), and completion is scheduled for July 2011.
- This project is implemented at undergraduate and graduate levels.
- The modular format allows other institutes to pick up as much information as they need that fits their program.

- The curriculum has been developed for multiple disciplines, integrated into accepted texts, disseminated to other universities by Internet as drop in modules, and tested at approximately 10 places.
- Significant progress has been made.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.7** for collaboration and coordination with other institutions.

- The project has involved a large number of collaborators including equipment supplier and external reviewers from both academia and industry. Discussions with industrial stakeholders regarding project goals and objectives were conducted. A number of presentations were listed with co-authors and co-presenters from several academic institutions.
- There is a long list of reviewers. They did help refine the program.
- It is not clear whether the reviewers had much up-front input to shape the work.
- The modules will be disseminated to other universities throughout the country.
- The project is attempting to develop more industrial interactions.

Question 5: Proposed future work

This project was rated **3.0** for proposed future work.

- Plans to complete this project were stated as continuing the activities underway and appear to be consistent with the stated project goals and objectives. This includes completing module development for supplements to existing engineering texts.
- The plans are on track for the program.
- The plans are suitable.
- The future work is consistent and well integrated with the project objectives.

Strengths and weaknesses

Strengths
- There is a strong approach to preparing course modules with an external review to guide and direct the effort.
- The module approach is excellent.

Weaknesses
- The schedule seems somewhat relaxed. Some effort should be taken to ensure that the project is completed on-schedule with maximum effort devoted to disseminating information regarding the availability of course materials and encouraging the inclusion of course modules in engineering classes at other institutions for additional testing and verification.
- There is a need to continue to work on strengthening industrial interactions.

Specific recommendations and additions or deletions to the work scope

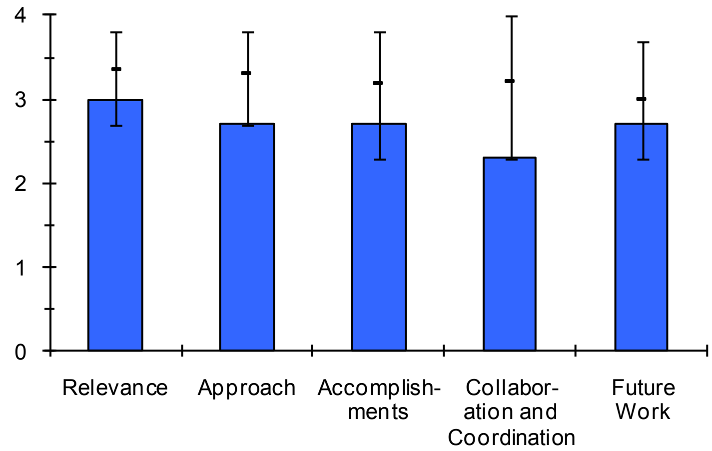
- Testing and validation of the module's effectiveness should be accelerated as much as possible.
- A proactive approach to promoting and advertising the availability of the course materials is recommended, including encouraging the testing and evaluation of the materials at other educational institutions.
- It would be beneficial if the module information could be incorporated into a stand-alone book that is an introduction to fuel cells and that can be purchased online on Amazon.com®, for instance. This might be quite valuable to students at other institutions and in other disciplines.

Project # ED-06: Hydrogen and Fuel Cell Education Program Concentration

David Block; University of Central Florida

Brief Summary of Project

Overall Project Score: 2.7 (3 Reviews Received)



The overall objectives of this project are to: 1) develop and sustain an education concentration in Hydrogen and Fuel Cell Technology (HFCT); 2) prepare students who can successfully work as HFCT professionals in government, industry, and academia, 3) produce graduates who will demonstrate knowledge, techniques, skills, and modern tools related to HFCT, and who will be able to apply current knowledge and adapt to emerging applications of HFCT.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The project is aimed at the right goals.
- It wasn't clear whether the initial program was positioned to be a substantive contributor to the program's goals. The fact that the University of Central Florida (UCF) discontinued the program made it even less relevant. While the co-PI appears to be giving good effort in trying to relaunch the program in North Carolina, the University of North Carolina at Charlotte (UNCC) has not shown much interest in being a leader in terms of considering hydrogen and fuel cells for economic development. UNCC is a small campus, and there is little industry interest or presence in fuel cells in the Charlotte area.
- These courses are vital to addressing the DOE's RD&D objectives.

Question 2: Approach to performing the work

This project was rated **2.7** on its approach.

- The approach is to develop and/or offer a series of courses supporting fuel cell technology, including laboratory and text courses. To some extent, these are available to local junior colleges, too.
- Any impact outside of the UNCC area seems limited at present, so the impact of this approach is also limited.
- The decision by UCF to discontinue the program has been a substantive handicap, despite the co-PI's efforts to re-launch at UNCC. There needs to be a presence and reinforcement by active industry partners for a college curriculum to be effective. This doesn't seem to be happening in Charlotte.
- It is good to see that the PIs were able to find a creative way to handle a difficult budget issue at UCF. The approach to incorporate hydrogen and fuel cell information into many different courses to reach more students and show the integration of the technology in many areas is admirable.

Question 3: Technical accomplishments and progress

This project was rated **2.7** based on accomplishments.

- From what can be learned through the AMR process, the courses have been developed and offered as planned and seem to be of good quality.
- It appears that the courses are being executed and received well.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.3** for collaboration and coordination with other institutions.

- There is collaboration that exists. Some of what is listed is similar to grant sources and not true collaboration. Grants are valuable, but they do not drive to the heart of what DOE is after in collaboration, such as intellectual cross fertilization and program improvement.
- It would be much better having them [collaborators] help shape or even write portions of the curriculum, offer lecturers, among other kinds of support.
- The project did place interns with a collaborator, but not a true collaborator as defined by the DOE.
- The project team does get some feedback from advisors.
- The Florida Solar Energy Center does not appear to be an active collaborator. The co-PI has made little progress in attracting other collaborators. It seems that he overstates the progress he has made; in several instances he appeared to be attributing phone contacts as collaborators. Having a meeting with a company or an institution is not a collaboration.
- The project team demonstrated good collaboration with Savannah River National Laboratory and other organizations. As with other projects in this area of university-level coursework, it is recommended that they collaborate with Humboldt State University and California State University, Los Angeles, that have other projects in this area. It seems like there's an opportunity here to build on their successes, and perhaps present all courses together to other campuses that may wish to replicate part or all of the different courses developed by this group.

Question 5: Proposed future work

This project was rated **2.7** for proposed future work.

- The plans are on track with funding and with the program plan.
- It is good to see that outreach through coursework will continue.

Strengths and weaknesses

Strengths
- The effort by the co-PI to sustain the project is a strength.
- The fact that university-level students are learning about hydrogen and fuel cells is good.

Weaknesses
- There are no collaborators.
- There needs to be more data and information on how students are reacting to the coursework. What are they learning? Would they recommend the course to a friend?

Specific recommendations and additions or deletions to the work scope

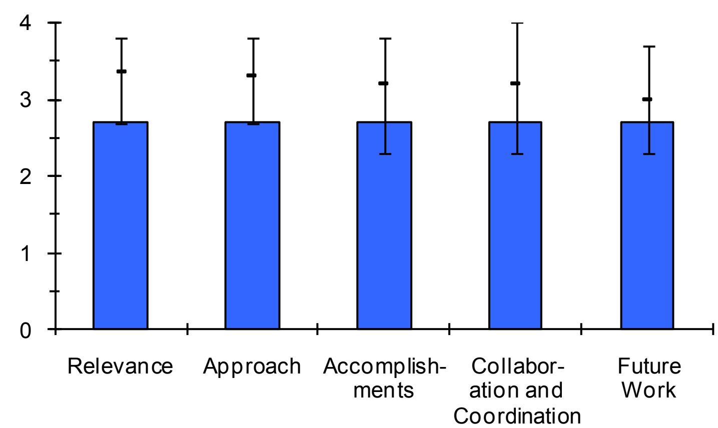
- The project should work to move the value in these courses to other teaching locations by exporting the curriculum or by remote teaching.
- This project will not be a substantive contributor to the DOE Fuel Cell Technologies Program mission and objectives.
- As suggested in the collaboration section, the PI should coordinate with projects underway at Humboldt State University and California State University, Los Angeles.

Project # ED-07: Development of a Renewable Hydrogen Production and Fuel Cell Education Program
Michael Mann; University of North Dakota

Brief Summary of Project

The primary objective of this project is to provide formal multi-disciplinary renewable hydrogen production and fuel cell training to undergraduate and graduate level engineers and scientists. The project includes training at three levels to maximize program benefits including: 1) a broad overview to expose a large number of students to the basics of hydrogen technologies, 2) mid-level training for a moderate number of students, and 3) detailed training for a smaller subset with interest and potential to make significant contributions to technology development.

Overall Project Score: 2.7 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- North Dakota is a good location for renewable energy—specifically wind—and hydrogen and fuel cells could help store and utilize excess electricity. It's a rural midwestern state and most hydrogen education activities have not occurred in this area.
- There is a good and varied range of target students.
- It is not clear how far the results will go outside North Dakota.
- Outreach to middle school is good, and including tribal colleges is excellent.
- Reaching out to university students is very important. However, this presentation seemed so much weaker than the Humboldt State University and California State University, Los Angeles, projects that it seems less relevant.

Question 2: Approach to performing the work

This project was rated **2.7** on its approach.

- There is a solid approach to provide educational materials on hydrogen and fuel cells.
- Multiple approaches, such as several levels of courses (general, engineering, and advanced engineering), are good.
- Using trained PhD students to teach this curriculum adds another dimension toward achieving the goals.
- Associated information streams, such as seminars, are probably helpful, though hardly extraordinary.
- This project may address the technical barriers better than the presenter conveyed. Based on the information conveyed, it's not clear that this project is doing as aggressive a job of breaking down technical barriers as other similar projects.

Question 3: Technical accomplishments and progress

This project was rated **2.7** based on accomplishments.

- The PI met most of the deliverables envisioned. However, these will not translate well outside North Dakota.
- Several courses are in place including classroom and laboratory courses.
- The curriculum shown was relevant and meaningful.
- The project was taught to engineers in several disciplines successfully.

- The project uses students to teach.
- Interns are placed in good locations, and one has been employed.
- The project is delivering some content to rural ND locations.
- There is modest progress in overcoming barriers; yet the rate of progress appears to have been slow.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.7** for collaboration and coordination with other institutions.

- The project partners are diverse and provide real value.
- The collaboration could be better. The existing collaborators are high quality, but there seem to be few people outside of this sphere who may know about this project.

Question 5: Proposed future work

This project was rated **2.7** for proposed future work.

- The work is aligned correctly and follows the plan that was established.
- It is suitable for the funding level.
- If the plan for future work is executed more aggressively, many more results could be realized.

Strengths and weaknesses

Strengths

- This is a rural state with the potential to use hydrogen for excess wind energy storage and/or fuel cells with agricultural waste.
- The project hits many levels of students and many disciplines, so it is informing a wide range of people from laymen to experts.
- It's important that coursework exists at the university level, but the specific strengths of this project were hard to understand.

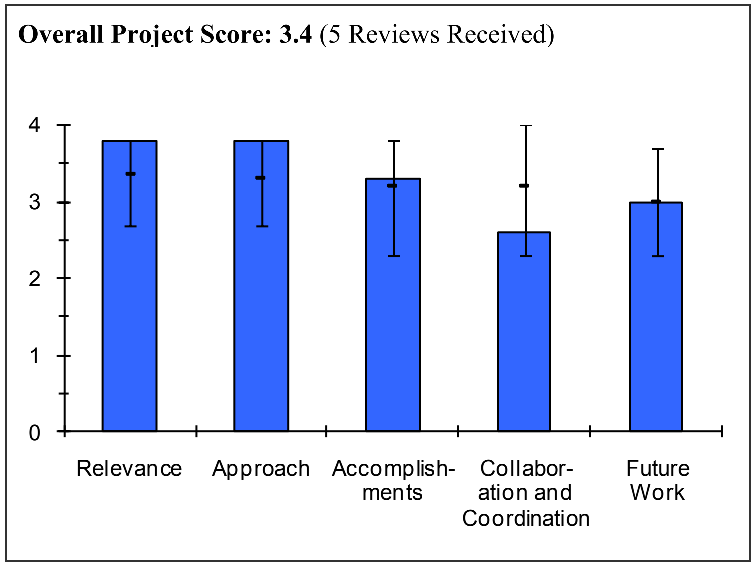
Weaknesses

- It is difficult to disseminate this educational material funded with federal dollars beyond the state.
- The project helps promote DOE goals in the area of the university and part of North Dakota but not nationally. It would benefit from a viable and meaningful dissemination plan.
- To be honest, the presentation was weak and the presenter was only able to get to half of the slides. More information needed to be conveyed on what was actually accomplished with this project. Some tables indicated that work was completed, but no real information was offered on what's been done. Case studies have been completed but aren't available anywhere. Questions about how many students have been reached and what they have learned in the courses remain unanswered.

Specific recommendations and additions or deletions to the work scope

- The team needs to make the project better known to educators so it can be used more extensively.

Project # ED-08: Dedicated to the Continued Education, Training and Demonstration of PEM Fuel Cell-Powered Lift Trucks In Real-World Applications
Tom Dever; Carolina Tractor & Equipment Co. Inc.



Brief Summary of Project

The LiftOne program objectives are: 1) to educate a broad group of stakeholders on the benefits of fuel cell and hydrogen technology by conducting hydrogen education seminars at various locations, 2) to demonstrate clean energy through the execution of a series of month-long deployments of two hydrogen fuel cell-powered lift trucks at large, strategically selected electric fleet user locations, and 3) to assist in the commercialization of fuel cell and hydrogen technology through longer and geographically diverse deployments in real-world applications.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- This is an excellent project for educating potential purchasers of hydrogen fuel cell-powered lift trucks. LiftOne is in a perfect position to show its warehousing clients new technology and have the experience and credibility to educate these potential future customers.
- The project is an excellent example of a market transformation and education activity. The use of a forklift marketer offers a good opportunity to inform the market. This helps to expand consideration for the technology.
- Hands-on education of hydrogen technologies is critically important to accelerate the deployment of these technologies. This is important for users, fire marshals, operators, and nearby communities.
- Material handling equipment (MHE) providers have continued to be a strong and successful early market for fuel cells and hydrogen. There is a lot of interest by the MHE industry and its customers on the benefits of fuel cells. American Recovery and Reinvestment Act (ARRA) projects should provide a substantial amount of data that could support a continued and sustained educational program.
- Education about hydrogen and fuel cells in the material handling sector is crucial to the success of this early market.

Question 2: Approach to performing the work

This project was rated **3.8** on its approach.

- Giving the potential clients a chance to use a hydrogen fuel cell lift truck — try before you buy — is a great approach to both educating and marketing these new early market applications. The battery industry people showing up allowed for a lively exchange. They provided great feedback on what the rank and file think about hydrogen and fuel cells and their perceived utility in industrial warehouse settings. Ingersoll Rand, Stanley Tools, Lowe's, Bausch & Lomb, BMW, and Electrolux represent a wide variety of potential large users of this technology.
- Although the demonstrations seem to provide some amount of exposure, it's not clear whether the project is collecting sufficient information for deployment considerations. The PI should reveal more data that can be shared on fuel cell performance.
- The coincidental deployment, education, and cost evaluation is a powerful way to get this technology recognized by the general public. This approach is good.

- Given the early market potential of fuel cells for MHE applications, the specific focus on this market is worthwhile.
- The approach of using hardware to increase education is a proven technique. The PI is a natural educator and spokesman for the technology.

Question 3: Technical accomplishments and progress

This project was rated **3.3** based on accomplishments.

- LiftOne diligently performed this project with multiple potential users and did so on-schedule with very good feedback received. The BMW demonstration eventually led them to a purchase, albeit a Plug rather than a Hydrogenics fuel cell power pack was used. The PI would receive an "A" for the delivery of Electrolux and Stanley Tool demonstrations as well.
- Of the three elements, there have been accomplishments on education and deployment demonstrations, but there wasn't much evidence that the work has assisted in commercialization. There are no indications of continuing or expanding deployments.
- They have had a minor bump in their deployment plans for reasons outside their control. It's disappointing that they were not a little faster in finding a new deployment site when it became clear that the planned deployment site needed to withdraw from the program because of internal production demands.
- In the targeted areas of North Carolina and South Carolina, the delivery has been successful in terms of working toward the adoption of fuel cells by the customers in this region. It may have been helpful to have made presentations at MHE conferences instead of at the National Hydrogen Association and the U.S. Fuel Cell Council conferences.
- There is great information and candor about the problems experienced. The PI should be commended for sharing these challenges. The accomplishments for the deployment are really impressive. It would be good to show the reactions and results from the educational parts of the program. Testimonials of the operators' experiences were presented, but information on how education improved their perceptions would be good to show in the future.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.6** for collaboration and coordination with other institutions.

- LiftOne collaborated well with each of the client sites to which they brought the Caterpillar Class I Hydrogenics fuel cell lift truck. They also rebounded well when their partnership with Michelin fell through, and they quickly identified Ingersoll Rand as a suitable replacement. Safety education was a large part of this effort, and they made tremendous inroads with rank and file workers in this regard.
- The only real coordination mentioned was with Hydrogenics. There must be trade associations and material handling venues that would be good to research for more potential collaborators.
- The collaboration is not extensive, but that is the nature of this project.
- There is concern that Hydrogenics is the partner because Plug Power and Nuvera appear to be making much more progress in this market. However, at the time the work was awarded, Hydrogenics was in a better competitive position.
- Collaboration with companies involved with deployments has been excellent. The project could use more collaboration with the educational elements. Perhaps they could seek out national associations or other entities doing similar DOE education projects.

Question 5: Proposed future work

This project was rated **3.0** for proposed future work.

- During the Q&A, the PI pointed out that a future effort would require the opportunity to let the clients use the lift truck for a longer period of time. A follow-on, two-year opportunity was recommended. A fuel cell forklift leasing program would be an ideal part of such an effort.

EDUCATION

- The project seems to be pursuing the same course for the duration. No efforts appear to exist to work harder toward commercialization.
- This program is moving ahead nicely. It's recommended that the project team be aggressive in getting its last planned deployment executed.
- With the amount of data reporting from the Defense Logistics Agency (DLA) pilot and the ARRA projects being delivered during FY 10, it may be strategically advantageous to have a conduit for a targeted public outreach effort.
- The presentation could use more detail on the educational part of this project. Other future work seems logical, and the reviewer looks forward to hearing the results.

Strengths and weaknesses

Strengths

- The project is going directly to a potential buyer of fuel cell lift trucks.
- The PI's reputation of working in the customer's best interest—to gain and/or retain their business—gave great credibility to the effort.
- The PI is outstanding.
- The targeted education objectives were twofold: 1) educate the future users of fuel cell lift trucks, and 2) help sell them at the same time.
- The area of material handling is an area where significant deployments could be made and supply chains energized for broader benefit. The stranded fleet issue is not present, which makes this an ideal opportunity to learn more about fuel cell deployments and benefits.
- The hands-on educational approach to get the commercial handling community familiar with this technology is good. The combination of hands-on experience and the educational model targeted toward operators and end-users, plus first responders and fire marshals, is excellent.
- There is a focus on the specifics and arcane needs of the MHE customers.
- Certainly, the deployment of the vehicles and the data shown on the use and performance of the lift trucks is a strength.

Weaknesses

- Having only one fuel cell power pack OEM involved limits the evaluation to just that product line.
- The project does not seem to be expanding much knowledge in the actual performance and business case areas for fuel cells in forklift deployments.
- There is concern that some information that is not really necessary for the safe use and familiarity with the technology is imparted to the community (hydrogen 101). A message that needs to be sent is one of excitement for the technology and how it is better than, or competes with, existing technologies. Another concern that in a safety discussion the comparison among propane, gasoline, natural gas, and hydrogen could be creating a wrong and confusing picture. It would be advisable to limit a hydrogen safety discussion to those issues that are needed for the operators and end-users to ensure a safe response in case of an inadvertent accident. Those in the organization who are responsible for the safe installation and fire accident responders would need the more detailed discussions. However, it does appear that the recipient of this educational module received it well.
- There is a regional focus. The project is not giving presentations at MHE industry conferences.
- The timing of the project preceding the availability of cost and performance data, such as the data that will be provided by the ARRA and DLA pilot projects, is a weakness.
- The presentation could use more detail on the education components. There is no doubt that education was provided, but more information on the 101 course and results should have featured more prominently.

Specific recommendations and additions or deletions to the work scope

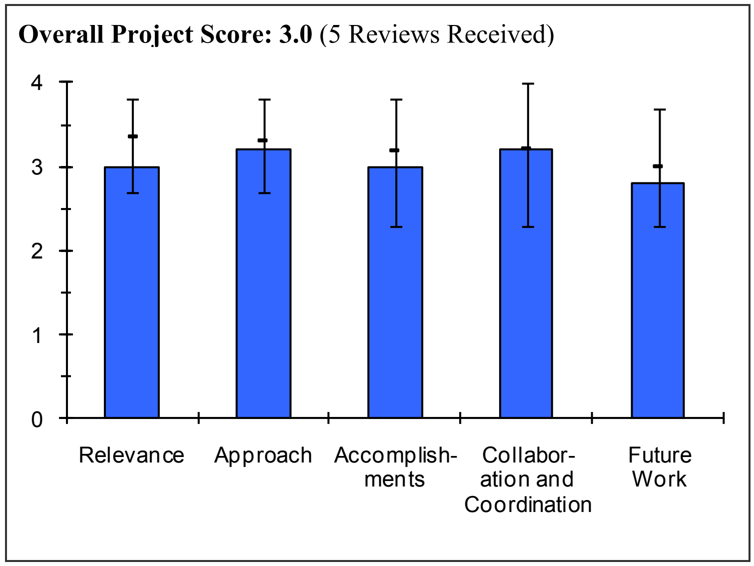
- Further continuation of this type of education scope is highly recommended.
- The project should try to capture a little more technical information during the demonstration pilots.
- The educational message should be reviewed and refined for appropriateness depending on the target audience.

- They should create tailored messages that differ among users (i.e., operators, refueling, etc.), as opposed to a one-size-fits-all message.
- With the amount of data reporting from the DLA pilot and the ARRA projects being delivered during FY 10, it may be strategically advantageous to have a conduit for a targeted public outreach effort.

Project # ED-09: Hydrogen Education in Texas

David Hitchcock; Houston Advanced Research Center

Brief Summary of Project



The goal of this project is to support the DOE goal of reduced petroleum use, greenhouse gas emissions, and air pollution and to contribute to a more diverse and efficient energy infrastructure by enabling the widespread commercialization of hydrogen and fuel cell technologies. This is done by increasing basic knowledge and awareness of hydrogen and fuel cell technologies among Texas state and local leaders. The project objectives are to: 1) establish communications that reach target audiences; 2) assemble accurate and consistent educational materials and presentations for the Web, workshops and Webinars, and meetings; 3) conduct workshops and Webinars for five major metropolitan areas to reach different regional audiences; and 4) assess how hydrogen knowledge has improved as a result of project activities.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Texas is a big state and a potential big user of hydrogen and fuel cells in the future. The Texas Hydrogen Coalition needs an education effort to complement their objectives.
- Because Texas is known as an oil state, hydrogen education is valuable in this location.
- The project clearly addresses the major DOE educational barriers of information availability and dissemination. It also addresses barriers at the state and local levels.
- The relevance of this project to DOE Hydrogen Program education goals was good. It was targeted at institutions in Texas.
- Educating Texas government leaders and other interested audiences in the state could result in early adoption of fuel cell and hydrogen technologies there. Texas might also serve as a model for other states and regions.

Question 2: Approach to performing the work

This project was rated **3.2** on its approach.

- The PI took advantage of a number of outreach opportunities across the state.
- The project has various methods for outreach and information dissemination to the target audience. It had a diverse audience with a majority of state and local officials at the workshops.
- The approach is logical and thorough. It addresses key stakeholders but doesn't break any new ground. The connection to DOE Clean Cities coordinators is a plus. The project team showed willingness to alter the project approach based on lessons learned.
- The project team is conducting workshops and events targeted at increasing knowledge of hydrogen.
- Multiple venues and face-to-face communication have resulted in, or supported, three fuel cell fork lift deployments.
- Twelve partners have been incorporated. This high number of partners is exemplary.

Question 3: Technical accomplishments and progress

This project was rated **3.0** based on accomplishments.

- There was good information dissemination at a variety of events.
- The project has a small reach but seems to be well received and effective. It definitely grew in several performance measures from 2008 to 2009.
- Having a couple of real demonstrations (busses and forklifts) helps people experience the technology firsthand, which is invaluable.
- The project has only reached 500 people, which seems to be fewer than other similar DOE-funded projects. It would have been good to see more people reached, especially in such a large state. The project has helped encourage three hydrogen projects in the state, which is good.
- The project has addressed previous concerns about performance measures. The measures identified are appropriate and show progress.
- The project has hosted workshops in four main Texas metropolitan areas with a diverse set of workshop attendees. The project measured the knowledge base of participants.
- A significant number of people (500) were exposed to hydrogen activities at the workshops and events.
- Participation in project activities may have been negatively effected by the recession and by the lack of government support for the development of a hydrogen infrastructure.
- Efforts by a good number of partners and presentations, has been able to reach approximately 500 people in 2009. This is good performance.
- The majority of presentations have been given at events and venues where “clean” or “green" audiences might be present. It could be helpful to target some venues where audiences who know less about the technology might be present to broaden the knowledge about fuel cells and hydrogen beyond the "clean/green" audiences.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.2** for collaboration and coordination with other institutions.

- There are good partnerships with numerous Texas organizations and universities, as well as the Gas Technology Institute (GTI) and the National Renewable Energy Laboratory (NREL). This helps with providing quality speakers and information at workshops. Sysco is a crucial industry partner.
- The project is coordinated regionally with Clean Cities and other regional organizations.
- There is collaboration with a wide range of organizations: Texas Hydrogen Coalition, GTI, Texas state organizations, and private companies.
- There is good collaboration with Texas institutions.
- Having 12 partners is excellent and outstanding.

Question 5: Proposed future work

This project was rated **2.8** for proposed future work.

- The project plans for future work are not impressive.
- With several months left in the program, they are laying the groundwork for future education efforts and collaborations.
- The future work looks good. The project is close to the closeout phase.
- Following up with workshop participants will be useful, as will the summary report of lessons learned.
- The project ends in FY 10. The final project activities are reasonable.
- Several additional presentations, tours and interactions are planned.

Strengths and weaknesses

Strengths
- Texas is a very populous state with a need for hydrogen and fuel cell education.

- A lot was accomplished in a short amount of time, which was expanded and improved from the previous year.
- The connection with Clean Cities coordinators and a wide variety of stakeholder types is a strength.
- There is an ability to spur development of hydrogen projects with several organizations.
- The exposure of participants to vehicles and equipment in real-world applications is a strength.
- The focus on Texas institutions is a strength.
- The high number of partners and presentations given is a strength.

Weaknesses

- None.
- There is small attendance. It would be good to maybe expand the audience to the oil and gas crowd in Texas.
- There is a limited reach of project workshop attendees, but this may be because of the economic climate.
- None.
- The relative lack of presentations to audiences that are not "clean/green" might be viewed as a weakness.

Specific recommendations and additions or deletions to the work scope

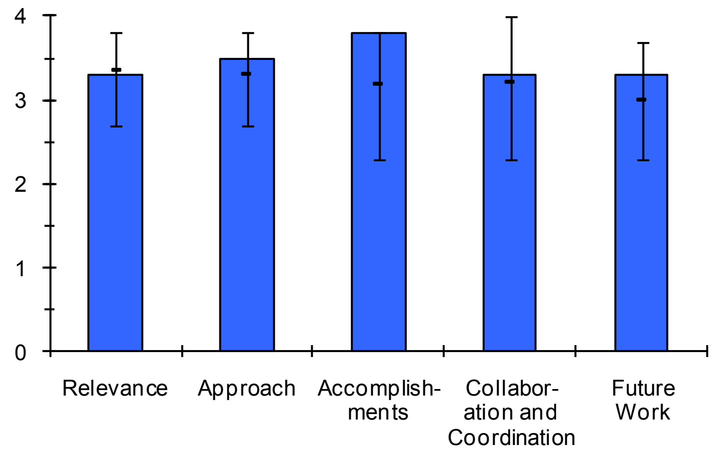
- The project should show how to implement similar programs in other states, such as describing best practices.
- Giving additional presentations to audiences that are not knowledgeable on fuel cell and hydrogen technologies could be helpful.

Project # ED-10: Development of Hydrogen Education Programs for Government Officials
Shannon Baxter-Clemmons; The South Carolina Hydrogen and Fuel Cell Alliance

Brief Summary of Project

The goal of this project is to accelerate the on-going construction of the hydrogen economy in South Carolina and the Southeast by providing accurate and reliable information to state and local decision makers. The objectives are to: 1) identify key messages for decision-makers, 2) develop varying presentation formats based on time available, audience interest and technical level, 3) develop Webinars for state and local government officials, 4) give hydrogen (hydrogen 101) presentations to a variety of stakeholders, 5) give monthly Webinars for interested stakeholders, and 6) collect feedback and improve presentations.

Overall Project Score: 3.5 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The project is educating and working with key South Carolina groups, which has resulted in demonstrations, installations, legislation, and permitting.
- The project is relevant to hydrogen education, because it addresses messaging issues. It is working to provide essential information to decision makers in the Southeast.
- This project is critical for reaching state audiences in South Carolina, and it's important for DOE to support these efforts because of their interest in hydrogen and fuel cells. Also, interest from other states helps to show nationwide support that provides validation for continuing the federal program. This is one of the key areas to introduce new technology, so it supports market transformation.
- Persuasive communications with state and local stakeholders, as well as businesses, can result in easier permitting, more state and local government adoption, state and local incentives, adoption of hydrogen and fuel cell technologies by businesses, and decreased risk to hydrogen and fuel cell developers.

Question 2: Approach to performing the work

This project was rated **3.5** on its approach.

- The project is completing more Webinars and presentations than scheduled.
- The project is developing materials to reach key audiences resulting in crucial legislation to help advance commercialization and installations.
- The focus on reaching decision makers is useful. It removes barriers to implementing hydrogen projects. The project is getting to the right people with targeted outreach.
- It brings the DOE message to the audience in a tailored fashion that will help these audiences to better understand the basics of fuel cell and hydrogen technology.
- Responding to evaluation feedback improves the program.
- The project is also doing more advanced education projects.
- The multi-pronged approach in making quality connections through in-person interactions coupled with the utilization of the media is smart and effective.
- The approach is very active and energetic with more meetings and presentations accomplished than originally planned. This is laudable.

- It may be helpful to state and repeat the essential messages. The central, key message for each target audience should be stated repeatedly during the presentations to improve the focus.

Question 3: Technical accomplishments and progress

This project was rated **3.8** based on accomplishments.

- The project is exceeding targets and conducting more Webinars and meetings than planned. It is reaching key stakeholders and branching out to more interested parties.
- The project includes meetings with political and environmental groups in South Carolina and educating at grassroots and local levels.
- The project has reached more than 1,500 decision makers and many other stakeholders as well, including 60% of the state's government councils.
- The project exceeded expectations for the number of meetings to be conducted.
- The South Carolina Hydrogen Permitting Act pushes permits to the state fire marshal's office, which maintains leadership for the state in hydrogen. This program appears to have had a role in this.
- This project is addressing the social networking aspect as well.
- The project team is taking feedback from its audiences seriously and making changes as best they can.
- There are a lot of other tools and resources. The availability of case studies for businesses will be particularly useful.
- The project team presented great metrics that measure how much outreach is happening. It's wonderful to see this project exceed its goals by so much.
- Having monthly Webinars is an excellent approach. Maintaining a consistent and persistent schedule should provide opportunities to reach out to the maximum number of participants. Keeping up the schedule may be essential to ensuring that all who wish to get the information can access it, and it allows prior participants to encourage others to attend.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.3** for collaboration and coordination with other institutions.

- The project team is working with state and local officials, universities, and organizations. It also works with national groups' universities in other states, as well as fuel cell groups.
- The project has very good partners in South Carolina.
- The coordination is very good. It's mostly focused within South Carolina, which is good for the project. However, other groups would benefit from knowing more about the basic strategies that are being employed. Those strategies can be replicated in other states or even at the national level using tactics such as the combination of in-person and media outreach combined with newsletters and other communications.
- Additional collaborations could be helpful and could include the state's public utilities commission, more utility companies, chambers of commerce, local labor groups, local environmental groups, schools and universities, among others.

Question 5: Proposed future work

This project was rated **3.3** for proposed future work.

- This is a very effective program.
- The project still has one more year to go, but future work was not specifically identified.
- This part may have been left out of the presentation, but if the PI is planning to continue outreach efforts as currently planned, that would make sense.
- The PI should continue the existing schedule while expanding the contacts to complete the project.

Strengths and weaknesses

Strengths
- There are tangible results with jobs, business attraction, and legislation.
- The project is building on leadership from the state in hydrogen technology, which pushes momentum forward.
- There is a great list of resources.
- The project has focused outreach efforts.
- There is a comprehensive strategy for outreach: in-person interactions, electronic media, and mainstream media like television.
- The project has numerous contacts with interested parties and decision makers.
- The project's handouts are a strength.
- The project's Web site is a strength.
- The broad scope of audiences and of information, case studies, payback evaluations, and similar persuasive materials is a strength.

Weaknesses
- It was unclear whether there is any connection between this hydrogen lift truck education project and the LiftOne education project. There appears to be some overlap in the two audiences. More coordination wouldn't hurt.
- More information on future work is needed in the presentation.
- None noted.
- It could be helpful to add more partners.
- It could also be helpful to focus more on the key messages for the different audiences and repeat them consistently. Since the project has such a broad scope of audiences, this sort of message management might be very helpful.

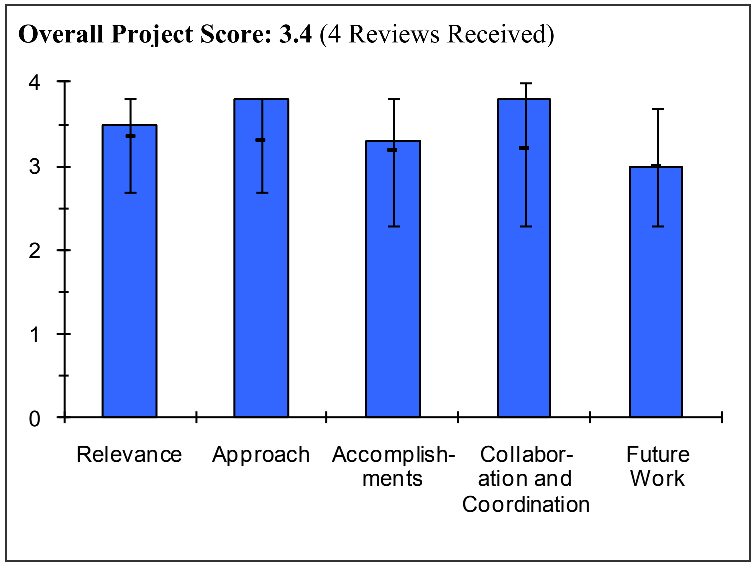
Specific recommendations and additions or deletions to the work scope

- It would be good if the project worked with other states to show how they succeeded in changing legislation and permitting and to share best practices.
- There is an opportunity to share strategies and messages with other groups doing similar work.
- More partners might be helpful.

Project # ED-11: VA-MD-DC Hydrogen Education for Decision Makers

Chelsea Jenkins; Commonwealth of Virginia

Brief Summary of Project



The goal of the project is to increase a targeted audience's understanding of hydrogen and fuel cells, including early market applications, and to provide specific examples of actions that the targeted audience (state and local government leaders) can take to support the development and use of hydrogen and fuel cell technology leading to better understanding of the community benefits. Objectives are to: 1) conduct up to a dozen in-person workshops by technical experts and professional educators; 2) produce video resources for public television, seminar use, the DOE, and the general public; 3) use hardware demonstrations when possible and provide real-world examples of the technology; and 4) produce electronic magazine-like articles on hydrogen technology demonstrations and other instructional project deliverables.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project's goals and objectives are to educate decision makers who will play a key role in the deployment of hydrogen and fuel cell technologies in their respective geographic region. It is very relevant.
- The project has a good objective and key target area. It is reaching a wide audience through various media avenues.
- The project is very relevant to hydrogen education goals and objectives by reaching a broad audience with consistent messaging.
- It is extremely important for decision makers, especially in this region, to have a reasonable understanding of the technologies and the issues.

Question 2: Approach to performing the work

This project was rated **3.8** on its approach.

- This project is associated with the Virginia Clean Cities program and involves Maryland; Washington, DC; and Virginia. The approach is based on presenting workshops for decision makers that include equipment demonstrations (such as ride-and-drives) and gives the participants an important, hands-on experience.
- The project is using new media to reach younger and more computer-savvy people.
- The videos and Webinars provide great resources.
- The approach is very reasonable and incorporates some unique features. Video products are broadly useful.
- The project team has incorporated new social media efforts as part of its program.
- Ride-and-drives and other demonstrations are a plus. They show the technology working in the real world.
- This group has a plan to put together many worthwhile activities and plenty of relevant educational material.

Question 3: Technical accomplishments and progress

This project was rated **3.3** based on accomplishments.

- The project has made good progress having hosted 12 workshops for regional representatives. The informational videos made with MotorWeek are excellent!
- The video is excellent. It covers all applications and doesn't sugar coat. Lots of real-world applications and footage really make the case.
- The project team has made very good progress working toward all key milestones. Careful resource management will reap benefits to complete additional work.
- The curricula are targeted and well received.
- The project team has made efforts to verify that the audience's understanding improves. It is good to immediately prove the effect of the work.
- The MotorWeek videos will reach audiences well beyond the three states in this project.
- The team's progress has been very good. It is probable that issues beyond its control have restricted some of the progress. The video the team produced was very good, but it seems that distributing a few hard copies of its newsletter would have had value as well. The team didn't hand out any newsletter copies.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.8** for collaboration and coordination with other institutions.

- This project exhibits outstanding collaboration with many appropriate organizations: state, local and private sector.
- The project has good partners within the DC metropolitan area and in Virginia, including the rural areas and other communities besides northern Virginia.
- The project has very good collaborations (GM, universities, others) and a very large number of collaborators (including local governments and Clean Cities).
- The project team is working with the Los Alamos National Laboratory project, which is also funded by DOE.
- The MotorWeek partnership is unique and represents a good crossover to Clean Cities program efforts that will connect to this audience. It will be pushing the information through the Clean Cities network and connecting with other areas (i.e., the Northeast and California) where hydrogen is being used.
- There looks to be some work with the other hydrogen education activities funded by DOE, specifically South Carolina.
- The very nature of this type of project almost insures a great deal of collaboration—and the project team did collaborate.

Question 5: Proposed future work

This project was rated **3.0** for proposed future work.

- Future work plans are to complete the project in September, continuing the activities presently underway.
- There will be more seminars and another video that will be reaching a larger audience.
- The project ran for 12 months. There is some very interesting work to be done, such as trying some more innovative approaches to reach more audiences, addressing some travel issues, and teaming with TV Worldwide to produce do interactive Webinars.
- The student thesis project on scooter build should be a great addition.
- They seem to be putting much of their future plans on more video and a few more seminars. These are certainly worthwhile but, under present economic conditions, a bit more innovation might be helpful.

Strengths and weaknesses

Strengths

- This is an excellent approach featuring multi-component activities.
- The video is huge asset. It needs to be circulated to key audiences outside the scope of this project.

- There is a careful marshaling of DOE resources, and extra cost sharing is a plus.
- Video resources are very interesting and can be widely used outside of the project area.
- They have lots of connections outside their project area. Clean Cities can be a good outreach tool.
- There are impressive efforts by this group for relatively small funding.

Weaknesses

- None apparent.
- None significant.
- None worth noting.

Specific recommendations and additions or deletions to the work scope

- Keep up the good work!
- The project should partner with other states to share best practices and share video and/or Webinar resources.
- It would be good to see more information on the curriculum development, although time limitations may have prevented this. It may be helpful to bring some samples next year.
- The project team should try to develop a few innovative approaches in addition to the more obvious. Also, it might be of value to have a few hard copies of the newsletter available for selected distribution.

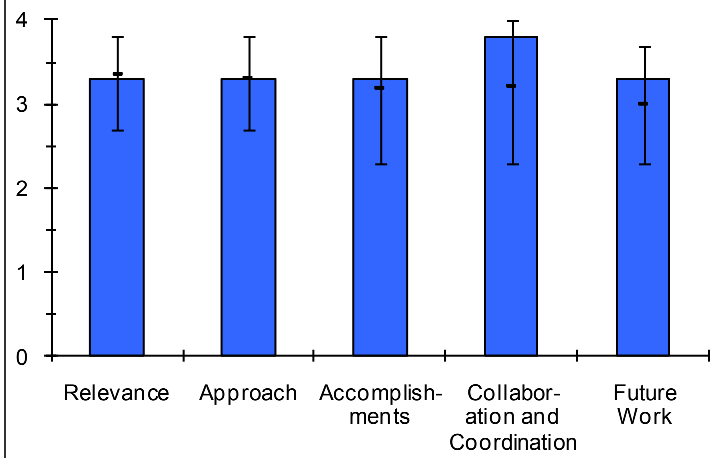
Project # ED-12: State and Local Government Partnership

Joel Rinebold; Connecticut Center for Advanced Technology, Inc.

Brief Summary of Project

The primary objective of this project is to build and strengthen partnerships between the hydrogen community and state and local governments. The partnership building project has five components: 1) identify key stakeholders and expand and strengthen partnerships, 2) develop resources to analyze potential sites for hydrogen and fuel cell deployment, 3) educate state and local decision makers and other key stakeholders, 4) integrate state and local development plans with federal and/or DOE objectives, and 5) identify financial and investment opportunities.

Overall Project Score: 3.3 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This project is relevant in its focus on the education of key stakeholders in Connecticut.
- This project is strongly linked to the goals and barriers that needed to be addressed by the education program.
- The project is relevant to addressing the hydrogen education barriers for state and local governments and for communities in Connecticut. It focuses on a state with heightened interest in hydrogen technology.
- With several of the major fuel cell firms in Connecticut, the state is already generally more aware and supportive of fuel cell technologies than most others.

Question 2: Approach to performing the work

This project was rated **3.3** on its approach.

- The approach of providing educational opportunities for key stakeholders who make decisions regarding the deployment of fuel cell technologies in respective areas of responsibility is sound and responsive to the projects goals and objectives.
- The focus on deploying projects that will be successful, and being an honest broker when hydrogen technologies will not work, is critical to the success of early deployment.
- There is a logical five-step approach to address barriers: identifying the audience, developing resources for analysis, educating stakeholders, integrating local plans with national DOE objectives, and locating financial incentives. The approach is thorough and carefully planned.
- Presentation slide 5, Approach and Milestones, looked good, but it is not clear that these efforts can be effective. For example, the issue of potential environmental benefits is extremely complex It involves things such as how the fuel is produced and transported, whether there is (or will ever be) carbon capture, etc. The same is true for economic issues. They are very complex and not subject to simple models.

Question 3: Technical accomplishments and progress

This project was rated **3.3** based on accomplishments.

- The approach of presenting workshops/seminar tailored to fit the specific audience, whether it be state legislatures or regional or local officials, is sound. An interesting variation in this project is the inclusion of

combined heat and power opportunities (considering both the advantages and disadvantages) and presents a full spectrum of fuel cell deployments to potential users.
- Outstanding progress is being made toward the stated goals. While some may criticize the use of models for fear of misuse, what this project has done in making appropriate tools available to targeted audiences is an excellent approach, particularly the focus on the bottom line of cost and cash flow. If these projects are not successful financially, the entire industry will suffer. The focus on the financial success and the willingness to say no to situations where these technologies will not work is outstanding.
- A unique aspect is the project benefit tool that is used to calculate the benefits of implementing hydrogen fuel cell projects such as return on investment, renewable energy credits, performance, etc.
- There is a wide range of educational efforts at various state and local venues to reach appropriate audiences.
- The project provides a connection of hydrogen and fuel cells with local and state energy plans. For example, they are working with the Connecticut Department of Transportation on a statewide hydrogen fueling and vehicle plan.
- It is good to hear that their analytical tools allow them to analyze possible projects and make the right fit. They can avoid giving organizations the go-ahead for projects that will not be successful.
- Mostly, the project seemed to have developed resources from which to draw information and assistance. How are the messages actually conveyed? How do they present cash flow analyses, and to what groups? What was the result of working with the DOT to develop strategies for vehicle deployment and refueling?

Question 4: Collaboration and coordination with other institutions

This project was rated **3.8** for collaboration and coordination with other institutions.

- The project has many collaborations and interactions with federal, state, regional, and local decision and policy makers. Bringing focus on integrated opportunities is very valuable.
- The numbers and types of collaborations are very appropriate for this project.
- There is focus on state and local partners. They have worked with a great many organizations in Connecticut, and the Connecticut Center for Advanced Technology appears to have very good connections within the state. Utility partners are a good addition.
- There are clearly many contacts with other organizations, although probably most are not true collaborations.

Question 5: Proposed future work

This project was rated **3.3** for proposed future work.

- This project is 85% complete and scheduled to end in August 2010. The project team's plans to complete the project include reinforcing contacts already made and extending the activities to additional contacts, which are reasonable and proper actions.
- This project is scheduled to end this year, and the team is on track for a successful conclusion.
- Closeout work should be useful such as regional briefings and conducting a post-survey for measuring the success of the program.

Strengths and weaknesses

Strengths
- The project has a sound approach.
- This is a really good project. It is unfortunate that it is finishing up without an obvious continuation project.
- There are thorough planning efforts.
- The project has employed advanced online analysis tools to assist with planning hydrogen projects and communicating their benefits to the local communities.
- The project team is being realistic with local communities and only encouraging projects where they make sense.
- This is an area where additional decision-maker education about the issues could be very useful.

Weaknesses

- None apparent.
- None.
- There is no actual project deployment yet.
- The project PI seems to be trying to do too much, which probably contributes to sub-par performance in some areas.

Specific recommendations and additions or deletions to the work scope

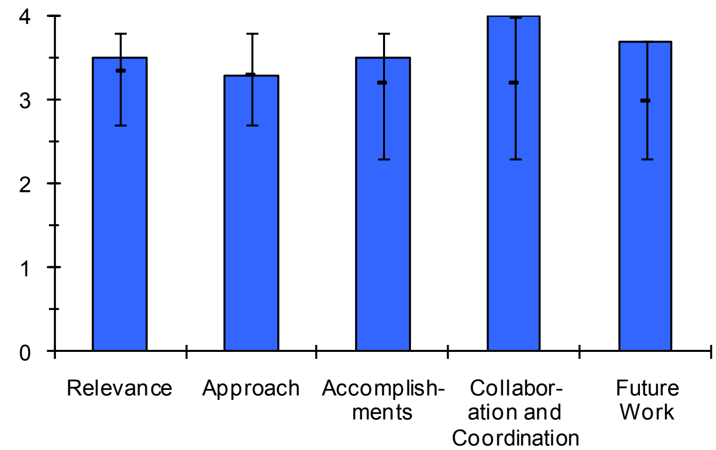
- The project team should continue making models for comparative analysis and municipal energy analyses available to potential early adopters.
- A general comment on the educational sub-program element: Education to all segments of society regarding hydrogen technologies is very important for the acceptance of these technologies, particularly if we want to accelerate the adoption. There is concern that funding for this program element is being sacrificed, which hinders visibility. Granted, the overall budget for hydrogen education needs improvement, but even in tight budget years, an increased focus on education is warranted particularly if we want to accelerate public acceptance of these technologies.

Project # ED-13: Raising H2 and Fuel Cell Awareness in Ohio
Pat Valente; Ohio Fuel Cell Coalition

Brief Summary of Project

The overall objective of the proposed program is to increase the awareness and understanding by state and local government officials in Ohio concerning hydrogen and fuel cell technology. The goal is to accelerate the deployment of clean energy solutions that will better the environment, decrease dependence on foreign energy, and bolster the manufacturing sector. This increase in the awareness and understanding will directly contribute to these Hydrogen Education Sub-program objectives: 1) by 2009, increase the understanding of hydrogen and fuel cell technologies among state and local governments by 10% compared to the 2004 baseline, and 2) by 2012, increase knowledge of hydrogen and fuel cell technologies among key target populations (state and local governments) by 20% compared to 2004 baseline.

Overall Project Score: 3.5 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The goals and objectives of this project to increase awareness of hydrogen and fuel cell technologies in Ohio with potential early market segments are very relevant.
- Outreach to the targeted audiences, including elected officials, economic developers, transit operators, utility companies, is critical to the successful deployment of these technologies.
- This project is highly relevant in a state with a need for new industries and technology.
- This seems to be an excellent effort, but naturally, it is confined to the state of Ohio.

Question 2: Approach to performing the work

This project was rated **3.3** on its approach.

- The approach involves serving as part of the Ohio Fuel Cell Trade Association, which is a sound plan with a focus on informing elected officials and promoting economic development.
- The design of specific forums to target the appropriate audiences is exactly what is needed. A one-size-fits-all solution is not appropriate and is not used in this project's approach.
- The forum approach is good and can reach a large number of interested people.
- While it's very good plan, it is apparently confined to forums involving targeted audiences. There are many other avenues that could be used to convey better understanding of hydrogen and fuel cells.

Question 3: Technical accomplishments and progress

This project was rated **3.5** based on accomplishments.

- The project team has been making good progress based on a large number of informational meetings that have reached targeted audiences totaling approximately 1000 interested participants. The emphasis on matchmaking

potential funding sources with early users is very good. The project has been very successful in getting state funding for developers and users of fuel cell technology.
- Initial target goals have been exceeded in some areas and are on track to be achieved in other areas. The project team initially planned for nine forums and has to date held 16 forums, which is excellent.
- The project team has exposed a significant number of people through the forum approach.
- Developing databases to integrate interested people with fuel cell capabilities in Ohio is good.
- Relative to the funding involved, the group has accomplished a great deal. The group has held more than the originally planned number of forums and has apparently been very effective in conveying its message to those it has reached.

Question 4: Collaboration and coordination with other institutions

This project was rated **4.0** for collaboration and coordination with other institutions.

- There is much collaboration including state and federal agencies, along with state, regional, and local interested participants, which is impressive. It is noted that the involvement of federal laboratories in Ohio was an excellent idea.
- The collaborations around Ohio are very impressive.
- The project team exhibited very good collaborations with key Ohio organizations.
- The project team has effectively interacted with and collaborated with many diverse and important groups.

Question 5: Proposed future work

This project was rated **3.7** for proposed future work.

- Plans for completion of this project appear to be reasonable and proper, and the project team builds on the sound approach utilized in this project to date.
- The project appears to be on track to finish, having met or exceeded all its proposed goals and targets. It is performing beyond expectations.
- The project activities may continue from other funding sources after the DOE funding ends.

Strengths and weaknesses

Strengths
- This is a sound and innovative approach.
- The project demonstrated expertise in fuel cell and hydrogen technologies.
- The use of focused forums to educate the public is a very powerful, cost effective, and a successful way to increase education and awareness of these technologies. This project has been quite successful in meeting its stated goals. The PI admits that at least one of the initial goals, economic development, may have been a bit too aggressive for the project team to meet. However, the success in other areas of this project far outweigh missing the aggressive target set in one area.
- There is strong commitment from the state of Ohio.
- The project is apparently well organized and has done more than planned, with the exception of the total number of contacts made to date.

Weaknesses
- No weaknesses are apparent.
- None.
- None.
- The project team seems to have relied almost exclusively on forums. The forums seem to have been very effective but other approaches, such as informational videos, could have added to the overall effectiveness and probably would have allowed many more people to have been reached.

EDUCATION

Specific recommendations and additions or deletions to the work scope

- It was good to hear that these forums will continue beyond the life of this particular project. DOE needs to continue these successful educational outreach activities, particularly now because hydrogen technologies are just beginning to take hold in the energy and transportation sectors. To lose momentum now will result in a significantly decreased rate of deployment. Indeed, the number of skeptics may increase, which will be devastating. As part of the executive branch, DOE needs to work with the White House to continue these educational outreach activities that are critically needed to accelerate the deployment of hydrogen technologies.

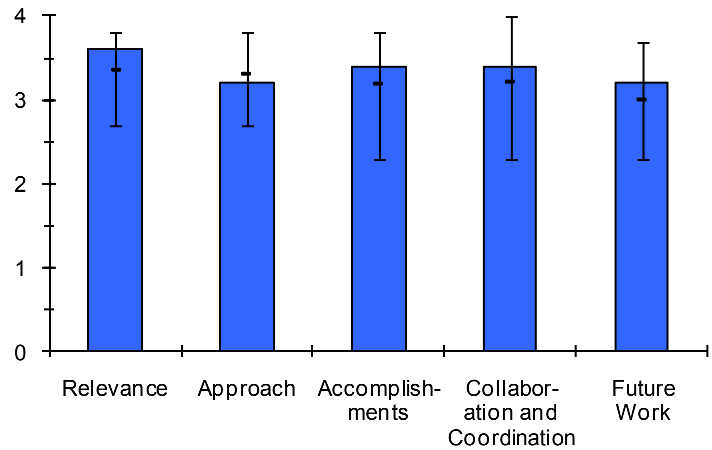
Project # ED-14: H2L3: Hydrogen Learning for Local Leaders

Patrick Serfass; Technology Transition Corporation

Brief Summary of Project

The objectives of this project are to: 1) create presentation materials tailored to effectively communicate with state and local government leaders, relating hydrogen to their interests and spheres of responsibility, 2) establish pathways for working with national associations of state and local officials as a route for disseminating information about hydrogen and setting a pattern for ongoing flow of information, and 3) hold learning sessions through workshops for local and state officials at national gatherings and achieve nationwide reach.

Overall Project Score: 3.4 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- This project is helping DOE overcome the barrier of disseminating good information on hydrogen technologies to the national, state, and local thought leaders.
- The content of this multi-dimension project plan to educate the public on hydrogen and fuel cells is critical to DOE objectives.
- The project addresses key barriers of information dissemination and education. It addresses the lack of information resources and mixed messages, specifically to local leaders. They work with information dissemination networks. The national focus is very relevant to DOE objectives to build coherent messages.
- The project is a solid provider of key information to decision makers and policy makers.
- Education for local leaders can help encourage hydrogen deployments. The idea is to encourage incentive funding and early adoption of hydrogen technologies by local leaders. This can help to jump start the commercial market.

Question 2: Approach to performing the work

This project was rated **3.2** on its approach.

- The project uses good mechanisms for disseminating information.
- Working and talking with local leaders are good ways and means to reach their goals.
- The project team's use of workshops and Webinars is an effective way to reach target audiences.
- The U.S. Market Report (www.hydrogenassociation.org/marketreport) is an excellent report that has been downloaded 30,000 times.
- Working with the Public Technology Institute (PTI), a national organization, was a good approach. The catch phrase "H2L3: Hydrogen Learning for Local Leaders" is good and allows a train-the-trainer approach for education dissemination. Engaging the National Association of State Energy Officials (NASEO) is also effective. Working with as opposed to "talking at" is a novel and effective approach in communicating with the target audiences. Telling more stories is an effective education approach as well. The Hydrogen Student Design Contest and Business Solutions Forum were both good ideas, too.
- Working with state and local leaders using peer presenters is a good approach using peer presenters. Relating hydrogen information to the audience (peers talking to peers) is important for this audience. The project team is also utilizing national organizations to reach local leaders.

- The project is outstanding in terms of collecting and reporting industry information and with its relationship with NASEO, which is an important stakeholder. However, the effectiveness of the delivery of services tied to the National Hydrogen Association (NHA) meetings, where attendance at those forums has been low, is questionable.
- The approach appears to involve encouraging networking and collaboration, mostly among those interested in hydrogen technologies.
- The learning sessions seem to occur mostly at hydrogen or energy events, not at events where local officials unfamiliar with hydrogen will be exposed for the first time. It might be better to make presentations at national associations of mayors, council members, city managers, and auditors, for example, rather than high technology venues where the messages won't be new.
- The use of peer presenters is generally effective. This is a strength.
- The organic approach is non-conventional and potentially risky. Typically, outreach opportunities would start with a presentation and move into the organic phase. Skipping the presentation could be confusing to the audience.

Question 3: Technical accomplishments and progress

This project was rated **3.4** based on accomplishments.

- The Hydrogen Student Design Contest was a huge success.
- The Hydrogen Business Solutions Forum at the NHA conference was successful.
- The Hydrogen Student Design Contest winners went to the World Hydrogen Energy Conference.
- The fact that Sandy Thomas supports the U.S. Market Report is significant, since he has provided some excellent analysis for DOE in the past.
- The project team continues to think outside the box and get the public involved.
- Each one of the efforts turned out to be quite effective, with the exception of the Hydrogen and Fuel Cells Business Solutions Forum. The latter project suffered from sending notification to potential attendees too late. The PI blamed that on inability to lock in speakers early enough. While partially true, it is common practice to write "invited" next to a prospective speakers name when to enable prompt publicizing to prospective attendees.
- Telling stories is a good way to reach the audience as opposed to relaying dry technical data. Tailoring the curriculum for specific audiences is important.
- The project developed a fuel cell market report with extensive information on the hydrogen industry, which was endorsed by NHA. It is a good resource that is downloaded frequently and well received.
- Their informal networking activities that develop personal connections to disseminate information is a novel approach.
- Communicating with university students through a student design contest reaches the next generation of hydrogen technology users.
- Their participation in some social networking sites can be particularly successful in disseminating reports and receiving comments.
- Some other educational sub-program projects used Webinars periodically and more frequent meetings to increase the number of interactions with audiences. More presentations and interactions such as Webinars and other ways to communicate could improve this project.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for collaboration and coordination with other institutions.

PTI is a good partner.
- The California Fuel Cell Partnership has been helpful.
- NASEO is an essential partner in this project.
- They continue to ask anyone who is interested to participate, and they are convincing some to join the cause.
- There is great collaboration with a number of activities. The student design contest was particularly effective. Working with as opposed to than talking at the intended audience is also effective outreach. Talking with local leaders helps to get the message out to a broader audience from people the locals know and respect.

- The project is partnering with PTI for strong outreach activities to local organizations. They are also working with NASEO.
- There are good partners in terms of their collaboration with PTI, the Schatz Energy Research Center, and NASEO. It is also beneficial to be able to easily access the Technology Transition Corporation (TTC) organizations.
- The lack of active partners may be holding this project back. Active partnerships with associations comprised of local officials may help and is encouraged.

Question 5: Proposed future work

This project was rated **3.2** for proposed future work.

- It is important to finish their future work. They are halfway through their outreach program.
- It's not certain that lessons learned were discussed for potential future work. Having said that, many of these techniques should be considered in the future.
- Future work builds on the activities by the core of local H2L3 leaders. The activities seem reasonable
- The Webinar-based educational effort is good in reaching a lot of people at a low cost.
- It seems like most of the future work is follow-up to the previous work. It might be helpful to hold more events at a wider range of venues for more exposure to new audiences.

Strengths and weaknesses

Strengths
- The project has good partners, especially PTI and NASEO.
- The project has a good focus for national and local leaders.
- Working with local leaders (H2L3) is a strength.
- The student design contest engages young participants.
- The relationship with PTI improves messaging.
- Having a strong connection with NHA through TTC, the contractor that operates the NHA, is a strength.
- The project employs some novel ways of reaching intended audiences through networking events and social networks.
- The extensive data report is a great asset.
- The ability to collect and present relevant information to both national and local interests is a strength.
- The market study is a good resource.

Weaknesses
- None.
- There was low attendance at the Hydrogen and Fuel Cells Business Solutions Forum.
- The project should ensure that the activities are reaching the right audiences and avoid "preaching to the choir" if possible.
- The NHA workshops have been spotty in terms of the ability to attract a large number of people.
- The project does most of its communication to audiences who are already familiar and probably supportive of the technology. Expanding this to new audiences would be an improvement.

Specific recommendations and additions or deletions to the work scope

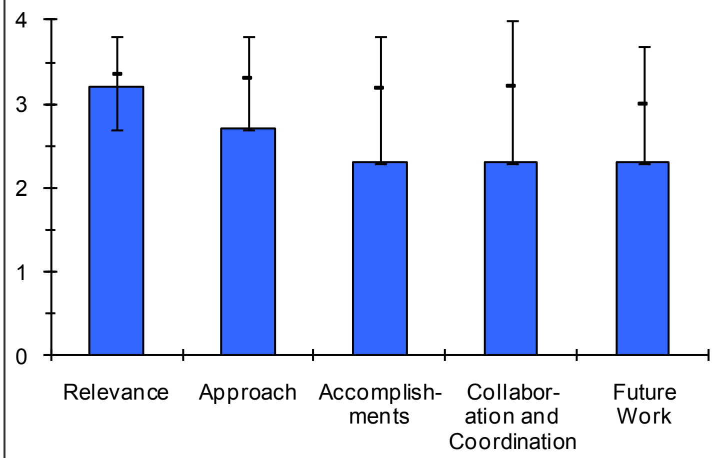
- Expanding the university courses in hydrogen and fuel cell technology may be a good step.
- A Hollywood or sports star image supporting hydrogen would be good to encourage.
- An annual drive across America with the DOE Clean Cities Program support would be a good idea, ending with an event at the Capitol steps. Dennis Weaver used to organize this.
- Encourage the project team to continue with the hydrogen market report for 2009 if funding is available.
- It could help the project to expand to new venues and to new audiences who are not as familiar with the technology.

Project # ED-15: Hydrogen Education State Partnership Program
Charles Kubert; Clean Energy States Alliance

Brief Summary of Project

The objectives of this project are to: 1) identify state hydrogen program best practices and policies, 2) provide information and technical assistance to state policy leaders and state renewable energy programs to foster the development of effective fuel cell programs, and 3) promote strategic opportunities for states and DOE to advance fuel cell deployment through partnerships, collaboration, and targeted activities.

Overall Project Score: 2.6 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The CleanEnergy States Alliance (CESA) writes reports for dissemination to policy makers.
- It is unclear how the reports are placed into the hands of the policy makers.
- The project is relevant to DOE interests because it proposed to coordinate state and regional efforts on a national scale. Delivery unfortunately was lacking.
- DOE isn't doing this type of work, so it helps promote technology and advancements to key audiences.
- It is strategically important to have a communications dialogue that can reach a number of key states that are well-positioned to be leaders in terms of shaping energy policy.
- The focus is on promoting education at the state institutional level.
- Making presentations to state officials regarding hydrogen technologies can help to persuade other states beyond California, Connecticut, Massachusetts, and New York to adopt incentives and programs in support of fuel cell programs.
- Coordination and collaboration among states with hydrogen programs can also help them to solve problems using resources available within that network. This can improve the efficiency and effectiveness of individual state programs through cooperative efforts.

Question 2: Approach to performing the work

This project was rated **2.7** on its approach.

- Only three states (California, Connecticut, and New York) are funding fuel cells. This is a limiting factor.
- Only three reports have been written so far.
- Targeting policy makers makes sense, but there was little evidence that it was done to any significant degree. The CleanEnergy States Alliance is a sound approach to nationwide education dissemination, but again, they were short on the details and follow through. Engaging with the Department of Homeland Security (DHS) was not well thought out and really out of scope.
- The reports are targeted and needed, but are they reaching the right audience. How are the messages marketed outside of the fuel cell industry?
- While the strategy is sound, the execution and delivery have been spotty at best. The primary weakness is the focus primarily on states with systems benefits charges and primarily on the National Conference of State Legislatures (NCSL). The former is limiting in the project's scope because there are many other state-level programs and policies that can be positioned to advance fuel cells. The latter is limiting because NCSL is not in a position to be a contributing partner, much less a sole partner.

- The project has a multi-pronged approach involving best practices, information and technical assistance, education and outreach, communications, and a focus on critical power.
- The approach of using meetings and venues that are likely to get to new potential early adopters is a strength.
- The publication of brochures is also a strength.

Question 3: Technical accomplishments and progress

This project was rated **2.3** based on accomplishments.

- The project includes development of a list serve and a Web page to link best practices.
- The project team held Webinars.
- The project team wrote three reports.
- The project team identified the best policies at the state level to support hydrogen fuel cells and technologies.
- For the funding provided, even the PI admitted little was delivered beyond three brochures. His response to questions was even more troubling.
- The project seems to be moving slowly. The team devoted two hours of a three-day meeting to tell the state fund managers who aren't investing in fuel cells (all but three) about the benefits of fuel cells. Why were only two hours devoted?
- A fair rating is generous. The summary page highlights the fact that stationary fuel cells continue to be largely unfamiliar to state policy makers.
- The description of accomplishments is not very detailed. There was no discussion of the reports generated or the case studies completed.
- The publication of brochures is excellent and is a strength of the program.
- The concentration on critical loads is viewed as a good approach.
- The flagging of National Electric Code Article 708 is helpful and laudable.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.3** for collaboration and coordination with other institutions.

- The project is partnered with the National Council of State Legislators (NCSL).
- Only California, New York, and Connecticut outreach was done. The PI did not talk beyond the slides to give confidence that a large collaboration effort was undertaken.
- There is a key partnership with the NCSL.
- NCSL is shown as the sole partner. This group could have established stronger relationships with stakeholders who were much better positioned to provide targeted information relevant to fuel cells. The National Hydrogen Association (NHA) and the U.S. Fuel Cell Council (USFCC) are two examples of potential collaborations that would have been much more effective than NCSL.
- There seems to be a reasonable level of collaboration.
- Partnership with the NCSL is viewed as a good partnership that could result in new early adopters.
- Adding other state-level associations as partners might be helpful, such as the National Governors Association and the Council of State Governments.

Question 5: Proposed future work

This project was rated **2.3** for proposed future work.

- Their attempt to get DHS and the Federal Aviation Administration engaged to promote fuel cells is a good idea.
- The project team should encourage state-level policy makers to include hydrogen and fuel cell technologies in their Renewable Portfolio Standard plans.
- The project works with State Energy Program Managers to build awareness and get favorable legislation passed that will support hydrogen fuel cells and technologies.

- The PI gave little confidence that additional funding would be of any use. This project should not continue beyond its contract and DOE should seriously consider re-scoping this effort to get more impact out of it, if possible.
- Case studies and targeted reports are crucial in letting the public know about fuel cell successes and experiences.
- This project needs to push the policy and legislation up because it doesn't want to miss the boat on a lot of the clean energy legislation moving through the states and federal government. Fuel cells need to be included.
- The future work plan looks reasonable.
- Continuing to reach out to organizations and make the brochures available will finish the project.

Strengths and weaknesses

Strengths
- They have a niche group at the state-level with whom they engage, but the results seem few and far between except for the written documentation.
- None.
- Their access to fund managers and state legislatures could really help to move industry in the states.
- Recognition of key stakeholders at state and local levels is a strength.
- None.
- Hard copy brochures are a strength and an exemplary approach.

Weaknesses
- The project needs more results and tangible goods.
- The project is not working with state organizations in an effective fashion.
- The PI is not doing much beyond the three reports. The remaining two case studies in 2011 will likely have minimal impact.
- The project needs to move faster or prioritize tasks to get the most effective ones, policy changes and legislation, out there.
- The project is very weak in terms of execution. For example, this group was not aware that the ARRA included fuel cells in substantial state block grant programs. Not including this in the message delivery is an egregious error in execution. Better coordination and collaboration with DOE and key stakeholders would have mitigated the risk of this happening.
- This project needs better metrics of success.
- More outreach at more meetings would improve the project.

Specific recommendations and additions or deletions to the work scope

- The project should publish the results of the state-level interest on wastewater.
- The project team should investigate Walmart and other grocery stores that are good sources for using fuel cells.
- Teledyne Energy Systems would be good to develop this area of interest.
- It is recommended that the PI review the 2011 plans and re-scope as necessary to gain a broader impact.
- This group was weak last year, and there is no evidence of improvement. They need better coordination with the DOE and with other message-delivery stakeholders such as the NHA and USFCC.
- The project should provide greater emphasis on some key areas of potential, near-term interest to state institutions. It needs to hone in on the highest potential critical facilities for fuel cell backup power.
- More partnerships with state-level associations such as the National Governors Association and the Council of State Governments would be helpful.

2010
Systems Analysis
Summary of Annual Merit Review of the Systems Analysis Sub-program

Summary of Reviewer Comments on the Systems Analysis Sub-program:

The reviewers considered the Systems Analysis sub-program to be essential to the DOE Hydrogen Program's mission. The projects are considered appropriately diverse and focused on addressing technical barriers and meeting targets.

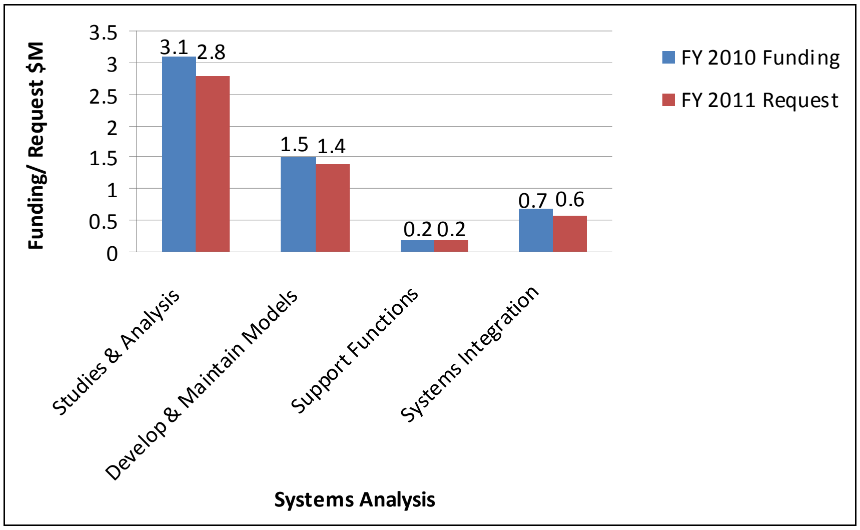
In general, the reviewers noted that the Systems Analysis sub-program is complex. Some reviewers commented that the sub-program is well managed and has adopted an organized approach for analytical support of the Program, which is appropriate for addressing the comprehensive list of barriers identified in the Fuel Cell Technologies Program's Multi-Year Research, Development, and Demonstration Plan (MYPP).

Recommendations identified by the reviewers for Systems Analysis were: 1) a common set of assumptions should be used for analysis projects; 2) analysis projects should identify collaboration; 3) fuel purity analysis should continue, with analysis of cost and performance tradeoffs; and 4) model validation and peer review is critical for sound and credible analysis. The Systems Analysis sub-program will continue to address these issues, and reviewer feedback will be incorporated in the sub-program's future plans.

Finally, the reviewers commented that the analysis and model portfolio was complete and progressing in addressing analysis topics. They stated that the MYPP barriers were being addressed by the Systems Analysis sub-program and put into the proper perspective.

Systems Analysis Funding:

Funding for Systems Analysis has shifted from model development to an analysis focus utilizing the models developed by the sub-program. In particular, analysis projects were concentrated on stationary fuel cell applications, infrastructure, early market fuel cell applications, and support for the Program's technology readiness goal to enable commercialization of fuel cell vehicles. The 2011 request-level funding profile, subject to congressional appropriation, provides greater emphasis on analysis of hydrogen for energy storage and transmission, and on transition, resource, and infrastructure analysis.



Majority of Reviewer Comments and Recommendations:

The maximum, minimum, and average scores by the reviewers of the Systems Analysis projects were 3.5, 2.6, and 3.1, respectively. Reviewers commented that the diversity of the Systems Analysis project portfolio addresses the "analysis and modeling gaps" of the sub-program, and the resource, infrastructure, transition, and early market analysis requirements. The major recommendations for the Systems Analysis projects are summarized below.

Model Development: Projects involved with model development received very favorable reviews. The majority of the projects were regarded as well-aligned with the current program goals and objectives. Reviewers consistently suggested that industry, academia, and the National Laboratories collaborate on model development and participate in the peer review and validation process. Reviewers recommended that models use a consistent set of inputs and assumptions, and that collaboration with industry be increased to ensure models are relevant to commercial applications.

Program Analysis: The analysis projects were ranked from average to good, and reviewers stated that the projects supported the Program's goals. In general, the reviewers concurred that the analysis projects should involve more collaboration with industry to calibrate information with actual operation and experience; analyses should be peer reviewed prior to issue and publication; and a consistent set of inputs and assumptions should be used. The reviewers felt that the Water Analysis project is important for hydrogen production but should be extended to include analysis of other critical issues such as water permitting. The resource and infrastructure analysis with the new Hydrogen Demand and Resource Analysis Tool (HyDRA) was well received by reviewers. The fuel quality for stationary fuel cells project received good reviews; its importance was recognized in addressing fuel cell cost and performance, but it was also suggested that air quality impacts be considered.

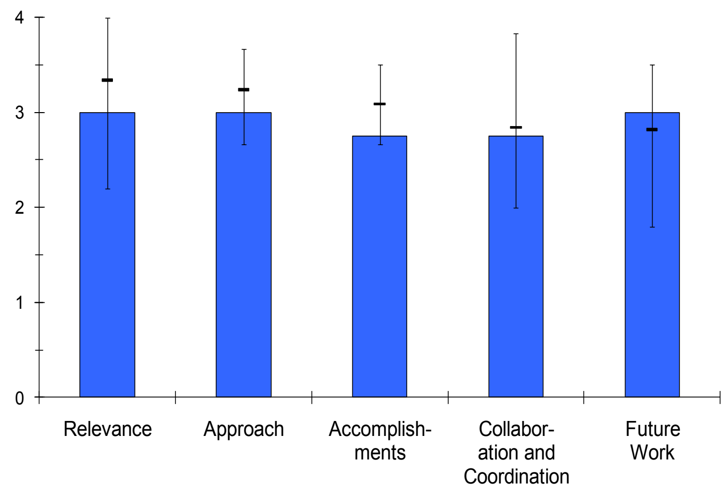
Programmatic Benefits Analysis: Reviewers commented that the Program's commercial benefits analysis project with PNNL, *Pathways to Commercial Success: Technologies and Products Supported by the HFCIT Program*, is a non-research project and should be removed from the merit review program. However, it was recognized that this project made progress and is an essential activity for the Program, providing information about the impact of federal R&D spending. The work provides an ideal communication tool to illustrate the benefits from the Program's research.

Project # AN-01: Infrastructure Analysis of Early Market Transition of Fuel Cell Vehicles

Brian Bush; National Renewable Energy Laboratory

Brief Summary of Project

Overall Project Score: 2.9 (4 Reviews Received)



The Scenario Evaluation and Regionalization Analysis (SERA) model is a tool for studying regional build-outs of renewable energy infrastructures over time by optimizing on the delivered cost of hydrogen. The project's objectives are to: 1) expand the interoperability of SERA with tools such as Hydrogen Demand and Resource Analysis (HyDRA) and import detailed Hydrogen Analysis project (H2A) cost models into SERA and 2) perform various hydrogen scenario analyses. The goals are to: 1) determine optimal regional infrastructure development patterns for hydrogen, given resource availability and technology cost and 2) geospatially and temporally resolve the expansion of production, transmission, and distribution infrastructure components.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The purpose of the project matches the DOE's objectives, but it's not clear whether the output is meaningful or useful based on the input information.
- This project seeks to model an early market for hydrogen transportation fuel, the potential market evolution with time, and its implications on the infrastructure build-out. However, it is not clear how the results of this work are intended to, or had intended to, influence on programmatic decisions. In Q&A, the PI describes the purpose of the work as strategic (for high-level planning) rather than tactical, attempting to provide individualized information to the many actors whose behavior the model seeks to capture.
- The work is aligned to the objectives in the Multi-Year Program Plan (MYPP).
- The project is aligned with the program's objectives, but is not critical to the program's success.
- The project will provide a better understanding of what is required to make hydrogen sustainable.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The technical barriers are more related to the model rather than its actual content. Overcoming these barriers will not result in a better output, but it would allow the computer models to work more smoothly together.
- The PI sees the relevance of the work as integrating the study model, SERA, with other models, such as HyDRA and H2A. This is an approach to the work rather than the end. The approach should follow from the questions that the project is trying to answer. Clarifying the questions the DOE's Fuel Cell Technologies Program needs answered for its strategic decision making could improve the project. On slide 4 of the presentation, the purpose of the work is described as "determining the optimal mix of hydrogen infrastructure … which pathways will provide least-cost hydrogen for a specified demand?" However, one would think that market participants will make these tactical decisions for themselves. It's also not clear how greater spatial detail, mentioned in slide 3, addresses a strategic need.
- The project's staged approach permits more efficient use of resources.

- The infrastructure growth appears to be very similar to the gasoline fueling station growth. That provides a check to indicate that the results are probably reasonable, but at the same time, it raises the question of whether it would have been easier to just look at gasoline station density.
- This project compares the cost of hydrogen produced from a combined heat, hydrogen and power (CHHP) system to the steam methane reformer (SMR) hydrogen production pathway which distinguishes this from the other projects.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- It's not clear that the model is capable of maintaining relevance to the latest technologies. The market is evolving and will constantly shift.
- The project has resulted in studies as listed on the right side of slide 8; however, the results and findings of these studies were not part of this review. These studies also were not delivered to the DOE separately. The presentation showcased the quality of the computer-generated graphics more than the lessons learned.
- The project is proceeding on schedule.
- The forecast of CHHP versus SMR is good and beneficial in understanding the role of CHHP.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Several partners are listed, but the ability of these partners to provide meaningful feedback on the validity and approach of the model in real-world economic conditions is in doubt.
- The project appears to be in close collaboration with the DOE laboratories: National Renewable Energy Laboratory (NREL) currently, as mentioned in slide 18, and Oak Ridge National Laboratory (ORNL) previously, as mentioned in slide 2.
- Additional collaborations from outside of NREL would be appreciated since previous years had more outside collaborations).
- The project needs industrial involvement, such as fuel distributors available to assure decisions are aligned with their logical path.
- The project should be coupled with ORNL's consumer choice model for fuel cell vehicle (FCV) introduction and market growth.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The project seems to address making the model more flexible and useful.
- The proposed future work includes adding features and elements to the SERA model, including CHHP hydrogen production and biogas technology. Notably, they are "iteratively improving the detail and accuracy of the cost models" already in SERA.
- Market competition should be added to the model.
- The future work looks at competition among technologies as being useful, as long as they consider the total cost of hydrogen delivered to the vehicles.
- Work, like developing a vehicle introduction model, appears to be duplicating developments completed by, or being performed by, others. Future plans should be new work or expanding partnerships with others that are already performing the research.

Strengths and weaknesses

Strengths

- As a cost optimization model, SERA can be used to predict the mix of technologies that could be used (e.g., SMR and CHHP distributed generation of hydrogen and centralized production of hydrogen) given different pricing models for infrastructure costs such as capital and operating costs for generation and distribution.
- The project optimization over a five year period is taking into account the (likely) rapid changes in drivers (i.e., economic and environmental).
- The project is looking at technology competition.

Weaknesses

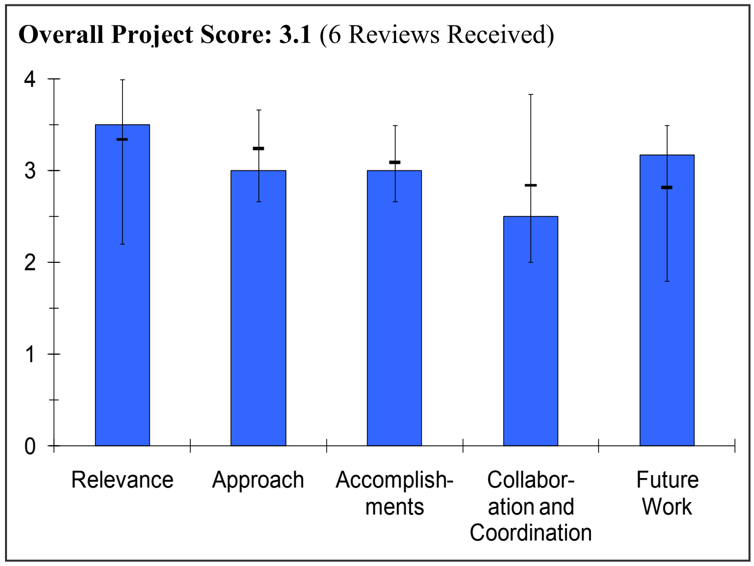
- It's difficult to validate whether the project output is realistic. There are many intangibles that are associated with siting and building fuel stations that don't seem to be taken into account.
- The project seeks to create detailed geospatial visualizations that have uncertain programmatic and decisional significance.
- Changes in supporting models that are used to determine inputs to SERA could make reported results less relevant, and it would not be obvious to the report reader.
- The project needs private sector input, especially for fuel distribution.

Specific recommendations and additions or deletions to the work scope

- It will be challenging to make the nominal economics work for the CHHP. It's also questionable if these stations would be practical for other reasons such as locations. It would be helpful if it was included in the analysis at a low level of effort.
- None.
- An analysis on the basis of a five-year moving window would be beneficial.

Project # AN-02: Analysis of Energy Infrastructures and Potential Impacts from an Emergent Hydrogen Fueling Infrastructure

Andy Lutz; Sandia National Laboratories



Brief Summary of Project

The objectives of this project are to: 1) use dynamic models of infrastructure systems to analyze the impacts of widespread deployment of hydrogen technologies, and 2) identify potential system-wide deficiencies that would otherwise hinder infrastructure evolution and mitigation strategies to avoid collateral effects on supporting systems. Since the transition to hydrogen fueling is expected to rely on distributed steam methane reforming (SMR) and stationary fuel cells (SFC), we must understand the impact of hydrogen vehicles and stationary fuel cells on the infrastructure.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project is useful in determining the most efficient ways to reduce emissions. It can be used to evaluate where the best reductions can be made with the least effort.
- The emergence of new technologies to generate power for industrial, residential, and transportation sectors requires careful analysis of their impact on emissions and potential to reduce consumption of imported fuels.
- The purpose of this project is to develop and use an economic model of energy systems to determine, under different influences of pricing and policy, the evolution of the energy infrastructure and changes in carbon dioxide emissions. By economic, the model attempts to capture the behavior of the public based on market prices. More specifically, the research sought to capture the effects that SFC might have on a generation of distributed power (electricity) and hydrogen fuel for transportation purposes.
- Analysis and understanding of the questions addressed in the project fully supports the DOE's Fuel Cell Technologies Program RD&D objectives.
- The project is relevant and can generate clearly useful information that can be used for policy decisions.
- The project is aligned with program goals.
- The project is well connected to DOE goals.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Although the project has successfully identified the key barrier, the evaluation of future market behavior, that wasn't effectively addressed.
- The project's approach provides a tool to model the impact of different scenarios to predict potential improvements in reducing carbon dioxide emissions.
- The project's approach is to create a detailed systems model of fuel production and consumption and to use it to predict future behavior and market trends. This project is notable for its engineering detail.
- The project's approach is good and sensitive to the audience since the presenter defined all the acronyms.
- The project has a focused methodology, and the analysis is at an appropriate level of detail.

- The SFC penetration model needs to be more realistic. The Smart Grid Interoperability Panel and the impact of cleaner power is important.
- The combined heat, hydrogen and power (CHHP) can support a smart grid and that will be beneficial.
- There should not be any berating of electrical efficiency for CHHP since it, in fact, gets slightly better.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Some of the project evaluation inputs seem questionable, especially the $4/kg hydrogen compared to $4.5/gal gasoline, the combined heat and power (CHP) heat output of a polymer electrolyte membrane fuel cell (PEMFC) , and reliance on distributed SMR generation.
- Significant progress has been made in modeling the impact of scenarios for the California market that relies on natural gas (NG) for electricity. Studies indicate the implementation of SFC in the California market has a limited impact due to strict California legislation. Other technologies, assuming they are successful, will compete against SFCs.
- Preliminary studies in other markets that rely heavily on coal to produce electricity show a bigger impact of SFCs to reduce emissions.
- There were several presentations this year on simulations of the effect of distributed hydrogen generation using CHHP. This project presentation stood out owing to its polish. Slides 12-19 indicate excellent progress toward their objectives.
- The project progress is acceptable, and perhaps more validation of the model results would be useful.
- The project is progressing according to schedule and presents good expansion of previous work.
- It's not clear if the hydrogen station is operated for public use.
- It's questionable whether heating or chilling could be sold when power and hydrogen are already sold.
- It's not clear whether the analysis would change if hydrogen is used for a smart grid to provide peak power.
- Criteria pollutant impact from fuel cell (FC) versus other options should be considered.
- The assumption of 85% hydrogen utilization is very high.
- The electrical efficiency reduction to 40% is not realistic since the actual efficiency should increase to more than 50%.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- There has been little interaction with other institutions so far. However, it is noted that involvement with a utility partner is scheduled for next year.
- Little or no discussion of collaborations was provided. Future work will explore working with NREL.
- The presentation did not discuss collaborations beyond participation in a workshop and reference to a potential utility partner in fiscal year 2011.
- They had a good response to last year's comments.
- Interactions with others doing similar or related work continues.
- There seems to be no collaboration with other organizations.
- The project could benefit from potential collaborations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- Future activities on coal-burning regions are critical to highlight the potential impact that SFCs could have by displacing high carbon dioxide emitters.
- Future work includes tighter coupling between the PI's model and other models (e.g., NREL's fuel cell power model). However, investigating SFC dedicated as PHEV charging would seem to be superfluous, because electricity is fungible.

- Future expansion plans beyond California are missing.
- The project provides a reasonable plan to continue expansion of the model and to improve its interaction and connectivity with other models.
- Supporting the project with a utility partner is a good idea.
- A CHHP partner is also recommended.

Strengths and weaknesses

Strengths
- It's useful to provide a clear pathway to emission reduction to assess most effective means.
- The project approach integrates production and demand to model impact of hydrogen technologies in complex energy sectors.
- The project can model the impact of legislative actions (e.g., carbon taxes) on the adoption of advanced technologies.
- The project presents high-quality, analytical work.
- The project was well presented and user friendly.
- The project's focused scenarios incorporating actual requirements make the results more compelling.
- CHHP analysis for co-products is an excellent idea.

Weaknesses
- The practicality of co-locating a fuel station with power production is questionable.
- The project's assumptions might have overlooked some practical limitations for equipment siting, capital, and operating cost.
- The results of the project are highly dependent on long-range projections of legislative activities.
- None noted.
- The project scope was limited to California.
- The thermodynamic estimates for electrical efficiency and electrical hydrogen co-production efficiency estimates are very low for CHHP. Reconsidering the estimates is recommended.

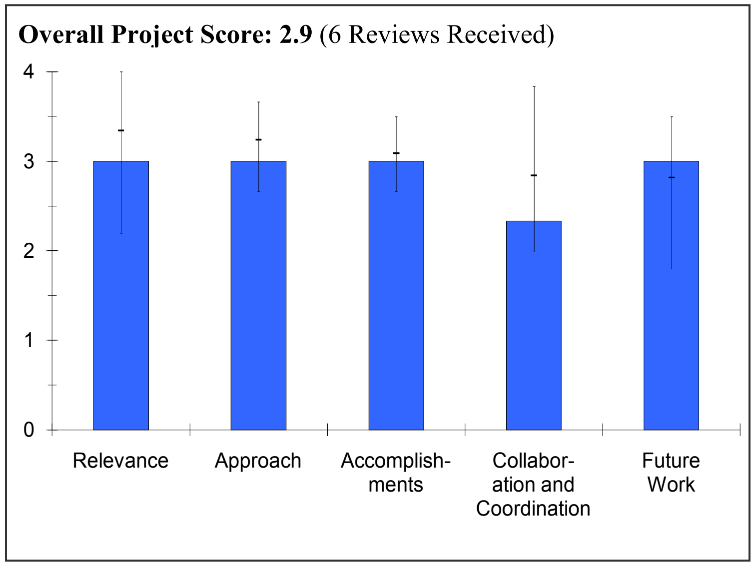
Specific recommendations and additions or deletions to the work scope

- It's recommended to consider real-world input from companies that would consider such investments and on what basis. If companies will not invest in the infrastructure assumed, then the model is not relevant.
- The PI should continue with consideration of the coal-burning regions. Include studies of nuclear and renewable (e.g., solar, wind, and geothermal) energy on SFCs.
- It's recommended to take the project scope beyond California.
- The project's technical analysis alone may lead to misleading conclusions. For example, hydrogen has a greater economic value in comparison to heat.
- A techno-economic analysis is encouraged.

Project # AN-03: Agent-Based Model of the Transition to Hydrogen-Based Personal Transportation: Consumer Adoption and Infrastructure Development Including Combined Hydrogen, Heat, and Power

Matthew Mahalik; Argonne National Laboratory

Brief Summary of Project



The objectives of this project are to: 1) explore the chicken-or-egg problem of "which came first?" through co-development of the hydrogen production and delivery infrastructure and the user base which supports it; 2) understand how the system works rather than provide one forecast of system development, including questioning how different policies affect the transition, how sensitive growth is to factors beyond the control of policy makers, and what role consumer attitudes and behavioral characteristics play; 3) consider, in a complex adaptive system, the interactions among hydrogen fuel producers and suppliers, consumers of hydrogen fuel and fuel cell vehicles, and manufacturers of fuel cell vehicles, and 4) extend the current agent-based model to include limited-service combined hydrogen, heat and power (CHHP) facilities as well as the regular distributed production hydrogen fueling stations currently modeled.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The use of CHHP for retail stations seems unrealistic and is not likely to support the fulfillment of the program goals. The premise is not promising. A hospital or other CHHP source will not necessarily be motivated enough by a small revenue stream with potentially significant distractions to their core business and assumed liabilities.
- The purpose of this project is to develop and use an economic model to simulate the market transition from personal vehicles that are petroleum-based to ones that are hydrogen-based. By economic, the model attempts to capture the behavior, presumably based on market price and other economic costs, of independent actors that might choose to produce hydrogen fuel, produce hydrogen-powered vehicles, and purchase hydrogen-powered vehicles. More specifically, the research focused on the effect that CHHP might have on-fleet deployment, as independent actors chose to produce hydrogen for transportation fuel using CHHP facilities. Analysis and understanding of this question fully supports the DOE's Fuel Cell Technologies Program RD&D objectives.
- The project's model will guide the hydrogen program's RD&D objectives.
- The project is relevant for assessing areas to focus efforts in development of hydrogen distribution network.
- The project provides information necessary for formulating policy plans.
- The project supports only a very specific scenario that has very little potential to make an impact on consumer behavior.
- The project's showcase of understanding interactions among producers, consumers, and manufacturers is important.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Considering the given inputs, the project model stands strong. However, it will be challenging to evaluate the qualitative reasons for people's decisions, as opposed to making an assumption based on financial considerations.

- This project has some similarities with project AN004, especially in terms of purpose. However, this project is unique as it involves a market research firm (Synovate) to develop different scenarios using different driver personalities. This approach creates a risk that the personalities will behave in the model as they are defined, rather than as the public as a whole would in real life. In economics, varying prices cause people to alter their behavior, rather than to continue to behave according to a stereotype. Nonetheless, the project seeks to develop insights for the questions raised in slides 3 and 4. These are interesting questions, and the approach is given the benefit of the doubt.
- The project's agent-based approach and ability to vary their behavior and the effect of policy makes the model a strong tool for understanding a possible transition to the hydrogen economy scenarios.
- The Los Angeles study is a useful and important model but contrasts between other large metropolitan areas, such as New York City and Chicago, would be appreciated.
- The inclusion of CHHP to the model is valuable, but it mostly relies on unproven technology (on a commercial scale), so it seems somewhat of a distraction and needs to be clearly compared to other alternatives.
- The project's approach is appropriate given the timeline and funding level and should accomplish its goals. Success with this approach should provide a model on how to approach more complex situations.
- The project has a very structured approach to develop and exercise the tool.
- The project's approach will evaluate driver acceptance of CHHP dispensers. However, it will not provide information on whether CHHP owners would really be willing to sell small quantities of hydrogen.
- Few, individual, small transactions of hydrogen sales are not practical for an organization that is not in the business of fuel sales to consumers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- The approach doesn't seem to match the relevance slides. It's not clear how the approach will answer the stated "chicken/egg" problem – whether users will decide to buy at hydrogen dispensing facilities (HDF), and whether CHHP prospects might actually make the decision to become fuel providers.
- The project is relatively new, as it started in March 2010 and has only had modest progress to date. (See data on slide 12.) However, the rate of progress could not be considered slow.
- The project's granularity of the model was increased to improve realism.
- The project used an original equipment manufacturer (OEM) agent to limit the number of vehicles available during early rollout.
- The model provided a more realistic simulation of driver agents.
- The project had limited progress to report on, since it just started when slides were submitted.
- The tool provides interesting graphics and visualization, but the question being answered is not particularly relevant.
- The progress is on par with the period of performance, but activities like increasing the granularity of the roadway are not particularly challenging.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- The project should consider actual CHHP prospects to identify what would drive a decision to sell fuel. They also need to work with actual FCV customers to identify whether they would shop at a non-traditional fuel station; only then can a model be viable.
- In addition to the subcontractor Synovate, the project uses the National Renewable Energy Laboratory's (NREL) power model. The project seeks to model consumer behavior in Los Angeles. It came out during the Q&A that the California Fuel Cell Partnership believes that they have data they would like to contribute to the project. Given the similarities between the objectives and methods of this project and AN004 (using HyTrans to get insights to similar questions), the two projects might benefit from the coordination of their efforts.
- The project presents collaboration with a national lab and a market analysis company. More collaboration with industry and government stakeholders is needed.

- The project could probably expand on what other organizations brought in, for example, the Department of Transportation (DOT).
- The project is interacting with appropriate experts.
- Enlisting an automotive market research firm is beneficial for characterizing drivers, but this reviewer questions how accurately they have characterized the CHHP owners. The project needs multiple current combined heat and power (CHP) owners and potential CHHP owners to assess and provide input to assumptions.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The future work should be revised in an attempt to better match the work with the goals. The model is jumping ahead on the easier quantifiable aspects, but it is not understood if the qualitative aspects will have more of an affect. The project should identify whether CHHP sites have the room and interest to be fuel providers.
- The project's future work is basically to run the study as planned.
- The project should expand to California and other metro areas.
- The project should add the possibility of transition failures.
- The project plans are appropriate given the scope.
- There is no need to expand the project scope.
- The team needs to look over and consider the competitive pricing of hydrogen.

Strengths and weaknesses

Strengths
- The project seeks to develop insights into important questions of interest to the DOE's RD&D objectives (slides 3 and 4).
- The project's agent-based modeling approach can be calibrated to new market data.
- The project's limited scope allowed results to be obtained in a short amount of time and with limited funds.
- The development of a driver agent appears to be good and may help in forecasting vehicle sales.

Weaknesses
- The project is unable to assess how consumers and producers will actually make decisions on criteria other than price and location.
- It is hard to imagine how this project will be able to do a credible job on such a complex and data-hungry subject in only six months. Given the similarities between the objectives and methods of this project and AN004 (using HyTrans to get insights to similar questions), the two projects might benefit from coordinating their efforts. There is slight worry that the development of driver personalities might bias the results by the propagation of stereotypes through the model.
- The project has too much emphasis on Los Angeles and seems hard to integrate a very fine grid.
- It is unclear how their method could be adjusted for a broader range of factors.
- The accurate prediction of hydrogen availability is very much in doubt.
- The accurate accounting for the total transaction cost of an institution, like a hospital, selling hydrogen fuel to consumers is very much in doubt.

Specific recommendations and additions or deletions to the work scope

- The project should incorporate real-world input from CHHP prospect companies that would consider such investments and on what basis. If no one will invest in the infrastructure assumed, then the model is not relevant.
- None.
- The project should expand to other metro areas.
- The project should contrast all methods of hydrogen production.
- The project should incorporate more data from industry and government stakeholders.

SYSTEMS ANALYSIS

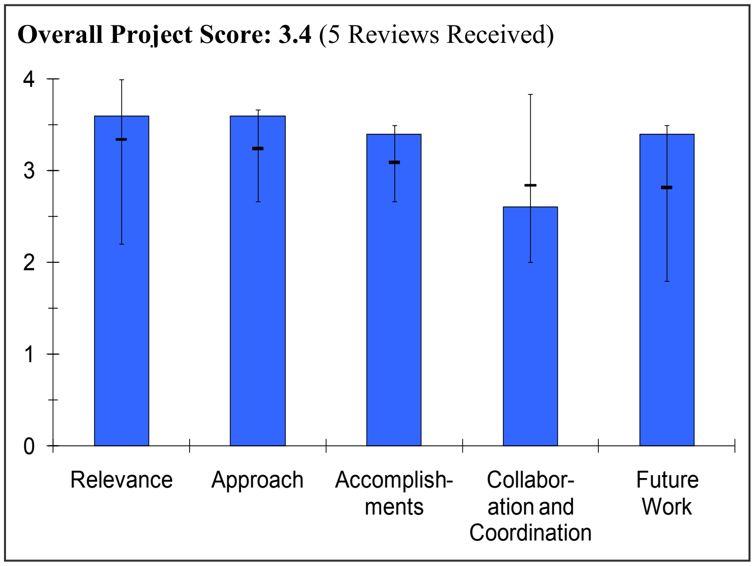
- The project work should be discontinued or refocused on a more relevant question.
- The project scope should be expanded to include competitive pricing, competitive uses of resources, and competitive technologies.

Project # AN-04: HyTrans Model: Analyzing the Potential for Stationary Fuel Cells to Augment Hydrogen Availability in the Transition to Hydrogen Vehicles

David Greene; Oak Ridge National Laboratory

Brief Summary of Project

HyTrans simulates the dynamic market transition from petroleum to hydrogen-powered vehicles. It represents hydrogen supply, vehicle production, and consumer demand for and use of hydrogen vehicles by 2050. For this study, the model was augmented to include a partial representation of the stationary fuel cell market. Fiscal year 2009-2010 research focused on inclusion of internal combustion engine and hydrogen plug-in hybrid electric vehicles and on analyzing the role that combined heat, hydrogen and power (CHHP) could play in increasing hydrogen refueling availability during the transition. The CHHP analysis contributes to understanding potential synergies between stationary and mobile hydrogen fuel cell applications.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The program is targeting real measures that can help subsidize the required initial investment, so it directly supports DOE efforts to figure out means for developing infrastructure.
- The purpose of this project is to develop and use an economic model (HyTrans) to simulate a market transition from a petroleum-based to a hydrogen-based transportation system. By economic, the HyTrans attempts to capture the behavior, based on market price and other economic costs, of independent actors that might choose to produce hydrogen fuel, produce hydrogen-powered vehicles, and purchase hydrogen-powered vehicles. More specifically, this year, the research focused on the effect that CHHP might have on fleet deployment, as independent actors chose to produce hydrogen for transportation fuel using CHHP facilities. Analysis and understanding of this question fully supports the DOE's Fuel Cell Technologies Program RD&D objectives.
- The project presents an important model to understand the transition from a petroleum-based economy to one based in renewables.
- The project appears to be relevant, but the use of multiple unexplained acronyms made it difficult to follow during the presentation.
- This project is looking thoroughly at hydrogen deployment scenarios, making it critical to the program and understanding of the distribution growth.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- The project seems to be well thought out with reasonable and practical assumptions.
- Presumably the PIs had tasks to further develop the HyTrans model to support new analysis of hydrogen ICE (internal combustion engine) vehicles, hydrogen PHEVs (plug-in hybrid electric vehicles), and CHHP-distributed hydrogen production. However, this was not emphasized. Rather, as the approach, the PIs describe developing three national CHHP deployment scenarios, and two methods of hydrogen delivery, as the framework in which to compare policy paths.

- The project presents a non-linear dynamic model that uses fuel supply, vehicle manufacture, and consumer choice agents.
- The project is integrated with other available models.
- The project's approach appears sound, but again the presentation is difficult to follow completely. This may actually be a problem related to the previous year's comments that energy industry collaboration and buy-in is lacking. The way the project is presented may be too difficult to fathom because of the nomenclature.
- Assuming that a CHHP owner will sell hydrogen to a fuel retailer is much more realistic than assuming that they will become a fuel provider.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- The project is progressing toward developing output that could be useful.
- The project accomplishments and progress are demonstrated in slides 8-18. They reached an interesting result (though one that follows from the design of the HyTrans model) that there is a fuel availability cost that affects vehicle purchase decisions based on the perception of the difficulty in obtaining hydrogen fuel when needed. That is, a cost separate from the actual cost of the fuel itself. Further, this phenomenon creates a situation where, while most of the fuel volume is produced by steam methane reforming (SMR, the least-cost method), the perception of fuel availability (i.e., the reduction in the availability cost) is mostly satisfied by CHHP (see slide 17). Such insights help to overcome barriers to deployment.
- The project emphasis this year was placed on CHHP using different amounts of government subsidies.
- There is no progress on energy industry buy-in and collaboration, and there should be. This should have been a high priority, and working on that would probably have made the project more understandable.
- The project recognizes the substantial subsidies required to make CHHP hydrogen happen.
- The project presents the most realistic scenario for CHHP deployment.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- The project has some good partners, but it could use more who could validate whether a small CHHP station is viable based on site and/or business constraints. It's not clear whether it is worth the effort and risk for an overall small amount of revenue, even if it's profitable.
- The project, by its nature, is highly collaborative. The PIs report seeking out information from other DOE labs (e.g., National Renewable Energy Laboratory or NREL, Argonne National Laboratory or ANL), academia (e.g., University of California, Davis or UC-Davis, University of Tennessee or UT), and the community of researchers that develop and utilize HyTrans.
- The project presents collaboration with other national labs and academic institutions.
- The project needs much more collaboration with industry and government stakeholders.
- The project should reach out to the energy industry.
- It is disappointing that industrial collaborators from earlier HyTrans work were not brought into this activity.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- The project goal to publish in a peer-reviewed forum is a good one.
- Slide 20 lays out a particularly well-motivated description of the intended future work and its objectives.
- The project team will develop and test an integrated policy framework for the transition to hydrogen and disseminate the results widely.
- Energy industry collaboration and buy-in should have been on the slides presented, as they were promised to be addressed by the PI.
- Project plans are very good, especially integration into HyTrans and incorporating uncertainty in technology success.

Strengths and weaknesses

Strengths

- The project presentation was well done and explained in a simple, non-academic fashion with relatively plain English.
- The project seems well organized and practical, recognizing uncertainty and risk.
- The HyTrans model was enhanced to analyze the potentially important synergy between the stationary and mobile fuel-cell markets.
- The project presented a sophisticated model that includes all required components.
- The project's technical approach assumes CHHP owners will sell hydrogen to fuel retailers and that installation of CHHP will require subsidies.

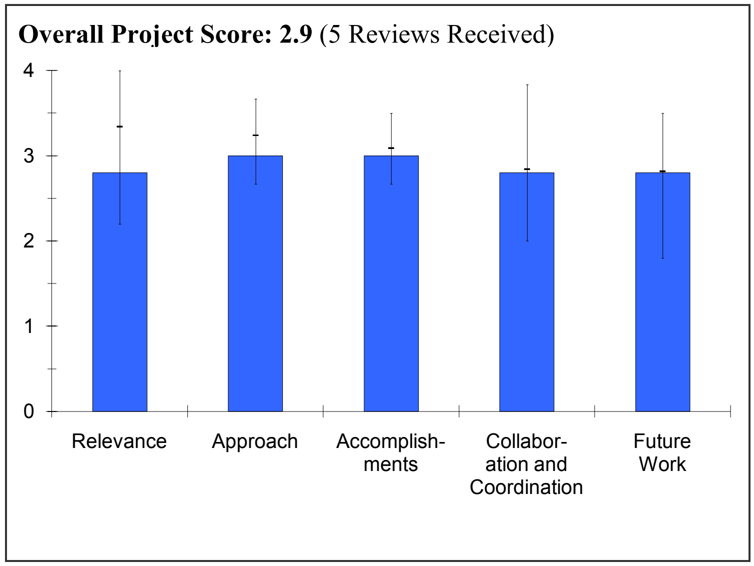
Weaknesses

- Some of the project assumptions seem unrealistic. Using 1,500 kg/day distributed reformers are not a viable answer for forecourt. Also, the ability to produce and install 60,000 CHHP stations in less than nine years from a zero base seems aggressive. It is not clear if it will be justified at lower volumes.
- The PIs are upfront about the weaknesses in the project on slide 25. These include the need for more accurate quantification of data, the uncertainty as to the pace of technological progress and the course of world energy markets, and the lack of integration in the model between U.S. and international fuel cell markets.
- The project needs more emphasis in comparing all scenarios.
- The project needs additional input from industry and government stakeholders and their peer review.
- The unnecessary use of acronyms is very off-putting. The energy industry collaboration and buy-in is not a high enough priority.
- The project lacks stakeholder involvement.

Specific recommendations and additions or deletions to the work scope

- None.
- There should be a comparison of all hydrogen generation possibilities.
- Present the project in a simpler language.
- Establish an advisory committee of representative stakeholders.

Project # AN-05: Biogas Resources Characterization
Ali Jalalzadeh-Azar; National Renewable Energy Laboratory



Brief Summary of Project

The objectives of this project are to: 1) develop a cost-analysis tool for bio-methane production from biogas based on the Hydrogen Analysis (H2A) Production model; 2) gather Geographic Information System (GIS) data on biogas resources in California and cost data on biogas purification systems, and 3) perform techno-economic analyses for various scenarios involving the production and utilization of bio-methane. The project can provide valuable insights and information to the stakeholders – utilities, municipalities, policy makers (at a macro level), and producers of biogas (at a micro level).

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The project uses bio-methane to produce hydrogen, which is an important pathway to renewable targets.
- The project addresses the development of an H2A tool to analyze the cost of implementing bio-methane production from landfills and agricultural waste. Such information is needed to evaluate the economics of bio-methane as an energy source.
- Opportunity fuels present a potentially valuable and under-explored hydrogen production pathway. This research appears to fill a gap in characterizing when and where these resources can be economically utilized.
- The project enables the understanding of the necessary steps and the potential costs needed to turn raw biogas into a usable fuel. While fuel cell is one of the potential uses of the upgraded biogas, it is not clear how this work supports the DOE's Fuel Cell Technologies Program. The upgraded biogas likely will have many other more feasible applications.
- Outstanding project for the abatement of greenhouse gas (GHG) and use of energy resources, but it has very little to do with the production of hydrogen.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The project has developed a reasonable model to evaluate the biogas potential. However, the result suggests that it's not cost effective.
- The project does not mention that the largest source of biogas is landfills, and that landfill gas cannot be legally put into pipeline systems at this time in California.
- The project approach builds on the utilization of an H2A production tool and assembling a GIS database in the California market.
- The project uses an existing H2A model and approach to project biogas costs, energy use, and GHG emissions. It also entails collecting of GIS data related to the location of biogas resources. The H2A cost-modeling approach seems strong. However, the overarching goals and deliverables for the GIS analysis portion are not totally clear, particularly as they pertain to enabling a transition to a hydrogen infrastructure.
- The project would benefit from a more clearly articulated concept of the problems being addressed by the GIS component and associated scenario analysis. For example, it is not clear that the sample scenarios that were

presented require actual GIS data. The work could also use a more explicit focus on issues with bio-methane-to-hydrogen production, as distinguished from the current focus on bio-methane production. One example would be analysis of onsite hydrogen production.
- It's not clear the reason behind using H2A (a model used to look at hydrogen cost) to estimate the cost of upgraded biomass where some assumptions and basis might not be valid. Also, if only California data is used, it's not clear how it will be useful for other locations in the United States.
- The project realistically looks at biogas capture and distribution.
- The project's approach of injecting biogas into a pipeline is very logical.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- It's not clear if "cost" is being mixed up with "price". If this is a cost, then the price to justify the investment to get a return will make the economics worse. Also, the price $9.5/GJ for natural gas is a retail price, not a wholesale gas production number.
- They also need to evaluate the risk premium to an investor. To justify a long-term investment, a higher rate of return may be necessary.
- They have made good progress on assembling a model and identifying sensitivity studies. There is also good progress on cost versus size and/or capacity studies. Progress has been made in an analysis of the cost of constructing a piping network that would be required to interface to a NG pipeline.
- The project work is largely complete, with good progress towards the overall goals.
- The project has developed an initial biogas-costing tool that incorporates factors such as fuel quality and size of the facility. Biogas GIS data has also been gathered for the state of California and has been used to perform a preliminary scenario analysis.
- The costing tool appears to be strong. While it is understood that the scenario analysis is preliminary, the results seemed to lack coherence. For example, some interesting results were shown, but the overarching questions being answered and goals being addressed were unclear.
- The capital cost and operating costs need to account for the cost of waste handling from the upgrading process and the cost of monitoring upgraded biogas quality, which is more applicable if upgraded biogas is injected to an existing pipeline. Construction of a biogas pipeline must be justified by biogas availability and sustainability.
- Economics of the process are not going to be accurate until the process has been defined in detail.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- The project had no identified partners. Collaboration was mentioned, but no feedback was offered.
- The project inherently requires significant collaborations to develop a GIS map for California.
- The project appears to have successfully integrated feedback from industry and municipalities to gather and verify data and to review results.
- The PI should consider reviewing the study and results with key stakeholders to appropriately vet the cost numbers, including those in the gas industry.
- The project approach of soliciting input from stakeholders through workshops is good, but it would be better if they were more directly involved in the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The project future work as identified will have limited usefulness. A better plan would be to find a vendor who is willing to build such a project to demonstrate the validity of the cost model and its conclusions. If the project had no adopters, then that will stand as final judgment.
- Potentially, the PI could develop a DOE-funded demonstration project to validate the costs, reliability, etc., of the equipment and pipelines.

- The project will address on-site utilization of bio-methane.
- The project is largely complete. It was mentioned that the PI would include analysis of on-site hydrogen production options, which is an important gap. There was also some discussion of characterizing the sensitivity of results to the resource quality (i.e., level of impurities). It is not clear from the presentation the depth with which these issues will be addressed, as there is limited time and funding remaining, but they are both important pieces of the overall analysis.
- Fuel cell (FC) use of biomass on-site is one of many options. It might be worthwhile to look into other monetization options. (It's not certain if this work should be done or funded by the Fuel Cell Technologies Program).
- The project should consider linking with other models to possibly develop a database of available biogas sources at various large municipalities throughout the United States.
- Including the tradeoff analysis for on-site use of bio-methane would be appreciated.

Strengths and weaknesses

Strengths

- The project presents a good model to understand sources and costs of biogas.
- The project addresses the utilization of bio-methane that is currently poorly utilized.
- The project analysis fills an important gap by characterizing a potentially cost-effective hydrogen production pathway.
- The project leverages previously developed tools and analysis (i.e., H2A) to insure consistency.
- The project demonstrates an economical process for eliminating a significant GHG.

Weaknesses

- The project identifies that the amount of biogas used is minor compared to the total gas used. So, there is a limited ability to affect the renewable target. The team focus seems to be on agricultural waste, but the amount available is small compared to landfill gas and total natural gas usage.
- The project conclusions are an attempt at straddling the issue. The model does not suggest that "biogas from farms can be cost effective."
- The project's current approach focuses on exporting bio-methane to a natural gas (NG) pipeline. Greater attention to on-site generation of bio-methane and generation and/or export of electricity to and electric grid would bypass the need to build feeder pipes to NG pipelines.
- The GIS component does not appear to be integrated with the rest of the project. It's not clear how the gap is being filled by this portion of the analysis.
- The project scenario analysis would benefit from identifying important questions.
- The project has limited relevance, if any, to hydrogen production and reducing hydrogen cost.

Specific recommendations and additions or deletions to the work scope

- The PI is encouraged to analyze an on-site conversion of bio-methane (fuel cells or diesel generators) to electricity versus the export of bio-methane to a NG pipeline.
- The team is encouraged to include parameters to characterize the cost sensitivity of gas to impurities. It has a strong effect on the price of fuel in other landfill gas-to-energy applications.
- It's recommended to consider the project economics taking into consideration a carbon tax.
- The PI needs to define the process to improve cost accuracy.

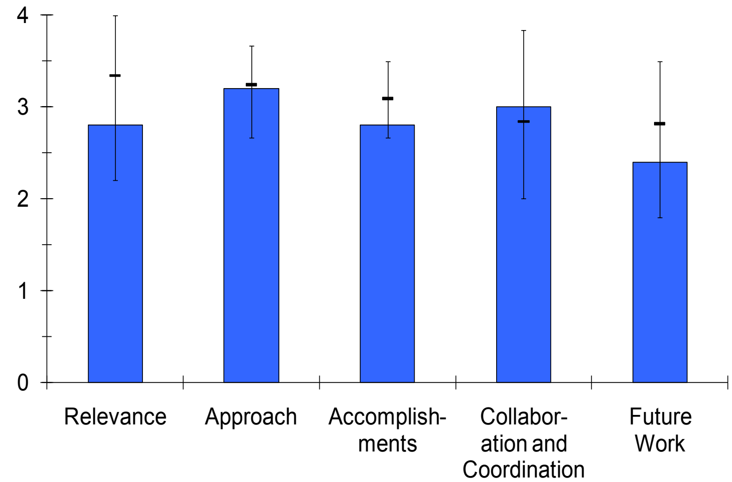
Project # AN-06: Cost and GHG Implications of Hydrogen for Energy Storage

Darlene Steward; National Renewable Energy Laboratory

Brief Summary of Project

Hydrogen has unique attributes as an energy storage medium. Hydrogen could play a dual role as storage medium for electricity and as a fuel for vehicles. The objectives of the project are to: 1) evaluate the economic viability of using hydrogen for utility-scale energy storage applications, in comparison with other electricity storage technologies (including a simple energy arbitrage scenario) and analysis of potential for cost improvements over time, and 2) explore the cost and greenhouse gas (GHG) emissions impacts of interaction of hydrogen storage and variable renewable resources, including specific locations and wind profiles and hourly energy analysis to capture detail.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The project focuses on balancing renewable production with demand, which is important to optimize the utilization of intermittent renewable sources.
- The project addresses the economic viability of using hydrogen as an energy storage media integrated with wind power farms. The idea is that excess wind power would generate hydrogen via electrolysis. In return, excess hydrogen would subsequently be used to supplement electricity generation during periods of low wind power generation.
- This project has two primary tasks. The first analyzes hydrogen storage as a load-leveling tool for the electric sector; and the second looks at using hydrogen as an approach for optimizing the production of electricity and hydrogen transportation fuels. The second task is very relevant to meeting the goals of the DOE's Fuel Cell Technologies Program, as this offers an opportunity to leverage synergies between renewables and hydrogen, thereby increasing the competitiveness of both. However, while hydrogen storage (the first task) could be a useful tool for integrating renewables into the electric grid – and is both important and promising – it is not clear that this is directly applicable to the goals of the hydrogen program. It seems to relate more directly to efforts focused on deployment of renewables. To be fair, however, this is an area that does not fit neatly into any one compartment. In addition, it is likely that the work related to the first task is a useful adjunct to the second task.
- This project looks at options to maximize available wind power and manage variability in wind power and electrical demand.
- This project only partially supports the objective of reducing the cost of hydrogen for fuel cell applications.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The project's approach seems reasonable and effective.
- The project utilizes a suite of economic analysis models and tools to examine the cost effectiveness of integrating two (hydrogen and wind) technologies.

- The overall approach, the questions being answered, and the experimental design are well constructed. It would be helpful to more explicitly identify the approach for the storage and transmission constrained cases (slide 17). It would be beneficial if this could be presented as a range of results rather than a single point.
- It's not clear how vetted the hydrogen combustion turbine data is. These numbers seem very good but additional verification and/or validation aside from the single source mentioned during the question and answer session is recommended.
- The project's option of 12,000 kg/day excess hydrogen storage is unrealistic, since wind farm and geologic storage will likely not be available in many locations.
- The project should consider future technologies, such as lithium ion (Li-ion) battery storage, where the round trip efficiency is going to be closer to 90% than the 60% cited for nickel metal hydride (NiMH) batteries.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The project suggestion of exporting hydrogen for vehicles only works in select geography where there is a need for hydrogen in close (economic) proximity. This would greatly narrow the applicability and potential advantages that it could offer.
- Competing technologies have probably better understood operating and maintenance (O&M) costs. O&M for hydrogen might be higher than indicated.
- The project's modeled cost of different storage routes relative to hydrogen storage is a sign of good progress being made.
- The project results for the first task were presented, which showed that hydrogen is potentially cost competitive as an energy storage medium. They also showed preliminary results related to several cases for the second task. In general, it seems good progress has been made toward meeting the project goals.
- The cost to run a dedicated transmission line from a wind farm and/or storage to a grid center must be realistically accounted for as this cost could be very significant.
- The project needs a more thorough understanding of the data inputs for competing technologies to really assess the accomplishments. Battery efficiency looks too low but is probably reasonable for NiMH. Turbine efficiency might be on the high side; although it might be reasonable with a good bottoming cycle. Hopefully, these have all been covered in a more detailed peer review.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The project has a good partner, Xcel Energy, to comment on real-world feasibility. During the project, the PI worked with Xcel Energy, Pacific Northwest National Laboratory (PNNL), and National Renewable Energy Laboratory (NREL) hydrogen teams.
- The project collaborations are adequate, since they included internal collaboration with other analysts and received data and feedback from Xcel energy.
- There are good collaborations with various modeling groups and NREL.
- The project needs a utility or several utilities participating.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- The project's future work seems pointless unless there is a credible path to this being competitive against grid power (e.g., worth the capital) and against competing technologies.
- It's suggested to work on better technology than to continue analysis. The areas needed for improvement ought to be clear from the data.
- The project's future work will incorporate greenhouse gas studies.

- The work is largely complete. Future work entails further optimization of the hybrid electricity and/or hydrogen production case and analysis specific to solar resources. These are important questions and will continue to improve the work.
- In addition to comparing the round-trip efficiencies and leveled outputs of electricity among various technology options, the PI should consider looking at the payback period and technology maturity and risk of each option, with and without carbon consideration. Ultimately, the decision to install a certain technology will be largely based on how quickly the technology pays for itself.
- Future plans will improve results, but it still won't make a critical contribution.

Strengths and weaknesses

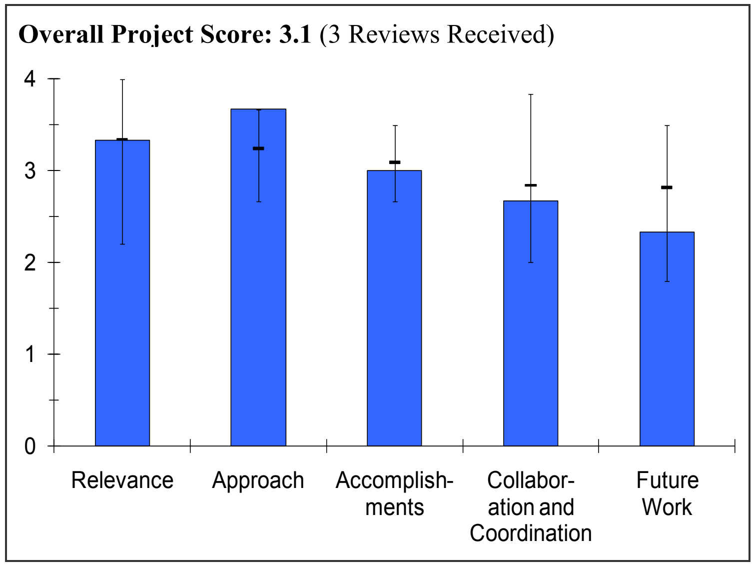
Strengths
- The project presents good analysis that shows the competitive position of the different technologies.
- The project recognizes the need to incorporate energy storage technologies into renewable energy production (solar and wind). Such approaches are critical to reducing the cost of solar and wind power, which must often build in significant levels of excess capacity to compensate for the wide variability on power production.
- The project's strong experimental design approach fills a gap in the overall knowledge space.
- The project makes a fairly good case for pairing fuel cell and electrolysis technologies with wind farms.

Weaknesses
- The project data seems to indicate that hydrogen is not cost effective as a storage means, but the conclusions do not directly indicate that.
- Electrolysis to hydrogen to electricity has a poor round-trip efficiency, which will be difficult to overcome.
- Hydrogen storage is a relatively immature technology compared to alternatives. Projections are subject to large uncertainties in projected costs of hydrogen storage compared to more mature energy storage routes.
- The project may not be acknowledging the strongest competitors to hydrogen technologies.

Specific recommendations and additions or deletions to the work scope

- The project's future work should not be considered until there is a pathway to lower the capital costs or increase efficiency to make it competitive with other storage methods.
- It would be interesting to focus more in depth on where and when hydrogen storage could actually be used (i.e., what resources, cost of electricity, etc.).
- The project should consider adding other energy storage options, such as Li-ion batteries to the study.

Project # AN-07: Hydrogen and Water: Engineering, Economics and Environment
A.J. Simon; Lawrence Livermore National Laboratory



Brief Summary of Project

The overall objective of this project is to quantify the impact of water on the future hydrogen economy, including the economic impact of water prices on hydrogen production and the regional impact of hydrogen production on regional water resources. The project addresses feedstock issues of the energy-water nexus and future market behavior regarding the timing and magnitude of hydrogen water stresses.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The project provides analysis on the impact of water treatment technologies and clean-up on the cost of hydrogen.
- It is critically important that the DOE understands the impact of any future hydrogen economy on water resources.
- Water resources could be critical to hydrogen production.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- This project utilizes the Hydrogen Analysis (H2A) tool.
- The project integrates other program models effectively.
- The project compares water resources with future hydrogen demands and suggests technically feasible approaches based on water resources.
- The project's approach is thorough and well thought out.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Although the project is nearing completion, the level of results seems to be marginal. The project compares centralized steam methane reforming (SMR) with distributed electrolysis and water treatment options.
- The project clearly shows water stress based on hydrogen rollout. However, the model needs refinement since issues such as the Great Lake water resources were not taken into account.
- The project presents a very good sensitivity analysis. The table indicates the preferred approach versus water price.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- The project coordinates with National Energy Technology Laboratory (NETL) and Sandia National Laboratories' (SNL) Energy-Water Nexus group.

- This project needs to collaborate with industry and government water resource stakeholders. It's not clear what the effect will be on future policy and legacy water rights.
- The project's biggest challenge appears to be securing permissions in highly stressed areas; therefore, the PI should consider bringing permit issuers into the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- The project should continue integration of other program models.
- The PI should consider expansion to international water resources.
- The project scope should expand to other hydrogen generation methods.
- The significance of adding this to H2A is in doubt.

Strengths and weaknesses

Strengths
- The project recognizes and/or addresses the potential impact and/or barrier of water availability and the cost of water treatment on the cost of hydrogen production.
- The project takes water resources into account, which must be a critical component of any analysis on the future rollout of technology.
- Understanding what will be required to obtain water permits for hydrogen production on a regional basis could be equivalent to a go/no-go decision on hydrogen production facilities.

Weaknesses
- The PI presented limited information.
- The project needs to drill down and make sure that all water resources are included.
- The project needs to take into account legacy water rights that may restrict the use of water in certain regions.
- The project needs to expand its methods of hydrogen production.
- This project will have little impact on the price of hydrogen.

Specific recommendations and additions or deletions to the work scope

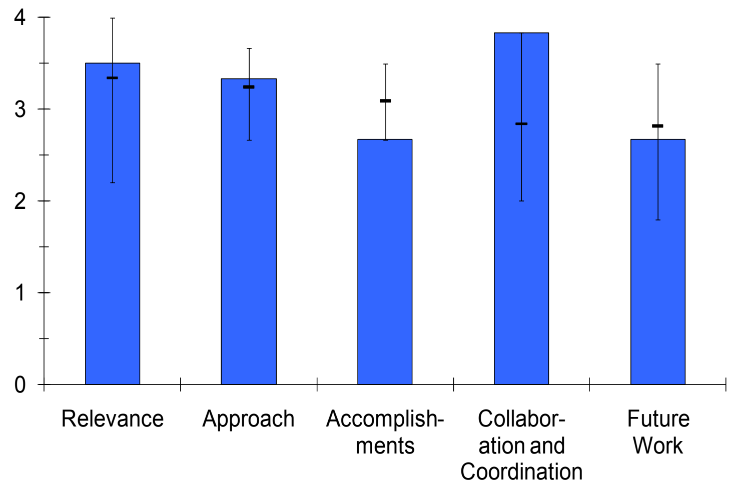
- Place your emphasis on water resources, and make a database of water resources that accounts for all available water, as well as restricted water by legacy water rights and local regulations.
- Continue to focus on the issues that are critical to hydrogen production, such as working with those that issue water permits to assure they understand the water and hydrogen impacts.

Project # AN-08: Analysis of Business Cases with the Fuel Cell Power Model
Marc Melaina; National Renewable Energy Laboratory

Brief Summary of Project

The objective of this project is to revise the Hydrogen Analysis (H2A) Fuel Cell Power model to suit the needs of business and finance decision makers and model end-users. To meet this objective, a Business Case Tab will be developed. This tab will not replace in-house financial models, but it will extend the capability to do financial and business analysis within H2A. This is especially valuable for tri-generation systems due to multiple revenue streams. Understanding this objective will require stakeholder feedback.

Overall Project Score: 3.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project is determining business case and defining deployment opportunities that are very relevant to assessing success of the program.
- The project is supporting stationary power, which is a good and relevant opportunity to reduce fuel cell (FC) costs and increase utilization.
- The project is critical to the determination of scenarios for introduction of hydrogen-powered vehicles.
- The project is offering business-oriented end-users a tool for quickly and transparently evaluating the economic aspects of combined heat, hydrogen and power (CHHP). This focuses on addressing a specific gap in the current analysis capability and could help in initiating early stage hydrogen infrastructure rollout.
- It's not clear the immediacy of this project. Given the current commercialization status of hydrogen and fuel cell technology and infrastructure, a tool like this might be premature.
- The project provides a useful tool for the potential end-users to evaluate the feasibility of CHHP, but the usefulness of the one-size-fits-all characterization in making the decision to adopt the technology is in doubt. Different end-users have different business models, evaluation tools, and drivers for adopting new, unproven technologies.
- The project has a very well-identified set of key stakeholders.
- The model being developed is very well focused and is needed to meet DOE objectives. The project is building on CHHP, which is a unique opportunity.
- There seems to be an assumption in many presentations that the supply of natural gas (NG) is endless. With the new production techniques, much more is expected to be available. However, with the pressure to convert coal-burning power plants to NG and the limitations on distribution, this may not continue to be true. There is little doubt that a better picture of economic issues involving relative productions of heat, power, and hydrogen (especially using biogas) is highly relevant.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- The project has a good approach of developing a model to help analyze the business case from perspectives of various end-users and decision makers.
- Including stakeholder feedback is crucial to get the required accuracy and relevance and make the model more user friendly in the end.

- The project identified the right set of stakeholder groups. Perhaps the PI should include more relevant industry players who will be using the model.
- The PI should try to ensure commitment from stakeholders. In the future, at minimum, the model users should share their findings.
- This project expands the usefulness of the FC Power Model.
- The project expands the scope of the analysis to reach new audiences. This will be critical.
- The project approach entails extending the H2A model based on feedback and ongoing review from both targeted and broad-based stakeholder interactions. The approach addresses a well-defined gap, leverages existing tools, and, through stakeholder interaction, determines what functionality is most useful to the target audience.
- Given the project focus is on addressing stakeholder needs, it would be useful to put a straw man working version of the proposed tool in front of prospective users to help facilitate feedback (i.e., determine whether it meets their needs.) There is potential for getting more fruitful feedback if users have something concrete to which to respond.
- The project approach of extending the capability of H2A is good. It is a good start to utilize both internal National Renewable Energy Laboratory (NREL) resources and external stakeholders.
- The project's considerations of all economic and financial parameters are well organized.
- The California stakeholder list is good. Adding Electric Power Research Institute (EPRI) may increase the project's strength.
- The role of IDC Energy Insights (IDC) needs to be better defined in terms of value proposition.
- The project estimates hydrogen cost by default or internal rate of return (IRR). This is a very useful feature.
- The project is heavily dependent on stakeholder feedback. It's not clear how much feedback has been received to date, and if the stakeholders have an incentive to do so. Also, it was not clear how much depth would be involved in dealing with biogas, and how the independent review is going to be accomplished.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The project presents a good alternate business case calculation scenario. It's very relevant for analysis from different perspectives.
- The project received good feedback from the NREL MBA team, and all of it should be included as design features of the model. Another financial criteria used by industry and useful to calculate is VIR (Value to Investment Ratio) = NPV (Net Present Value)/NPC (Net Present Capital).
- The project problem formulation is adequate at this point. The key issue is there are multiple connected users of this approach, and it would be nice to get all of them on the same table to discuss how this can be best used.
- The project reviewed H2A model structure with the intent to identify possible extensions.
- The project collected feedback from the internal NREL MBA review team, NREL Technology deployment team, Federal Energy Management Program (FEMP), and Strategic Energy Analysis Center.
- The PI outlined the theoretical tab format and the to-do list of potential external revisions.
- The project is largely complete with limited concrete results to date. Since the H2A modifications were not presented, the implementation in H2A is in doubt. The project to date has focused primarily on identifying needs based on stakeholder interactions.
- The project appears to be behind schedule with respect to implementing the feedback after presenting the model and numbers to key stakeholders.
- The project's co-product cost estimates and user-friendly approach are quite good and efficient.
- The project approach of linking technology to strategy is very useful to new applications and users.
- State versus federal tax credits and ownership organization impact on economics will be valuable to add to this project.
- The utility side of economics, such as benefits to a smart grid, should be added to strengthen the model's value proposition.
- It appears that relatively little, beyond planning (which is certainly an important part of the process) and the workshop, has actually been accomplished. The International Partnership for the Hydrogen Economy (IPHE) workshop was an important step forward.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- The PI presented a good list of reviewers and stakeholders.
- The PI needs to get a list of first practical end-users.
- The PI presented very good collaborations with NREL.
- The PI presented good international collaborations.
- The PI presented good interactions with industry.
- The PI presented very strong and extensive collaborations with a diverse array of stakeholders throughout the project.
- As mentioned, the project appears to have strong collaborations with internal NREL resources and external stakeholders.
- The project interfacing with the fuel cell and hydrogen stakeholders is very productive.
- The PI should consider adding self-generators and distributed generation-type users.
- The project's actual formal collaboration seems to involve only NREL and IDC. However, it is assumed that there will be enough interaction with many stakeholders that they can also be considered collaborators.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Most of the work the PI presented set the requirements for the proposed future work and next steps.
- The project's future work seemed more focused on presentations. One would like to see more actual use of the model to expand the scenarios investigated.
- The PI should focus on continuing stakeholder interactions and actually implementing changes to H2A. As discussed in the "approach" comments, it seems like it would be useful to get the changes to the H2A in front of users prior to project completion. It was not clear from the presentation whether this is intended or not.
- The project should continue to collaborate with the Hydrogen Utility Group (HUG), California Fuel Cell Partnership (CaFCP) and IDC to verify and update the models. The team should talk to entities that have installed fuel cells to validate the model with real-world data. They are not CHHP, but lessons learned going through the process should be helpful.
- Very good work has been proposed by this group.
- Future work appears pretty vague at this point and highly dependent on more stakeholder feedback.

Strengths and weaknesses

Strengths

- The PI, Marc Melaina, is a good leader for this type of work.
- The project presents good coordination and approach to getting extensive feedback.
- This is a great new project.
- The project was designed to address a well-defined gap in current capability and has incorporated extensive collaboration with internal and external stakeholder groups.
- The project's outreach strategy is very good.
- The project is presenting what could be a very useful tool.

Weaknesses

- The project needs to identify how this would work for the end customer when there are multiple customers (station owners versus building owners) sharing the overall costs. It might be useful to think about a simultaneous Web-based interface to allow for negotiations.
- Ideally, a straw man version would be distributed to stakeholders for comment with some opportunity to respond prior to the project's conclusion. It is not clear given the timeline and funding whether this is feasible at this point. This could perhaps be included as a future additional scope.

- The project opportunity is to show how to improve the value proposition by adding relevance to a smart grid. This can help reduce the cost of hydrogen.
- The tri-generation fuel cell is not realistic. It's not clear at this time what hardware or costs would be involved in having the capability to choose the relative output distribution among heat, power, and hydrogen. This is especially true in dealing with biogas.

Specific recommendations and additions or deletions to the work scope

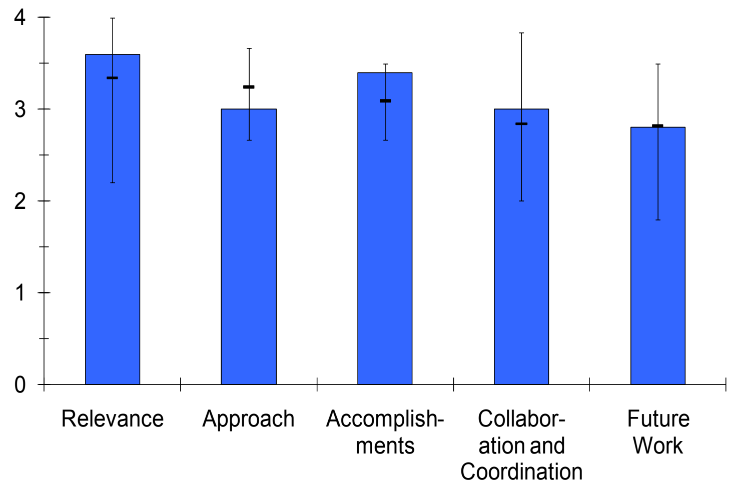
- The PI should consider adding a tool or interface to allow for simultaneous negotiations among various end-users.
- The PI should consider involving some good-level business school to develop a cost-effective business model, which could provide valuable input.
- EPRI may also be a beneficial partner for the project.
- The PI should try to incorporate, and make provisions for updating, some realism regarding the tri-generation fuel cells.

Project # AN-10: Fuel Quality in Fuel Cell Systems
Shabbir Ahmed; Argonne National Laboratory

Brief Summary of Project

The objectives of the project are to: 1) study the impact of impurities on fuel cell systems, including the components effected, performance loss, degradation and clean-up strategies, and their cost factors; 2) identify the impurity through system configurations that are most constrained by impurity effects, and 3) recommend research and development that can mitigate the deleterious effects and provide alternative and less expensive clean-up options.

Overall Project Score: 3.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The project is a necessary activity that is essential for deciding on the value of research into impurities and system design that will allow fuel cells (FC) to have sufficient lifetime in the field. The work is very relevant and covers most of the potential fuels. The weakness lies in addressing new fuels and perhaps developing a more general approach to impurity impact. The lack of good data from industry might be an issue that could be better addressed.
- The project helps identify the bad actors in hydrogen fuel for fuel cells, as well as identify the impacts and recommend approaches to deal with the bad actors. This work has more value by broadening the scope to look beyond natural gas (NG) and look at biogas and coal-derived syngas. With more R&D focus on bio-derived hydrogen production pathways, perhaps the scope needs to expand further to look at impurities for these bio-derived feed stocks.
- The project's focus on impurities is key to understanding the capital and operating costs and is very important to DOE goals.
- Impurity, system approach, and clean-up technology are very important to the success of fuel cells.
- The project is very relevant since hydrogen will likely be supplied from many sources for many types of fuel cells.
- It is critical that we have an understanding of the impurities and their impacts.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- This is an excellent project that is perhaps constrained by the data from industry.
- Two good approaches they have taken involve researching through literature review and working with those in the field. A possible recommendation would be for the team to look into the Tech Validation sub-program and the experience and data they obtained from their look into the impurities in a hydrogen feed and fuel cell vehicle operational data. Obviously, there are many factors that affect fuel cell performance. A good way to determine the cause of these problems would be to analyze the compiled composite data to see if there are any correlations.
- The project's hydrogen pathway list is pretty good.
- Combined heat, hydrogen and power- (CHHP) based hydrogen may have superior quality. The PI should consider including that if possible.

- This project has been going on since 2007 with nearly $1 million invested, and there are relatively few results. There must be a better approach than depending on responses from various organizations. The chart of results shows little new information. There is obviously value in knowing the content of landfill gas, but even here, there is such a wide range (both species and concentration) of possible contaminants that the results shown are clearly limited. A different approach for obtaining information should be considered.
- The project presents a very thorough review of impurities.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- The project has made progress toward the objectives outlined. The problem of data availability has impeded progress.
- The project database set up to document the impurities, its removal strategies, and their effects difficulties appears to be extensive and should be very useful for fuel providers and fuel cell developers and operators.
- The oxygen may have an adverse impact on the system and should be studied further.
- The list of contaminants presented by the PI is a good one.
- The project's coal gas contaminants analysis is very good.
- The project's analysis of the impact of multiple contaminants and their interactions is very comprehensive.
- The project resulted in few results relative to the time and funds invested.
- They have speciated the gases down to compounds that are present at the parts per million (ppm) level.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The project encompasses some collaboration with industry, but there is not much coordination with other national labs who work on the effect of impurities. It is recommended to use the other fuel cell research groups in the program as a source of information and data mining.
- The project has good collaborations with fuel cell companies. The PI needs to consider collaborating with more fuel providers. All the work should be shared with those working on establishing a hydrogen purity standard. The cost to make purer fuel must be balanced with the cost to make fuel cells more tolerant to certain impurities.
- The PI developed a good outreach strategy.
- Many other organizations are involved in the project, and it is likely that the work will include collaborative efforts.
- The project collaborators are the right organizations to identify which impurities will impact fuel cell performance, but it may not be the best for establishing how clean the fuel really needs to be.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Future work should be focused on finishing the project.
- The PI needs to have more discussions with fuel providers. The collaborations with fuel cell developers also need to look into longer term effects of impurities.
- This is very good work, especially considering that the topic is very complex.
- The project does not have a focused plan and has a slim chance of success.
- For future research, it would be great if the project could identify the key impurity that is limiting life.

Strengths and weaknesses

Strengths
- The project is very necessary.

- The project's comprehensive strategy for all fuel cells and multiple fuels is very good.
- It is a project where the right kind of results could be very useful to many entities. Very capable personnel and facilities are involved.
- The project is conducting a thorough analysis on impurities.

Weaknesses
- Data acquisition appears to be a critical limitation of the project.
- Past DOE reports from 1970 to1990 have lots of data that could be beneficial for the project.
- The Gas Research Institute and Electric Power Research Institute (EPRI) also have some good data that could be beneficial for the project.
- The project isn't getting done relative to the time and funding.
- The project is unable to determine how clean the fuel really needs to be.

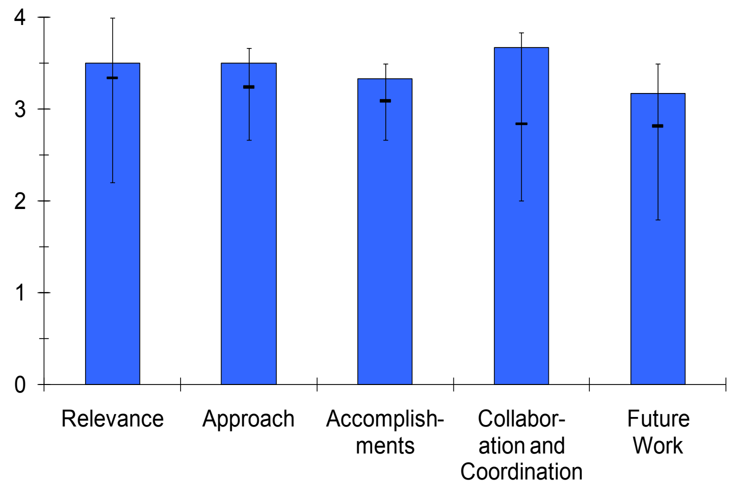
Specific recommendations and additions or deletions to the work scope

- The PI should attempt to obtain better data from the industry for future use.
- It will be very beneficial if the PI would perform capacity measurements in the presence of multiple contaminants.
- The PI should consider contacting biogas companies such as the American Biogas Council (ABC).
- The PI should consider an end date instead of an open-ended timeline. Additional funding should be based on getting certain results from current funding, in addition to having regular updates on progress and roadblocks.
- The project should consider air quality and its impacts.
- The project should consider trade-off analysis of cleaning the fuel versus sacrificing life and performance.

Project # AN-11: Macro-System Model
Mark Ruth; National Renewable Energy Laboratory

Brief Summary of Project

Overall Project Score: 3.4 (6 Reviews Received)



The overall objective of this project is to develop a macro-system model (MSM) aimed at: 1) performing a rapid, cross-cutting analysis, utilizing and linking other models and improving consistency of technology representation such as consistency between models, 2) supporting decisions regarding programmatic investments through analyses and sensitivity runs, and 3) supporting estimates of program outputs and outcomes. The objectives for 2009 and 2010 are to: 1) increase graphical user interface (GUI) functionality and capabilities; 2) utilize the MSM to compare hydrogen production, delivery, and dispensing pathways; 3) develop MSM links to the Hydrogen Demand and Resource Analysis (HyDRA) spatial data and visualization tool; 4) develop MSM links to the Directed Technologies, Inc.'s hydrogen production model (HyPRO) to analyze build-out scenarios, and 5) improve a pathway analysis by incorporating a vehicle cycle.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project is very relevant since it converts all the models developed under this program through one interface. That increases their usability significantly.
- The project relevance will be established once there is sufficient proof of industry use of the model.
- Integrating all the available models into one GUI allows important powerful analytical capabilities for addressing issues with the transition to the hydrogen economy.
- The project represents a very necessary policy-making tool.
- The project is providing a central link between hydrogen analysis tools and the enabling of consistent cross-cutting analysis, thereby filling an important role in the DOE Fuel Cell Technologies Program's systems-level analysis.
- The project's MSM model does a good job of linking various models to force consistency so end-users have one central basis.
- The project is very relevant, especially for planning future research and funding.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- The project approach is great since it links a very relevant model to provide common interfaces and to increase the usability of otherwise unused models.
- The PI needs to provide some perspective on model validation, because the model's accuracy is unclear.
- It's great that the project is offering flexibility where users can either use built-in assumptions or define their own.
- The PI needs to hold a training workshop and create documentation for industry stakeholders and the analysis community to make it a marketplace tool.

- The project's macro-system model provides a central transfer station to guarantee consistency in simulations that involve multiple models.
- The project's GUI (graphical user interface) allows users with minimal understanding of the models to use them.
- Apparently, validation is included, but there does not yet seem to have been rigorous validation testing. It would be good to see some explicit testing and a report on that.
- The PI has adopted an approach in which they developed a basic model framework in past years and have incrementally added additional functionality during subsequent development. This is a good approach, as it allows the PI to respond to the community's evolving needs and incorporate stakeholder feedback on an ongoing basis.
- Going forward, the PI should be mindful of the trade-off between maintaining usability while adding functionality such that the PI does not sacrifice the former for the benefit of the latter. (It's not clear that this has been the case to date, but it could become an issue.)
- They've done excellent work in setting the friendly GUI interface and allowing the user to download their own input files and results.
- The approach seems appropriate for the intent. There is always a question as to whether a mega-model such as this will be so unwieldy as to be almost unusable. It would also seem that incorporating updates could require a continuing effort.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The PI presented good progress on the GUI front to store the default values and multi-parameter sensitivity.
- The project's pathways report is a good comprehensive accomplishment for the last year and has very good, detailed description.
- HyDRA integration is a vast improvement for local calculations and identifying local constraints.
- HyPRO integration is also great to calculate optimized rollout scenarios. It's not clear whether HyPRO and HyDRA work together with MSM.
- The project increased GUI functionality.
- The PI used MSM to produce a pathways report contrasting seven hydrogen production and delivery pathways.
- HyDRA and HyPRO were incorporated into the MSM.
- Rigorous validation testing is needed to give a high degree of confidence.
- The project has added several additional pieces of functionality, including integration with HyDRA (which is potentially very useful) additional GUI functionality; and links to HyPRO. It also produced a fuel pathways report that was used to identify gaps in the current analysis, identify fuel production low-cost pathways, etc.
- Good progress was made toward meeting the 2009 and 2010 objectives, and most objectives have been met. The vehicle cycle component of the analysis is still in progress.
- It's difficult to ascertain, with this type of tool, the extent to which the additional functionality is actually used by the analysis community and fills gaps in overall understanding. It's hard to determine whether the additional tool is actually being used, and if it clearly advances the overall goals of the Fuel Cell Technologies Program.
- The project pathways report is very useful for those working on production and delivery areas. Improving GUI functionality and linking MSM to HyDRA and HyPRO added more functionality to the MSM and made it more useful to larger groups of end-users.
- The project has notable accomplishments, such as making GUI more user friendly and apparent progress on the mega-code. However, the project has been underway for many years at considerable cost.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- The PI needs to bring in industry stakeholders and future users as collaborators. There are plenty of models out there, and this could be the platform of choice for industry as well.
- The project included a very good mix of collaborators from national labs and energy companies.

- The project could benefit from more academic and automotive collaborators.
- The project has very good collaboration.
- The project includes collaboration with a number of stakeholders, including other analysts, technology developers, and energy companies. This is one of the strongest points of the MSM. This collaboration does a good job of ensuring that assumptions are vetted and the model is well integrated with other tools.
- The extensive list of collaborators is excellent.
- The project involved many groups in a collaborative effort.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The PI presents a good list of models remaining to be integrated.
- The project's proposed future work should include a defined plan on dissemination of the model and one on increasing the model usage in the technical and business community.
- Maintenance of MSM is an important task to keep the utility of the model.
- Evaluation of failure modes is important.
- The continued analysis of all production and delivery pathways and modes is important.
- The PI should include validation testing as a distinct objective and task.
- The future work is focused on linking to other tools, performing scenario analysis and pathway comparisons, and adding additional details to the MSM calculations. These are all useful, but all fall under the category of being either additional functionality or an internally driven analysis.
- It would be good if project work could include some future focus on usability. That would include identifying options to increase the speed of execution and stakeholder training. It was also suggested by one reviewer during the question and answer session to identify through stakeholder interaction how the tool is used.
- The project is linking many models, which could introduce more complexity, and it should be kept simple when in the revision process. The PI could consider focusing on continually updating the MSM model with validation data and updates from the other linked models.
- The future work seems to be mostly a continuation of present and past efforts. However, some of the future work should add important capabilities.

Strengths and weaknesses

Strengths
- The project team possesses excellent technical strength and has made a lot of progress in a very limited time.
- The project's combination of all models into one GUI is very powerful.
- The project's use of their model for continuous evaluation of hydrogen pathways and scenarios is a very strong aspect.
- MSM facilities' connectedness, integration, and consistency in hydrogen analysis efforts make for an excellent collaboration.
- The project could be an excellent planning tool. There are excellent personnel members and facilities.

Weaknesses
- The PI needs to include beta testing with the industry and analysis community.
- The PI needs to define a set of industry users and get them involved.
- The project needs more funding so that hydrogen scenarios can be continually analyzed as the energy economy evolves.
- The project validation is too weak. The PI should work on model credibility.
- The PI needs to ensure that additional functionality is continuing to add significant value. Anything to speed execution would be helpful.
- Even when the project is completed, it is likely to be used by relatively few groups. It's doubtful that the project will actually be completed.

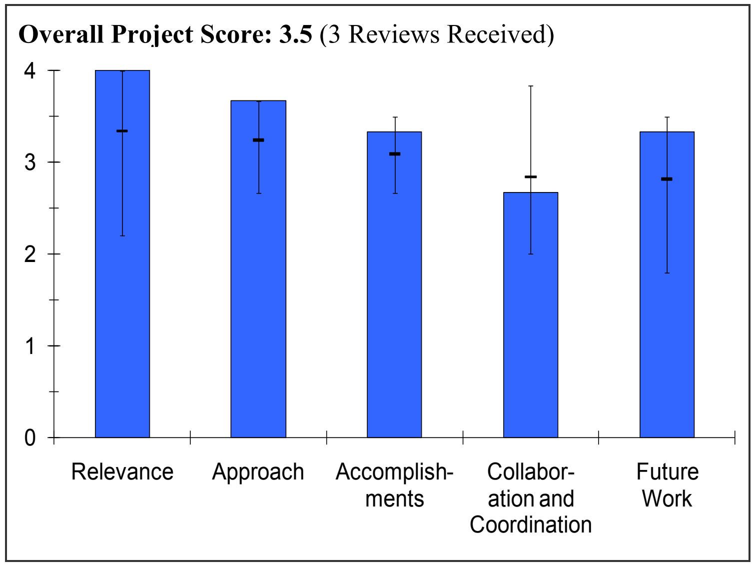
Specific recommendations and additions or deletions to the work scope

- The PI should define a plan for model deployment and dissemination in the technical community.
- The PI should include a training plan within the work scope.
- The project should include more analysis of hydrogen scenarios, such as considering more out-of-the-box ideas.
- See comments under “future work”.
- The PI should determine who are the likely users of the end product and plan accordingly.

Project # AN-12: Life-Cycle Analysis of Criteria Pollutant Emissions from Stationary Fuel-Cell Systems
Michael Wang; Argonne National Laboratory

Brief Summary of Project

The objectives of the project are to: 1) Expand and update the GREET model for hydrogen production pathways and for applications of fuel cell vehicles (FCVs) and other early market fuel cell systems; 2) conduct well-to-wheels analysis of hydrogen FCVs with various hydrogen production pathways; 3) conduct fuel-cycle analysis of early market fuel cell systems to help development of hydrogen production and fuel cell technologies; 4) provide fuel cycle results for DOE OFCT activities such as the Posture Plan and the Multi-Year Program Plan; and 5) engage in discussions and dissemination of energy and environmental benefits of fuel cell systems and applications.



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Note - This review is based on the written presentation alone: This project continues the success that Argonne National Laboratory (ANL) has enjoyed in developing and utilizing the Greenhouse Gases Regulated Emissions and Energy Use in Transportation (GREET) Model. This model is used to estimate the environmental impact, on a "well to wheels" basis, of different potential transportation fuels and the different methods of producing them. The systemization of this information is highly relevant to DOE RD&D objectives.
- Fuel cell vehicle (FCV) and hydrogen are very friendly to the environment. This focus is very important for fuel cell (FC) commercialization.
- The use of GREET is very important to better understand potential reduction of GHG and petroleum, due to both mobile and stationary fuel cell systems (FCS). Any potential improvements to GREET must be considered very relevant.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- Based on written presentation only: The approach of the PI is natural and straightforward. They obtain emissions data on both fuel production pathways and the operation of vehicles (and systems) using that fuel. The data was gathered by various means such as open literature, industry contacts, and other models, such as Hydrogen Analysis (H2A). Then, they incorporate these data into GREET, which is used to simulate scenarios of interest. The presentation noted that the PI "analyze and present" their results, but did not discuss validation to provide feedback to further improve the model.
- It's not clear if the PIs are considering using the U.S. Environmental Protection Agency (EPA) California Air Resources Board (CARB), or South Coast Air Quality Management District (AQMD) data for emissions—such as emissions from steam methane reforming (SMR).
- The relative value of co-products is very important since some sites may not have any value of heat.
- The approach seems to be both appropriate and likely very effective.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Based on written presentation only: This year's major effort was to analyze criteria pollutant emissions of combined heat and power (CHP) and combined heat, hydrogen and power (CHHP) systems, with an eye toward analysis of CHHP as a means of distributed hydrogen generation for transportation. The direction of the analysis (slides 7-11) and the quantitative results (slides 12-16) indicate that excellent progress was achieved.
- Emission of sulfur oxides (SOx), and nitrogen oxide (NOx) for fuel cells seem to be higher than anticipated- (sub-parts per million (ppm) level and orders of magnitude lower than the engines).
- The emissions for electric and hydrogen alone (no heat) should be considered for credit calculation to show the true value proposition of fuel cells.
- The project results are impressive, but it is not clear that they are compatible with time and funding expended. Just in the last two years, over $1.6M has been expended.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Based on written presentation only: By its nature, this work is highly collaborative. The PI reports seeking out information from labs such as National Renewable Energy Laboratory (NREL) and industry stakeholders.
- The GREET model is publicly available on the project website, which encourages its wide adoption and use.
- The PI should consider fuel cell manufacturers' data and the statistical average of emission data.
- There seems to be relatively little actual collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Based on written presentation only: The future work suggested by the PIs (slide 18) seems reasonable and appropriate.
- The emission database from California should be analyzed for greater confidence.
- Future research appears reasonable and would continue to expand the capabilities of GREET.

Strengths and weaknesses

Strengths
- Based on written presentation only, the systemization of environmental impact data in a single model is important when making comparisons between different technologies and policy options.
- Looking at criteria pollutants is an excellent need for externality benefits.
- The project involves making improvements in one of the most useful models yet formulated. The personnel and facilities involved are excellent.

Weaknesses
- Based on written presentation only, the PI should add emphasis to validating, not merely reporting, model results for target scenarios. The lack of uncertainty and a sensitivity analysis makes it impossible to distinguish the precision of the predictions. It’s not clear if it’s accurate within a percent or two, or simply a coarse estimate.
- Inclusion of heat in overall efficiency confuses the outcome when you look at the emissions.
- It is not clear whether there is a reasonable return for funding and time allocated.

Specific recommendations and additions or deletions to the work scope

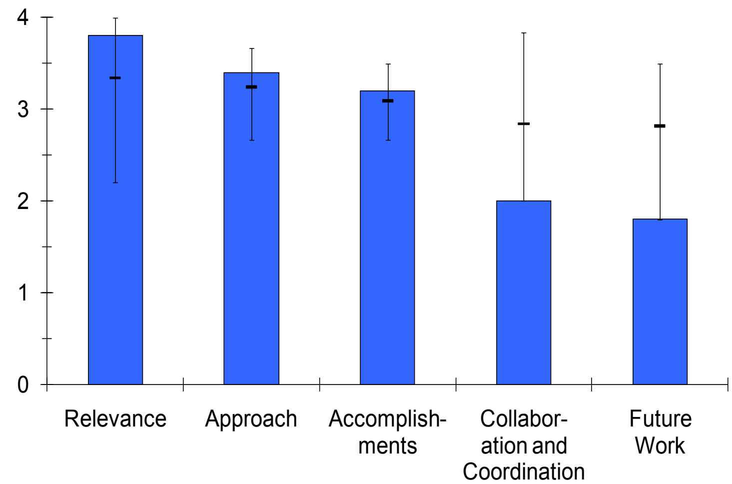
- None

- The PI should consider looking at how to maximize the value proposition using emission data and reviewing the data with CARB or AQMD or EPA.
- It would be helpful if the PI would do some site specific analysis of emissions.
- Perhaps increase expectations relative to funding.

Project # AN-13: CO2 Reduction Benefits Analysis for Fuel Cell Applications

Paul Friley; Brookhaven National Laboratory

Overall Project Score: 3.1 (5 Reviews Received)



Brief Summary of Project

The objective of this project is to perform an analysis of topics of interest to the Fuel Cell Technologies Program (FCTP) related to projected carbon dioxide (CO_2) benefits of fuel cell (FC) applications. The primary tool is the 10 Region U.S. MARKAL model developed by Brookhaven National Laboratory (BNL), which is calibrated annually to the Energy Information Administration Annual Energy Outlook and covers all energy consuming sectors of the United States from resource extraction to end-use. Analyses for fiscal year 2009 and fiscal year 2010 include: 1) sensitivity analysis of fuel cell vehicle (FCV) market penetration to changes in production, distribution, vehicle costs and CO_2 prices; 2) the impact of biomass-to-hydrogen in deep CO_2 emission reduction scenarios, and 3) additional analytical support to respond to departmental data requests and DOE program analysis needs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- The project addresses the impact of the potential CO_2 tax scenarios on the projected production, distribution, and cost of hydrogen-powered vehicles.
- The project studies the possible role of fuel cells in the future energy market where there will be strict controls imposed on CO_2 emissions. This should lead to important insights informing the policy debates on both fuel cells and CO_2.
- The project is important since it supports DOE's understanding of any future benefit based on carbon sequestration.
- The project is very relevant since there is so much concern nationally about CO_2. Hopefully, the many required assumptions will prove to be reasonably valid and not diminish the relevance. Also, it is not clear what the intended use of the output is, and whether it is for political purposes or technical planning.
- The project is supporting the understanding of how fuel cells can impact greenhouse gas (GHG) emissions, and that is critical.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- The project uses state-of-the-art market tools (MARKAL) to model the impact under a wide range of scenarios for the United States. The tool is based on future regulations that to date have not been approved by Congress.
- The project uses scenario-based analyses and BNL's 10 Region U.S. MARKEL model (which was developed in house) that is calibrated to the Annual Energy Outlook to investigate topics of interest to the Fuel Cell Technologies Program. The key feature of the analyses is the prediction of consumer behavior given various differences in cost – capital costs ($/kW), fuel cost ($/kg), and carbon cost ($/tonne). The PI should be commended for analyzing scenarios where DOE target costs are not met, as this makes the analyses potentially more relevant.

- The project approach is good, but it needs to be better integrated into other cost-analysis approaches in the program.
- The approach is very comprehensive and enough sensitivity analysis is performed to allow for many alternatives. However, there is a good chance that the range of sensitivity variables might be too optimistic, especially for the range of hydrogen prices and the range of prices for onboard hydrogen storage.
- The project approach of starting with the CO_2 Cap and Trade policy and the MARKAL model is good.
- The project is evaluating impacts on a national scale that may not be providing the detailed insights that would result if you looked at each region individually.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The project examined a wide range of conditions and assumptions concerning the impact of hydrogen technologies on the projected cost of CO_2 emissions. Important conclusions were on the use of biomass-to-hydrogen, coupled with carbon capture and sequestration (CCS), to reduce the cost of meeting proposed CO_2 emission reductions.
- During the year, the PI modeled a number of relevant scenarios, including those involving carbon caps and carbon offsets and various ways of producing hydrogen fuel (e.g., from biomass and with carbon capture and storage). The results demonstrated that not only would carbon policy make fuel cell technology more widely used, but also that this substitution effect would help reduce the CO_2 market price (under cap and trade).
- The project sensitivity analysis was good, although many of the costs involved have not yet been attained.
- The PI needs to see the effect of wind-electrolysis and other carbon-zero approaches on the analysis.
- The project biomass analysis needs to account for non-fuel drivers to market.
- The PI should perform a CCS sensitivity analysis that includes efficiency variability and leakage variability from reservoir (often quoted as 1% per year).
- There are good results, but the project is open-ended and has been in progress since 2007.
- They produced a very complete evaluation of a large number of scenarios with varying carbon tax.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- The PI presented limited data on collaborations.
- The PI reports interactions with DOE staff and other analysts from national laboratories. Since the project is primarily an econometric model, the minimal interaction with the technical (engineering) community is not a problem. However, the results could perhaps be better vetted by increased interaction with the academic community.
- The project included very little collaboration and needs input from other cost analysis projects, industry, and especially government stakeholders.
- There seems to be little collaboration with anyone outside BNL and DOE.
- This project has virtually no collaboration. It might benefit from a partnership with the stakeholders that will be paying the carbon tax.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- The PI provided limited discussion on future activities.
- Proposed future work was not explicitly laid out in the presentation. It is presumed that the PI will continue to analyze scenarios as proposed by the DOE.
- Future work was not delineated, but it should be expanded from biomass.
- There were few specifics. The plan seems to be for the project to continue open-ended with periodic updates.
- The project has no future plan currently.

Strengths and weaknesses

Strengths
- It's an important project used to understand the role of hydrogen-based technologies and their potential impact on proposed CO_2 reduction legislation.
- The project provides useful insights into the possible outcomes and effects of aggressive GHG (CO_2) policies.
- The reduction programs, as shown in the conclusion (slides 11-18), were very beneficial.
- Their sensitivity analysis to CO_2 reduction based on costs was a strong aspect of the project.
- There are excellent personnel involved and good execution by them.
- The project presents a very good analysis of the impact of CO_2 tax on hydrogen price.

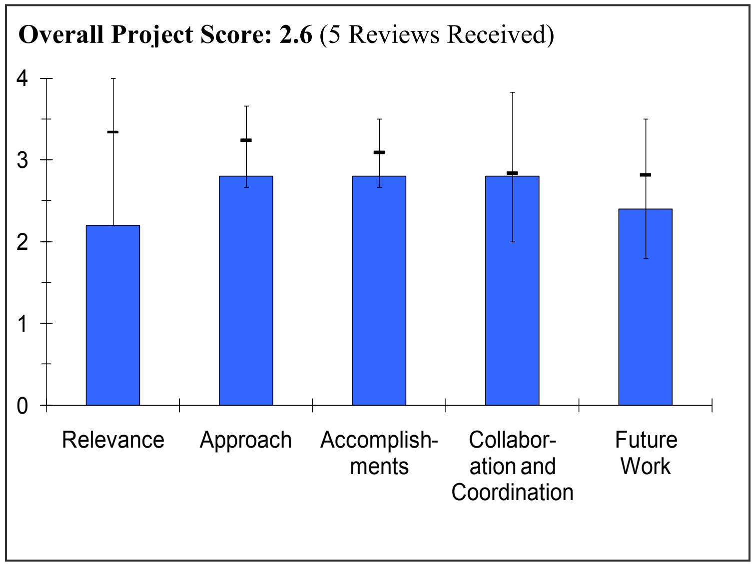
Weaknesses
- The projections are based on large uncertainties.
- As with all modeling and simulation efforts, the results are somewhat speculative and subject to overly precise interpretation. The results could be best interpreted by seeing what variation in results might arise were the research to engage independent academic economists as well.
- There are too many assumptions on biomass CCS technologies, and the project needs to be expanded to other methods of hydrogen production and also needs stakeholder input. They should consider multiple government CCS legislative approaches.
- There is no clear project end or intent for utilization. There seems to be an expectation for the project to continue open-ended.
- It's not clear who would use the results of this research. If it is the producers of hydrogen, then they should be involved in the project.

Specific recommendations and additions or deletions to the work scope

- None.
- Add other hydrogen production technologies. Consider other CO_2 reduction approaches than CCS, such as sustainable biomass production and carbon-zero approaches to hydrogen production.
- Determine by whom and for what purposes the project results are intended. Also, the range of sensitivity variables should be increased.
- Identify who the user of this work will be, and then get them involved and answer their questions.

Project # AN-14: Pathways to Commercial Success: Technologies and Products Supported by the HFCIT Program

Steve Weakley; Pacific Northwest National Laboratory



Brief Summary of Project

The objective of this project is to assess Fuel Cell Technologies (FCT) Program benefits by tracking the commercial success of technologies developed by FCT and FCT predecessor programs and by estimating their impacts and benefits. The milestones for 2010 are to: 1) update the FCT technology tracking database containing information on commercial and emerging technologies, and 2) update the FCT report on the status of commercialized and emerging technologies and patents.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- The project objective is to document and compile a list of patents that were supported wholly or in part by the DOE. It is hard to understand how the act of compiling a list of patents addresses goals of the DOE R&D plan.
- The project is relevant to assess the technology and commercial benefits of the work funded by the DOE over the years. However, this work should not be subcontracted. It should instead become a routine part of DOE's own work where they can monitor the progress of funded technologies.
- The DOE should have internal assessment platforms and criteria that are quarterly reviewed to see how progress has been made in terms of commercial deployment of funded technologies over the years. Perhaps this project is trying to tackle that, but it's not a research activity for a merit review.
- The project is good for information sharing not for merit reviews.
- This is a "back end" project to research and quantify, retrospectively, the impact that federal funding had in spurring energy innovation. The metrics used are patents and commercialized products. This is essential information in the research cycle, as it both informs appropriators of the merit and the effect of federally funded research and informs program managers as to what characteristics of proposals ultimately might lead to worthwhile (desired) outcomes.
- While it is often useful to have composite information on commercialization, the big issues that remain to be solved include many technology problems associated with the fuel cells, on-board storage, and also production and delivery.
- Understanding the benefits and being able to show commercial products flowing from the program can become critical for continued program funding.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- The PI performed a fair job on compiling a list and following through on patents that were actually used commercially.
- The project consumed a large budget for this assessment activity. This should be a routine tracking activity within the DOE. The budget should be significantly reduced.
- There is nothing new, different, or extraordinary about the project approach. It is very similar to the routine competitive assessment done in the industry (at much lower budgets).

- The PI surveys the patent literature and interviews the DOE headquarters staff and patent holders to develop a database of commercial and emerging technologies. This database is actively maintained and updated annually.
- Slide 3 is a bit controversial, because it's not that patents are an alternative to "Further Development by Industry," as drawn. Rather, it is preceding patent protection that encourages "Further Development by Industry," by allowing a firm to capture the benefits from what resources they put into the "further development."
- The project approach is generally okay, but it was almost doomed to failure from the beginning due to no apparent incentive by participants to cooperate or to be accurate.
- Conducting a patent search and then going to the organizations that own those patents to follow through is the right way to go.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The PI identified more than 100 patents that were funded by the DOE. The presentation was followed by a lively discussion indicating interest in the topic.
- The project cannot be differentiated from a routine competitive analysis-type work that industry does at a lot lower budget. The project progress relative to the budget is poor.
- The project is not a technology activity but more of a programmatic assessment activity, and it should be out-of-scope for merit reviews.
- The major accomplishments of this project are its data mining, data aggregation, and results reporting.
- It is particularly noteworthy that the project reports in multiple formats: a polished, peer-reviewed printed as a PDF report, posted for public use (e.g., on the web), and in database form for in-house DOE use. These products are updated annually.
- Even though there are top-notch people working on the project, the accomplishments are modest, especially considering the time and funding dedicated to it.
- The search and documentation of the results are on par with the funding expended.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- The level of collaboration was limited to discussions with patent holders and follow-through on commercialization. There was no collaboration on accelerating or assisting patent holders on commercializing technology.
- The project entails fair collaboration with DOE staff and industry proof of concepts (POC).
- By its nature, this work is highly collaborative. The PI seems to be doing an excellent job of seeking out information from former grant recipients and convincing them to voluntarily contribute to his research (approximately 81 sources reported).
- There is little collaboration other than contacting and trying to question previous contractors.
- Virtually all hydrogen and fuel cell researchers funded by the program are given collaborators.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- The project's future research should be more of the same.
- The project is routine programmatic work with nothing new or different, which should be done continuously within DOE and not outside of it as a separate project.
- The future direction of this work arises naturally. Continue to search the patent database for newly issued patents, and continue to actively monitor (query the companies about) the business activity of former grant recipients.
- The future work seems to involve doing more of the same things that they have been doing.
- The project output needs to be updated on an annual basis.

Strengths and weaknesses

Strengths
- The project identifies success stories as measured by patents. It provides one metric to demonstrate success of fuel cell projects to DOE management and Congress.
- None. It is just an essential activity that DOE should be doing to track the progress.
- This project provides essential and highly desired information to policy makers about the impact of federal R&D spending.
- There are excellent personnel members involved.
- The project provides a communication tool to show that, although we have not moved to a hydrogen economy, the nation and economy are benefiting from the program's research.

Weaknesses
- The project focuses solely on patents and does not address invention disclosures, industrial collaborations, publications, etc., as metrics for progress.
- The project is not relevant to any specific technology developments or new research. This is mostly a programmatic activity that should not be merit reviewed.
- It's recommended to make the starting point of the survey the list of former grant recipients rather than the patent record. This might provide a greater volume of information concerning commercial products protected by trade secrets rather than patents and about commercial products with pending patents.
- This project is not, in and of itself, advancing hydrogen and fuel cell technologies.

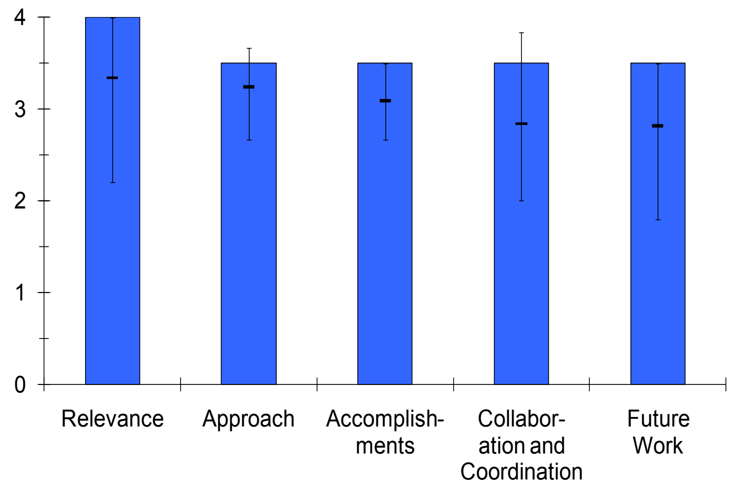
Specific recommendations and additions or deletions to the work scope

- This project should not be considered a research project.
- The project should be deleted from the merit review, as it is not relevant for technical assessment. It's a DOE programmatic activity and one hopes that funding is not taken out of research dollars.
- Since many grant recipients were universities and labs operated by universities, it might be interesting to study the effect of Bayh-Dole on the energy innovation process.
- They should use national labs for help resolving the many technical issues remaining and pass this type of project to a different type of organization.
- It would be helpful to identify how the commercialization successes of this program compare to other programs.

Project # AN-15: Fuel Cell Power Model: Evaluation of CHP and CHHP Applications

Darlene Steward; National Renewable Energy Laboratory

Overall Project Score: 3.6 (2 Reviews Received)



Brief Summary of Project

The objectives of the project are to: 1) accurately model performance for stationary fuel cells (FC) in combined heat and power (CHP) and combined heat, hydrogen and power (CHHP) applications and 2) combine detailed performance information with a comprehensive discounted cash flow methodology to evaluate lifecycle costs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The purpose of this project is to model the economics of CHP and CHHP systems to see, among other things, whether CHHP systems might be a practical source of distributed hydrogen production. The question to be answered is whether CHHP systems, which are stationary fuel cells servicing large buildings, might be able to also produce hydrogen fuel for transportation applications. Since this is a transition strategy of interest, the project fully supports DOE objectives.
- The project could become a valuable tool for potential early adopters of CHHP to evaluate the economics of the system.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- The project approach is to first model the technical performance of stationary fuel cells suitable for CHP and CHHP operation; and second, to overlay this with the best available cost data to determine discounted cash flow lifecycle costs. Among other things, the PI developed a simplified system model for molten carbonate fuel cells (MCFC) in order to execute this approach. It is inherently difficult to validate both the technical performance and the cost data in such early-stage technology and to predict how costs might trend down over time. Nonetheless, the PI has made a reasonable effort through numerous interactions with staff at DOE labs, universities, fuel cell companies, and organizations where CHP and CHHP systems have been installed.
- The project should primarily support those looking at adopting the CHHP option, based on their location and available infrastructure, resource, utility costs, incentives, etc. The project should help determine how much and when one should be producing heat, hydrogen and power to maximize return on capital investment.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- The PI developed the model to provide the analysis results sought. Slides 13 and 14 compare the cost of hydrogen production from CHHP systems and small-scale steam methane reforming (SMR) systems, an alternate technology choice. The poster also demonstrated other results of the model. An example is the economics of using a CHP system to derive value from dairy farm waste.
- The model appears to assume a relatively low capital cost for the system and an unrealistically low internal rate of return (IRR). Different end-users have different business models, evaluation tools, and drivers for adopting

new, unproven technologies. The model should allow as much flexibility for users to input these numbers as possible.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- This project was highly collaborative with long lists of reviewers and partners on slides 2 and 12.
- Good collaborations with the National Renewable Energy Laboratory (NREL) hydrogen analysis team and other entities. The PI should consider continually validating the model with feedback from industry and available data from real-world testing and operations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- The PI describes a number of fruitful activities as proposed in slide19. Most importantly, the PI intends to continue to "modify and enhance" the model as further real-world experience is gained. Further, the PI intends to study solid oxide fuel cells (SOFC) and residential-sized systems and also better integrate his model with a Macro-System Model (MSM) and Scenario Evaluation and Regionalization Analysis (SERA).
- The PI should consider other renewable sources outside of wind and solar.

Strengths and weaknesses

Strengths
- The project has important practical value, since it's providing a tool to enable potential early adopters' means to predict the costs of installation and operation of specific fuel cell systems (FCS). Similarly, it provides means to predict the costs of the proposed CHHP strategy of distributed hydrogen production for transportation applications.

Weaknesses
- None noted. However, as with all modeling efforts, one might potentially extrapolate incorrect conclusions based on the limited data.

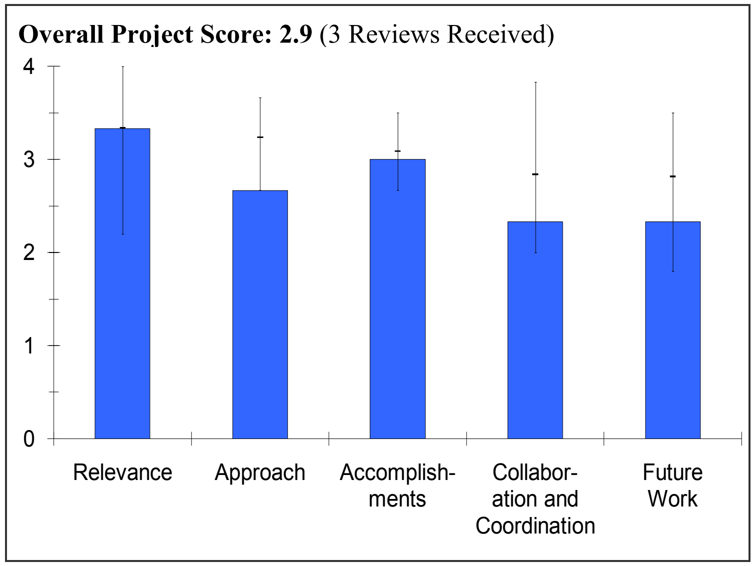
Specific recommendations and additions or deletions to the work scope

- None.

Project # AN-16: Geospatial Analysis of Hydrogen Production Pathways
Matt Kromer; TIAX, LLC

Brief Summary of Project

The objectives of the project are to: 1) develop a tool (the Hydrogen Logistics Model) to compare hydrogen production pathways and policy options; 2) compare production pathways using a single common framework to input assumptions consistent with other hydrogen analysis tools, account for geographically sensitive characteristics, and offer flexibility to test a variety of input assumptions, and 3) perform a scenario analysis to identify low-cost hydrogen production pathways at demand centers across the United States, and characterize the effect of monetizing carbon emissions, varying hydrogen demand scenarios, and economic inputs on hydrogen price, resource utilization, and carbon dioxide emissions.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- The project has a useful purpose and can assess the viability of hydrogen to reduce emissions. However, it's not clear that the cost input information is consistent with other models.
- The project appears to be relevant to DOE goals; however, it seems to be taking a somewhat slow stepwise approach that appears to lead to more contracts. The general picture of what the PI can really do is not clear, and the PI needs to address this rather than addressing smaller sections.
- Hydrogen resources and demand are very important parameters for fuel cell vehicle (FCV) and infrastructure development.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The PI mentioned some of the project barriers but did not really address the way to overcome them.
- The project approach has been reasonable given the small amount of money. Validation of the tool is now a priority, and this seems to be absent.
- The project is presenting good considerations of hydrogen resources.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Progress has been made as the model is close to completion. However, the accuracy of the model is unclear.
- The PI's comment on carbon mitigation costs seems quite optimistic at less than five percent of hydrogen cost.
- The delivery cost of over \$3/gge also sounds high.
- The project progress is reasonable for the funding.
- Hydrogen delivery cost comes out to be more than 50% of hydrogen cost. This indicates that on-site production is a favored pathway.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- The project has no external partners to validate and provide input. It's not clear if companies actually make the investment needed.
- The project collaboration is definitely not good enough. They really should have more players involved.
- The project may benefit more by including additional stakeholders such as large hydrogen suppliers.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- The project is effectively complete. Other models seem to address some of the work that is proposed for future research, so it is not clear if it's needed.
- The project is ending and proposed future work is not overwhelming. Increasing the project funding is in doubt without a large increase in the collaborative net and pathways for validation. It's not clear how to confirm that the project is correct.
- The PI should consider a transition strategy for the project.

Strengths and weaknesses

Strengths
- If the model is used, then it could be useful.
- They have a good list of hydrogen sources.

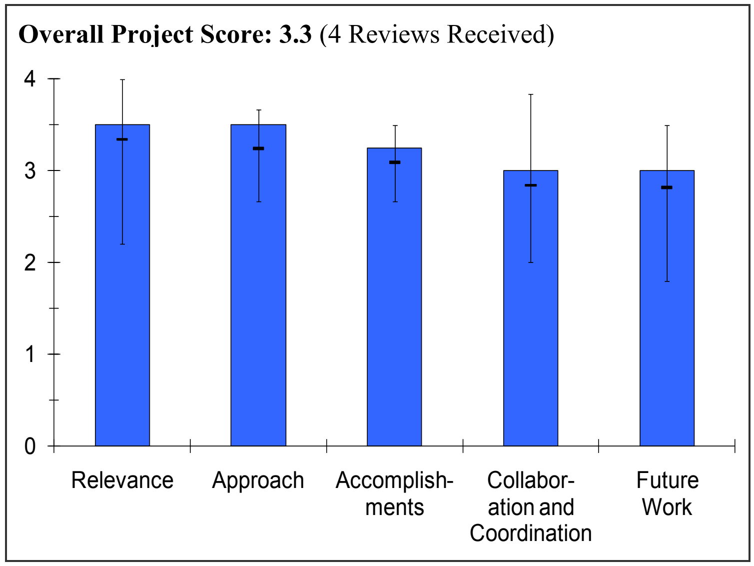
Weaknesses
- The model validation is weak.
- The carbon capture analysis methodology and impacts are not clear.

Specific recommendations and additions or deletions to the work scope

- The project is effectively complete.
- The project should have much more collaboration with stakeholders in future stages.
- Higher delivery costs of hydrogen, as identified in the analysis, lead to on-site hydrogen production. CHHP using biogas and/or natural gas offers a promising option to investigate.

Project # AN-17: Recent Developments in the Hydrogen Demand and Resource Assessment (HyDRA) Model
Johanna Levene; National Renewable Energy Laboratory

Brief Summary of Project



The transition to hydrogen requires an understanding of the spatial relationships and interdependencies of a wide range of changing datasets. Estimating hydrogen demand, finding and organizing resources, and designing, building, and managing the hydrogen production and distribution infrastructure all require spatial and temporal modeling and analysis, which require and produce spatial and temporal datasets. Hydrogen Demand and Resource Analysis (HyDRA) is a repository for spatial demand and resource and infrastructure data related to hydrogen. Data are provided in maps and via model integration. The objectives for fiscal year 2010 are to: 1) implement functionality to support further interoperability between HyDRA and the Scenario Evaluation, Regionalization and Analysis (SERA) model; 2) develop a process for automatically updating SERA input data in the HyDRA application on a regular basis, and 3) implement automatic updates of data and prototyping of the exploration of temporal and multivariate datasets.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project likely aligns well with the DOE's objectives to evaluate hydrogen as an alternative fuel.
- Spatial data is essential to determining localities to implement hydrogen infrastructure.
- HyDRA supports program goals related to sharing of data, developing common assumptions and platforms, and it also appears to be widely used within the community.
- Web tools are very useful for potential users of hydrogen infrastructure and fuel cell vehicles (FCV).

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- The project seems impressive with regard to the amount of connectivity from various models.
- The project's spatial data display was integrated with over 90 databases. It addresses multiple technical barriers well.
- The project's overall approach is good. It uses a well-designed Web-based interface to enable visualization and the transfer of geospatial data and integration with other tools. The team developed an initial platform several years ago and has incrementally added functionality over time that is focused on improved usability and integration with other tools. The use of open, Web-based tools facilitates communication and collaboration.
- They employed an excellent strategy to connect the user's needs with available data.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- If the modeling data can truly be automatically updated, that is a plus for its usefulness and longevity.

- The model has a growing number of new users.
- The project has a growing integration with other models including Marco-System Model (MSM) and SERA.
- The project is working on new layers.
- Significant progress was made this year toward adding functionality to enable collaboration and integration with other analysis tools.
- The model's Web-access features are very desirable.
- Hydrogen price and greenhouse gas (GHG) emissions by county are useful in the future decision making process.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- The project challenge is to get meaningful feedback on issues that people are willing to share. The only partner shown is National Renewable Energy Laboratory (NREL), although it appears as if there is a lot of Web-based feedback. It's not clear how effective that Web-based feedback is.
- The PI needs to present better information on the project collaborators to clarify if the project is serving all communities well.
- The project collaboration is very strong. Much of the work during the last year focused on improved integration with other tools and improving the usability of the tool. It is now able to accept data from external applications and export data for use in other applications.
- The PI presented a very good list of university and industrial partners.
- The project is cross cutting among different agencies such as the U.S. Environmental Protection Agency (EPA), Federal Energy Management Program (FEMP), the DOE Fuel Cell Technologies Program, Clean Cities , Vehicle Technologies Program, Office of Biomass Programs (OBP), and Solar Technologies Program.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The PI should also work to validate the model and see if businesses would use it to make decisions.
- Increasing reliability and functionality of this model can only be a good thing.
- The ability for third party users to more easily integrate with the model will be useful.
- The project is presenting a very useful model that can be used by different stakeholders.

Strengths and weaknesses

Strengths
- The project's spatial data representation with other models and a large database is a strong aspect.
- The collaborative work has focused on improving coordination and accessibility with other contributors in the analysis community.
- The visualization tools and overall design for usability are very good.
- Automatic update features will allow the model to be useful for many years in the future.
- Involving many stakeholders in the project is very good.

Weaknesses
- It's doubtful if the data would actually be used by a business with the thought of building infrastructure.
- The PI should consider more collaboration.
- It's recommended to further identify how end-users use the tool.

Specific recommendations and additions or deletions to the work scope

- It's recommended for the PI to get feedback from companies who would actually build infrastructure to see if this analysis would be used to make decisions.

- The PI should include cases where the model was used to demonstrate how the tool can be or is used to manage hydrogen infrastructure development and communicate information. HyDRA is primarily a tool that enables other analysis activities, so it is difficult to ascertain exactly how it is used within the community without seeing example results from end-users.
- A joint meeting or a Webinar of stakeholders to review the model and its application will be very productive. The meeting should include industry, EPA, Office of Biomass Programs, Vehicle Technologies Program, etc

2010
American Recovery and Reinvestment Act
Summary of Annual Merit Review of American Recovery and Reinvestment Act Activities

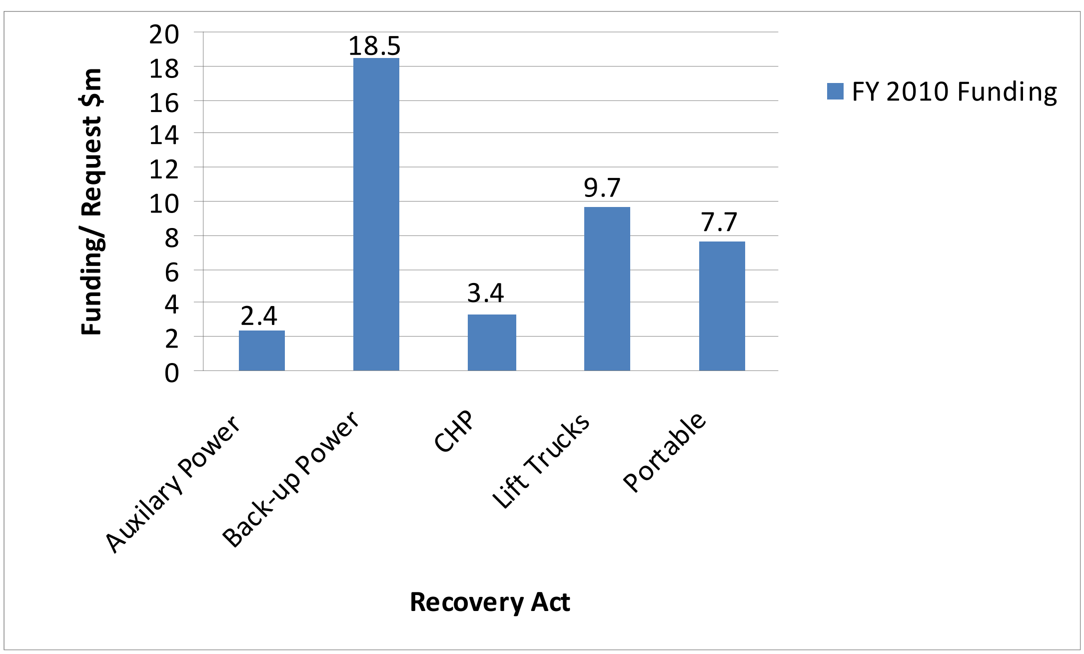
Summary of Reviewer Comments on American Recovery and Reinvestment Act Projects:

This review session evaluated fuel cell market transformation projects funded under the American Recovery and Reinvestment Act of 2009 (ARRA). The ARRA projects are generally considered to be well aligned with the goals and objectives of ARRA and the DOE Hydrogen Program.

The ARRA projects include the development and deployment of a variety of fuel cell technologies including polymer electrolyte, solid oxide, and direct-methanol fuel cells in auxiliary power, backup power, combined heat and power, lift trucks, and portable applications. Overall, the projects were judged to have made progress toward development and deployment goals. Reviewer concerns and recommendations varied by project and are summarized below.

ARRA Funding by Technology:

In April 2009, DOE announced the investment of $41.9 million of ARRA funding to accelerate the commercialization and deployment of fuel cells and to build a robust fuel cell manufacturing industry in the United States, with accompanying jobs in fuel cell manufacturing, installation, maintenance, and support services. Twelve grants were competitively selected and awarded to develop and deploy a variety of fuel cell technologies. These projects address the objectives stated above as well as the overall ARRA goals to create new jobs and save existing ones, spur economic activity, and invest in long-term economic growth. The cost share provided by the project teams is approximately $54 million, which represents over 56% of the total cost of the projects.



Majority of Reviewer Comments and Recommendations:

All twelve of the ARRA projects provided oral presentations. Three of the projects were not reviewed due to relatively late starts in FY 2010. The remaining nine were reviewed. In general, reviewer scores for the ARRA projects were very good, with scores of 3.6, 3.2, and 2.6 for the highest, average, and lowest scores, respectively. Six of the nine projects had a score of 3.1 or higher. The scores are indicative of the technical progress that has been made since the project grants were awarded in late FY 2009 and early FY 2010. Recommendations and major key concerns for each project category are summarized below.

Auxiliary Power: One project in this area was reviewed, and its collaborative efforts received favorable comments. The project is leveraging the Solid-state Energy Conversion Alliance (SECA) program to implement an aggressive on-road test program for an auxiliary power unit (APU). Reviewers commented that diesel APU systems are a key early market fuel cell technology with the potential for substantial impact on U.S. manufacturing and air quality. The reviewers recommended that deployment to the field test be accelerated since this will identify additional opportunities for product improvement. They also recommended that the project team involve the Department of Defense (DoD) in the demonstration phase to help identify other applications that could be beneficial for military use.

Backup Power: Two projects addressing 72-hour backup power for cell phone towers and DoD sites were reviewed, with both projects recognized for their statements on jobs retained and/or created with the ARRA funds. The reviewers recommended re-examining fuel choices for the various backup power applications, such as using alternative hydrogen delivery solutions (e.g., on-site methane reforming) depending on the site, and re-examining the possibility of operation on hydrogen for 72 hours; they also suggested that the economic trade-offs of using a battery for initial start-up versus using a hybrid hydrogen/LPG system should be considered. Additionally, project teams were advised to work on increasing public and market awareness of their products.

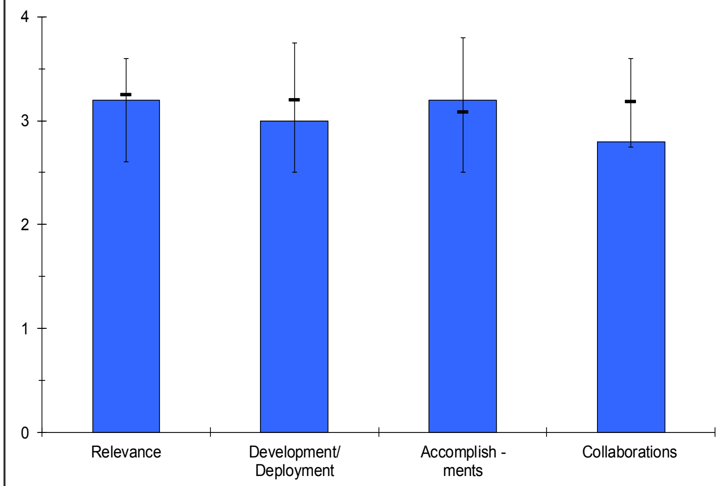
Combined Heat and Power (CHP): One project in this area was reviewed. The reviewers suggested that there is widespread opportunity for this application, particularly in California, and they remarked that a strong partnership has been assembled. The reviewers advised the team to focus on cost and performance and competitiveness with other CHP technologies, including other fuel cell CHP systems. The reviewers also suggested additional bench testing in addition to field tests, in order to accelerate the durability verification process.

Fuel Cell Powered Lift Trucks: The projects in this area were generally highly rated. At of the end of the 3rd quarter of FY 2010, a total of 206 fuel cell lift trucks had been deployed into a significant cross-section of the U.S. economy. This achievement is well aligned with the ARRA objectives. These projects have the potential to accelerate the commercialization of fuel cell lift trucks—leading to large-scale market adoption—and to benefit the hydrogen fueling industry. Reviewers encouraged the project teams to continue to monitor and evaluate fuel cell lift truck performance, document lessons learned, and clearly identify the value proposition for future deployments. It was also recommended that the projects complete additional economic analyses to characterize fuel cell life and to validate or re-evaluate the estimate that a market deployment of 1000 units will drive fuel cell costs from $3,600/kW to less than $2,000/kW (David Greene, ORNL, 2008).

Portable Power: One project in this area was reviewed. The reviewers recognized the potentially large consumer market for portable fuel cell applications but advised that the technology be presented to niche markets first to gain acceptance. There is significant competition offered by incumbent technologies such as battery recharging. Additional performance analyses and lifetime predictions for comparison to batteries and other competing technologies were recommended.

Project # ARRA-01: Commercialization Effort for 1W Consumer Electronics Power Pack
Chuck Carlstrom; MTI Micro Fuel Cells, Inc.

Overall Project Score: 3.1 (5 Reviews Received)



Brief Summary of Project

The overall objective of this project is to demonstrate and field test a commercially viable one-watt direct methanol fuel cell (DMFC) charger for consumer electronic devices. The objectives are to:, 1) reduce the cost to attain a competitively priced product, 2) complete design for manufacture and ease of assembly, 3) demonstrate performance across a range of environmental conditions, and 4) conduct a user field test of 75 fuel cell (FC)-powered chargers.

Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.2** for its relevance to ARRA objectives.

- This project showed an excellent return rate on jobs sustained and/or created. However, keep in mind that at the moment it was presented, these jobs are not necessarily sustainable jobs because of the start-up nature of the company and the resulting product.
- This is an impressive product. I’m looking forward to seeing an operational unit (and data) at the 2011 AMR, together with an overview of user feedback, manufacturing ramp up (and additional generated work and/or jobs at supplying companies), additional attracted investment funding (from any source), and market sales.
- Charging of consumer electronics is an outstanding market, given the large numbers of personal digital assistants (PDA), cellular phones, and BlackBerry®-type devices being used. If FCs can penetrate that market to a significant degree, it will create skilled jobs and improve FC performance through scale production. It is still questionable whether FCs can compete with other chargers (i.e., lithium battery chargers). This is especially true regarding cost. All the job growth is predicated on FCs displacing a very attractive, incumbent technology, even if it takes longer to charge with batteries. Many people use the nighttime to charge these devices and don't experience a negative impact.
- This project evidently had a good effect on MTI's employment, and the portable charger appears to be a good potential business for the future.
- It’s important for U.S. leadership to support development of a micro fuel cell product given activity by other countries.

Question 1b: Relevance to overall FCT ARRA objectives

- This research has definitely shown that ARRA funding sped up the development process of this product (and attracted outside investment). The process has not yet reached the mass or large-number manufacturing level. It’s understandable that the product has not been introduced to the market based on the status of the product development process.
- Next time, there will hopefully be a large number of units either being manufactured or released in market!
- Next time (not applicable on 2010 AMR), show what jobs are created and /or maintained at suppliers and those companies that develop to support peripheral needs.
- This type of project focuses on large scale production of FCs, which is very important to this technology. It also addresses a market that, if successful, could translate into many jobs. Given the global use of these consumer electronics, it could also help grow the export of FC products. The fact that this project saved 14 jobs -- which

were probably all at MTI -- may have been the only thing to keep a domestic fuel cell technology development effort in this application area alive.
- One element could be strengthened, which is to enhance the acceptance of the FC as a portable charger. The presenter did not make clear how this acceptance was facilitated by the project. For example, there is a barrier that is the change of concept from charging to having to buy cartridges. It will be interesting to see if the consumer is willing to buy these in a store or online and keep them on hand. If they run out of cartridges, they are stuck; whereas a larger battery could be charged somewhere.
- The relatively small number of units (75 units) and the small power level (one-watt) will not achieve FC commercialization, but it's a small step in the right direction.
- While I strongly support the technology, I question that the consumer market is being targeted. The combination of a relatively low energy density in the DMFC product and the initial cost is going to make consumer market penetration difficult when going up against the incumbent battery charger products.

Question 2: Development and Deployment Approach

This project was rated **3.0** based on development and deployment approach.

- It is past the idea, seed, and prototype development phases, but there are still significant challenges ahead, which will become clearer when doing (or after) the field test of 75 units.
- For the 2011 AMR, make sure to compare this one-watt fuel cell power unit to products with competitors like all Li-ion (lithium-ion) products and regular battery packs for recharging.
- The technical and deployment milestones were sound ones. Having these 75 units used by people outside of MTI will give objective feedback on their performance. Testing the units before and after they go through their trial phase is also important and a good approach. The slides did not go into enough detail about who they were targeting, how long the testing is expected to last, or what type of MTI engagement with the customers will take place. One would think the customers would have been identified by now. Working with the Methanol Foundation is a wise plan, as there are clearly some codes and standards issues that need to be addressed.
- The project appears to be well managed and executed in a timely manner.
- The team had a good technical approach with a clear focus on achieving the objective.
- There was not much information on cost approach, such as the projections for manufacturability and reducing the cost down to what the market requires. The project could be a technical success, but it may not go anywhere beyond the limited demonstration units because of the cost of the product.
- The project is outstanding in terms of technical progress. However, there was not much discussion on the competitiveness of the product versus incumbent battery charging products in the consumer market.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **3.2** for technical accomplishments and progress.

- Next time, the PI should include jobs that are created and/or maintained by paying contractors to build specific parts for product. This translates into hours worked as contractors that can be credited to the funding your company received.
- The passing of the first go/no-go point was a good sign. The testing results appear to be very promising, both in the environmental testing and performance testing areas. The manufacturing maturity plan is also good; and will drive down cost.
- There could be a better definition of the cost progress in total and the comparative analysis to technologies such as batteries. Also, the effect of operating versus capital cost should be a bit clearer.
- The technical progress to date is good. The operating lifetime appears to be adequate, but the calendar or shelf life of the system is not certain.
- This is more of a product development project rather than an ARRA-type deployment project.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **2.8** for collaborations.

- It appears good, but perhaps they could consider reviewing rechargers.
- The PI should see if component suppliers can achieve additional reduction of labor cost and cost of components.
- The collaboration with the Methanol Foundation is fine, but there doesn't appear to be a lot of effort in recruiting the 75 users for these devices.
- The collaborators might consider companies (i.e., tool vendors) that know how to make high-volume consumer electronics.
- This project shows good collaboration with the Methanol Foundation and with component suppliers.

Strengths and weaknesses

Strengths
- Several strengths include the power density, current performance of FCs, the packaging, and the fact it lasts as long as seven to ten mobile device recharges.
- This product has a potentially large consumer market. The testing regimens are very sound. Building quantities of 75 units provides a large enough data set from which to evaluate consumer acceptance.
- This project was executed in a timely manner, which saved several jobs.
- The project is clearly focused on a specific niche application.
- The technical progress in the micro market segment is strong. This is an area where the United States is behind the progress of other countries.

Weaknesses
- It is an unproven product.
- The PI should focus on finding the niche market that definitely will use the product, because the amount of power it provides is comparable to seven to ten recharges of mobile electronic devices.
- Logistics fuel distribution is a weakness.
- The PI could be up against a tough incumbent technology in battery recharging. MTI has not identified the 75 users of their devices as of yet.
- There is a lack of clarity with respect to cost, even on a percentage basis.
- Getting the costs down enough to make the device affordable to a widespread market is going to be a challenge.
- It is not clear how this project will continue after the ARRA subsidy is depleted. Venture capital investment may be a way to go to the next needed level of commercialization.
- Until the data are presented to show cost and performance competitiveness versus incumbent battery chargers, The PI might view a successful market entry for a consumer product with some skepticism.

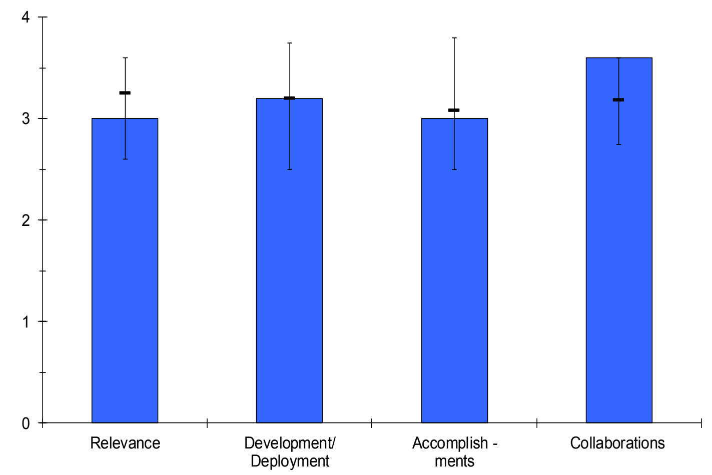
Specific recommendations and additions or deletions to the work scope

- The PI should work really hard (below the radar) on presenting this product to niche target markets and make sure that users have easy access to either rechargers or available refills. Even though the Li-ion brick doesn't last as long, it can be recharged in a hotel room or any other electric plug, assuming the price is similar or lower.
- Consider exploring the potential to make the unit even smaller.
- Based on degradation numbers, the PI should attempt to make a prediction of the unit's lifetime and compare that to batteries or competing products.
- Make clear how acceptance barriers are being addressed, and consider adding manufacturing prowess to the sub-tier team. Perhaps there are high-volume manufacturers in Michigan or other depressed areas that could help.
- Seek a partner in consumer electronics or public relations to help with the marketing side of the devices.
- The PI should continue to work towards Underwriter Laboratories (UL) or Canadian Standards Association International (CSA) certification and get the product on the U.S. General Services Administration (GSA) schedule.

- When the demo units are ready, get MTI invited to demonstrate the units to an inter-agency working group meeting. Not only could these agencies be customers, but they also may be able to identify commercial or military applications for this product, such as power packs for remote sensors or unmanned autonomous vehicles (UAVs).

Project # ARRA-02: Solid Oxide Fuel Cell Diesel Auxiliary Power Unit Demonstration
Steven Shaffer; Delphi Automotive

Overall Project Score: 3.1 (5 Reviews Received)



Brief Summary of Project

The overall objective of this project is to dramatically increase both the technical and commercial viability of fuel cell (FC) auxiliary power unit (APU) technology. Objectives are to: 1) define system specifications and commercial requirements, including subsystem requirements, and develop a subsystem requirements document, 2) design, build, and test the diesel APU system, including verification testing of APU subsystems, form and packaging redesign, plus APU system vibration analysis, and 3) perform a one-year vehicle demonstration and data analysis.

Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.0** for its relevance to ARRA objectives.

- The program continues an important effort in the United States, which is, in fact, differentiated from other solid oxide fuel cell (SOFC) programs around the world. This is an application that the United States may be the technology leader in and the United States should pursue it aggressively. The number of jobs created and/or retained, albeit small ones, are good jobs in a part of the country where stimulus is very much needed.
- This project saved approximately five engineering jobs. Future manufacturing of this product could create jobs and improve the APU industry.
- This is a key, early market, FC product and technology. Success of this product would have a substantial impact on U.S. manufacturing and also mitigate a serious air quality issue.
- The effort has created approximately four full time equivalent (FTE) jobs and certainly has contributed to the maintenance of others. As the basis of a future commercial product, it contributes to long-term growth.
- The presentation didn't address the ARRA objectives. In the question-and-answer period, the PI responded that five jobs were created, which is low for $2.4 million.

Question 1b: Relevance to overall FCT ARRA objectives

- From a technical point of view, focusing on vibration and successful operation in the actual application seems entirely appropriate.
- This project is creating a product that will fill a niche that supports both environmental needs and may have economic impacts on the trucking industry. This project is accelerating both the commercialization and installation of this type of FC technology.
- This project is directly impacting the speed of commercialization of the planar SOFC technology into a meaningful market. Furthermore, it's a demanding application. Success here will open up other markets for the technology.
- This market is challenging and may be difficult to enter near term, but this project will advance the technology.

Question 2: Development and Deployment Approach

This project was rated **3.2** based on development and deployment approach.

- There are some positives and some negatives to this, which balance the score. The weakness is the length of time to develop the required documentation. It doesn't seem like it should take that long. On the positive side, the one-year road test is potentially quite important. While things may break, this should be considered okay. The sooner the road test can start, the better – even if it requires iteration on the design. There is concern that trouble in the early demonstration could be perceived negatively, and it should not be.
- By December 2010, plan to have a unit on a truck.
- The project includes having a lab unit in development now. They are simplifying components and reducing the size and weight of the unit to fit on the truck.
- It is running on low-sulfur diesel.
- The heat exchanger will be revamped in the next phase.
- This is an aggressive development schedule. The demonstration phase will identify additional opportunities for product improvements.
- The milestones are clear. The project is focused appropriately: vibration, weight, packaging, cost, manufacturing, and reliability. The key is the use of commercial diesel APUs as the cost target for competitiveness.
- They employ a vague schedule. There is no real definition of risks and how to address them.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **3.0** for technical accomplishments and progress.

- The total progress appears on track to a soft plan.
- There is a requirement for 3.5 million miles on the system. Shake table testing at ambient conditions is planned.
- They have modified the desulfurizer. They plan to replace the desulfurizer every six-to-twelve months during normal service of the truck.
- Solid oxide technology has presented several challenges that this project will have to successfully resolve in an aggressive development schedule.
- The project team shows excellent progress, and the plan is for limited fleet trials in FY11 and commercial roll-out in late 2012 or early 2013.
- They have significant management support. The shaker table work will yield valuable feedback on the design and is very important.
- There is a good list of accomplishments. The presentation was vague on system test hours. The Q&A indicated appropriate system test accomplishments.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.6** for collaborations.

- They made very good use of input from truck OEMs (original equipment manufacturer).
- They used a truck donated from PACCAR International (PACCAR). They collaborated with TDA Research in Colorado and ElectroCorp, both of whom assisted with report writing and project management.
- PACCAR is a great partner in terms of developing a new technology accessory product for trucks.
- Their effort relies on stack technology developed by the Solid State Energy Conversion Alliance (SECA), and they are working closely with OEMs via PACCAR. They're using vibration data from Peterbilt, and one of their trucks as a test bed. They are also working with well-known TDA to deal with sulfur.
- There is good involvement with this project and good leverage from SECA.

Strengths and weaknesses

Strengths

- There is good focus on an aggressive on-road test program and good use of leverage with the SECA program.
- The technology seems to fill a niche in environmental air quality zones. It is assumed more regulations will be put in place that will further limit the idling of trucks. As these regulations increase, there will be a greater need for this product.

- This is a well thought out program development plan.
- The project as a whole reflects a serious intent towards commercialization with meaningful interactions between OEMs and realistic testing.
- The SOFC technology is very promising, but very challenging. Pushing an SOFC system for small power is a way to resolve issues and show capability.

Weaknesses

- The project plan appears to be a bit soft.
- This transportation application, in particular, is very challenging. An assessment of trade-offs between delivering a reliable system and product specifications is needed (What specifications drive risk up?).

Specific recommendations and additions or deletions to the work scope

- The PI should consider accelerating the deployment to the field test.
- The presentation did not address if users and potential users have been surveyed to see if this is a relevant need, and how they would utilize this technology.
- When units are ready for demonstration, bring Tank and Automotive Research, Development and Engineering Center (TARDEC) into the project so that DoD is involved in the demonstration phase. This may identify complementary products and applications in a military environment.
- The next review should have a better definition of schedule and actions taken against specific barriers.

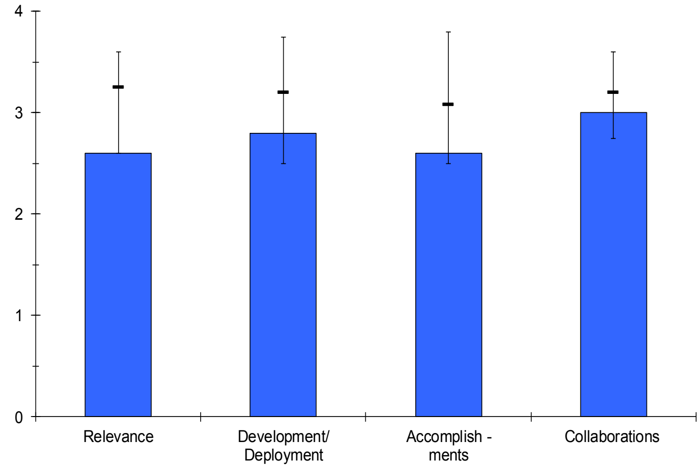
Project # ARRA-03: Highly Efficient, 5kW CHP Fuel Cells Demonstrating Durability and Economic Value in Residential and Light Commercial Applications

Rhonda Staudt; Plug Power, Inc.

Brief Summary of Project

Plug Power believes that high-temperature proton exchange membrane fuel cell (PEMFC) technology creates a compelling value proposition in the residential and light commercial micro-combined heat and power (CHP) market. GenSys Blue is Plug Power's CHP fuel cell system (FCS). The objective of this demonstration program is to substantiate the durability and economic value of GenSys Blue and verify its technology and commercial readiness for the marketplace.

Overall Project Score: 2.7 (5 Reviews Received)



Question 1a: Relevance to overall ARRA objectives

This project earned a score of **2.6** for its relevance to ARRA objectives.

- It is not exactly clear how many jobs are created, sustained, and/or saved. Ten to 15 jobs is pretty general. Next year, the PI should better quantify this in the presentation according to the ARRA definition and what the expected impact is from ordering parts from suppliers for funded systems, contracted labor, etc.
- The long-term impact of this project is not completely clear.
- Creating green jobs is clearly part of the ARRA goals. Residential and/or commercial CHP using fuel cells (FC) clearly fits into that category. Having that said, it is tough to ignore the fact that Plug Power themselves have mothballed this capability to focus their resources more on the lift truck sector. This may not have much, if any, job growth associated with it in the near term.
- There were 10 to 15 workers on this project, plus there was leveraging across the company to retain other jobs.
- The project does not create many new jobs, nor does it seem likely to spur economic investment and growth.
- The project is all development, with very little deployment of only six units. This has limited jobs creation except a few R&D jobs.
- The presentation didn't address the ARRA objectives. In the question-and-answer period, the PI responded that between 10 and 15 jobs were created.

Question 1b: Relevance to overall FCT ARRA objectives

- It was clear how funding supported deployment of FCs, but it was not clear how ARRA funding accelerated commercialization, FC manufacturing, and lessons learned by organizations involved with installation, maintenance, and support services.
- This is an application with a lot of potential and could create stationary FC jobs. The fact that Bloom Energy is also in this market space and has received a lot of venture capital funding attests to the relevance of commercial and residential CHP. The ability to "harden the design" of this high temperature PEM technology throughout the course of this project and to have the University of California, Irvine (UCI), do a system model for product development are important for any FC product launch. However, it will not create a lot of jobs in the near term.
- There is a wide-open market for stationary residential FCs that not many companies are focusing on. Being cost competitive with large segments of the country will be difficult, but certain segments may be attainable.
- The project's cost goal is $10,000 per kilowatt. Even if it succeeds in achieving its goals, its FCs will not be marketable at that price, even with subsidies and tax credits.
- This is a good application for the technology, assuming the market segmentation analysis is correct.

Question 2: Development and Deployment Approach

This project was rated **2.8** based on development and deployment approach.

- It's good to work the California market, because they have good incentives for this technology. It was also a good choice to team with the UCI National Fuel Cell Research Center (NFCRC), since they can give Plug Power valuable input for the local California market. Sempra Energy is also a good partner to work with.
- June 2010 is a go/no milestone for deciding on building the units for the field trial. The systems will be built and installed starting in September 2010. The units for the field test will be control-optimized versions of the units they are currently testing in their lab. The lab units have increased efficiency and reliability and have had material cost reduction since the program started.
- This is more of a demonstration than a deployment.
- The modeling effort does not seem to contribute much to this project.
- The stated objective is "to substantiate the durability and economic value of GenSys Blue and verify its technology and commercial readiness for the marketplace", and yet no business case is presented.
- This project has a good layout of schedule and metrics.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **2.6** for technical accomplishments and progress.

- The impact of jobs created was not quantified.
- There was some inconsistency in the presentation on how the cost per unit is reduced, even though there is a reduction in material cost, build time, etc.
- Plug Power's decision to back away from GenSys Blue's product support at this time can't help the technical progress, because they are not currently committed to this product line. They also had some issues finding locations on the UCI campus to host the CHP systems. There were other issues associated with converging the UCI dynamic performance model into the Plug Power system architecture.
- There was large improvement in unit build times to approximately two workers per week to build out a unit.
- The project shows a reduction in material costs of approximately $40,000 per unit.
- The units have had 8,000 hours run in the lab with the newest version, and about 1,000 hours of lab time with no degradation of the cell stack.
- The lab experienced 76% unadjusted CHP reliability, which is low for a lab unit. It can be assumed that this reliability will decrease in the first field units because it will be outside of the laboratory. Since this is early on in the project, the low reliability is acceptable, but should be monitored throughout the project.
- The technical accomplishments are good, but progress seems a little slow, and the future of the company is on shaky ground. The go/no-go decision should be examined closely; if the company can produce the number of units needed and can fulfill the requirements of the project.
- There is no clear answer on the status of life testing.
- The project provides a clear statement of metrics and test results including a long list of technical improvements.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.0** for collaborations.

- They have strong partners in UCI and Sempra.
- They presented a paper at the FC seminar on this project and highlighted high-temperature PEM technology for residential and/or commercial CHP. They held a ribbon cutting for the deployment at Union College Schenectady, New York. Participation on the Ballston Spa High School field trip also shows good collaboration. Their teaming with Sempra and the UCI NFCRC provides further information dissemination. South Coast Air Quality Management District (AQMD) and NREL are also participating to some degree in this effort, which is a good sign.

- There is coordination among several significant organizations. UCI, Sempra Energy, LPA, AQMD, and NREL. This seems to be a good cross section of partners, as they could each play a significant part in development and market acceptance of this product. Partnering with Sempra and AQMD could improve the ability to install these units in California. The appropriate partners seem to be involved in this project.
- The project would be better served by replacing the university collaborator doing modeling with an industrial organization experienced in controls development and system integration with an eye towards cost reduction.
- There is good, broad involvement.

Strengths and weaknesses

Strengths
- The units appear to be competitive based on efficiency, reliability, and durability potential.
- A new, potential widespread opportunity for stationary FCs is the centerpiece of this effort. Plug Power is teamed with two California-based entities (UCI and Sempra) that can help them work into this market. Also, having AQMD involved is another positive.
- There is good coordination among the partners.
- The technical progress looks good based on what was revealed. They need to get a true customer-based test going to report that the product design and engineering are good and something that DOE should be funding.

Weaknesses
- It is not clear how the cost compares with existing solutions (i.e. per kWh of heat or of electricity).
- It’s uncertain how reliability of this system compares with existing technologies.
- It's a new product in the market.
- Plug Power shows a lack of commitment to the GenSys Blue product line. This is not going to be a job-creator anytime soon.
- Progress seems a little slow, and the go/no-go decision will be extremely important for the project.
- This project appears to be a bridge to nowhere. Its cost goal is $10,000 per kilowatt. Even if it achieves its goals, it will not be marketable at that price, even with subsidies and tax credits.
- The value of the modeling is not certain. Perhaps next year this can be presented in the form of how it will help product engineering. The cost analysis and market price target are critical but were only addressed on a superficial level. Maybe it can be addressed more fully next year.

Specific recommendations and additions or deletions to the work scope

- Plug Power should compare this technology with existing systems, like what Japan is doing, and competing products such as Bloombox. It would be good to know whether Sempra or PG&E is testing this in California.
- California AQMD doesn't exist – South Coast AQMD does.
- Field testing is ideal, but bench testing may be quicker. The verification of thousands of hours of durability is going to take more than a few years. This could potentially slow down the development process.
- The PI should monitor this project closely to see if the company can still support the product build out and installation of the units. If the company will not be able to support this product line, the project scope may need to be reduced.
- This is a good project. Plug Power should strengthen the market assessment and drive product reliability.

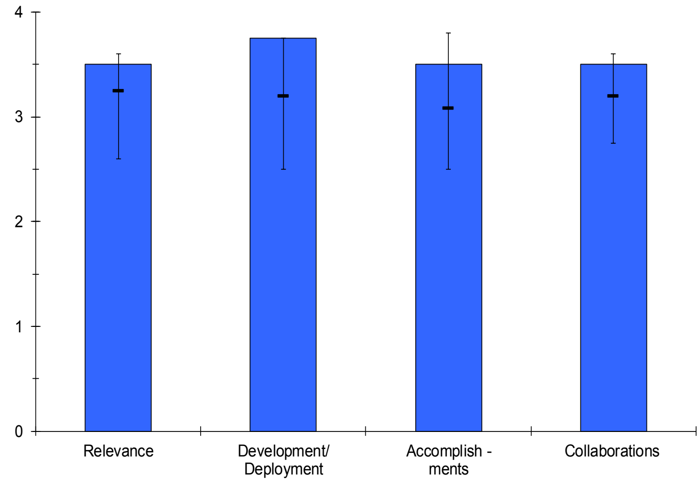
Project # ARRA-06: PEM Fuel Cell Systems Providing Backup Power to Commercial Cellular Towers and an Electric Utility Communications Network

Mike Maxwell; ReliOn, Inc.

Brief Summary of Project

The goal of this project is to install and operate hydrogen fuel cells (FC) as critical emergency reserve power for cell sites operated by AT&T and as back-up power equipment for communications sites in use by California-based Pacific Gas & Electric (PG&E). Up to 189 sites will be served. The goals of the first year were to identify specific sites based on power load and fueling access and then begin deployment. The manufacturing and installation of up to 189 fuel cell systems (FCS) creates and retains direct and indirect jobs at ReliOn and indirect jobs through the service supply chain, and it develops growth in new service industries to install and refuel these systems.

Overall Project Score: 3.6 (4 Reviews Received)



Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.5** for its relevance to ARRA objectives.

- The team provides a good explanation of jobs created according to ARRA definition, but it would be nice to see what other impacts this project has. For example, Air Products developing a more feasible method (four lighter, smaller trucks with composite tanks) for hydrogen delivery to the sites would generate more business for them, as more sites will be accessible.
- It's already a commercially available product.
- This project directly supports ARRA goals including job creation at ReliOn's FC production, for its FC installation, and at its hydrogen fueling.
- Their general statement on jobs is good.

Question 1b: Relevance to overall FCT ARRA objectives

- The impact of bringing down the cost of FCs is not clear.
- It seems apparent that the number of units deployed is increasing because of funding assistance!
- Their support services could be better described.
- This project directly supports FC technology (FCT) ARRA goals with deployments of FCs in substantial amounts and in substantial numbers that make an impact.
- This project has done well in planning its wide spread installations and multiple host user sites.

Question 2: Development and Deployment Approach

This project was rated **3.8** based on development and deployment approach.

- The PI should pay more attention to how the commercial risks are addressed.
- The project is managing risks well in regards to hydrogen delivery.
- This presentation provided a thorough discussion of approach and criteria.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **3.5** for technical accomplishments and progress.

- What does it take to make this product competitive for any site proposed? The PI should consider using a different solution for hydrogen delivery to FC, such as methanol and reforming.
- The project is currently on schedule with some installations completed, and they are likely to finish all installations on time.
- The project's work to modify delivery trucks to reduce delivery costs is good.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.5** for collaborations.

- The team reported good collaboration with telecoms and with hydrogen fuel suppliers.
- There is a geographically broad set of installations that should be excellent exposure of FCs.

Strengths and weaknesses

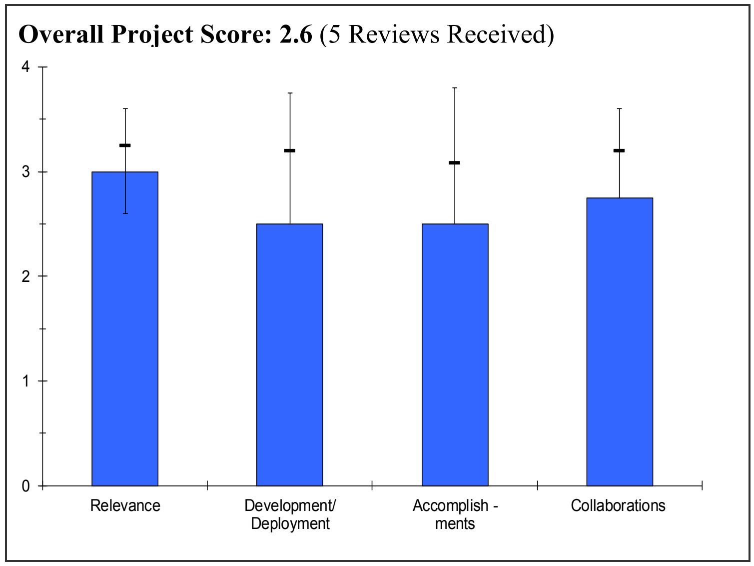
Strengths
- This is a commercially available product.
- The timeliness in getting FCs deployed early is a strength.
- FC size and the number of units are large enough to make a difference towards commercialization.
- There is broad involvement of gas suppliers, customers, and regulators to gain a foothold in the market and to chart a pathway to wider adoption.

Weaknesses
- The product cannot be used for every site.
- It is a new product.
- There was no business plan shown for a sustainable path forward in commercialization after ARRA subsidy ends.
- None.

Specific recommendations and additions or deletions to the work scope

- Compare this technology's cost with existing technology costs, including external costs.
- As soon as data is available, develop a public outreach plan to increase public and market awareness of the benefits and value proposition of this product. This plan can include presentations at industry conferences and web-based materials. In addition to ReliOn and its collaborators, groups with education program experience could disseminate such communication.
- This is a good project. Next year, the team should share lessons learned and an assessment of the economics.

Project # ARRA-07: Accelerating Acceptance of Fuel Cell Backup Power Systems
Rick Cutright; Plug Power, Inc (7A).



Brief Summary of Project

The objectives of this project are to: 1) demonstrate market viability and increase market pull of hydrogen and fuel cell systems (FCS) within our government customers and partners, 2) support 15%-30% of Plug Power's technical staff through this funding effort, 3) establish a large automotive supply base delivering stack, reformer, and balance of plant (BoP) components, 4) deploy 20 GenSys hybrid hydrogen start/liquefied petroleum gas (LPG) or natural gas run units that provide economically viable backup power in excess of 72 hours.

Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.0** for its relevance to ARRA objectives.

- Assuming the 12 to 15 jobs mentioned are calculated using the ARRA formula, this is good. The three to five additional in-company jobs are also good.
- It would be good to note whether the jobs are sustainable or not.
- The presenter explained that jobs have been preserved. The product being developed, should costs be reasonable, appears to have future growth potential.
- This is not a conventional, "shovel-ready" project. There is still a lot of product development to do.
- Most of the jobs so far have been primarily in analysis and project management, yet it was indicated that 15%-30% of its employees were technical staff.

Question 1b: Relevance to overall FCT ARRA objectives

- The key to relevance is if the reliance on fossil fuel for the power generation is really reduced.
- It is too early in the project timeline to give a good evaluation.
- The deployment of significant numbers of units supports DOE's goal of accelerating deployment.
- The deployment of 20 units of good size (six kW) will help advance the market, if field operation is successful.
- This market is a good entry point with well understood, controlled requirements.

Question 2: Development and Deployment Approach

This project was rated **2.5** based on development and deployment approach.

- The PI should find out if increased maintenance costs are caused by new, unproven preventative maintenance or other problems.
- There was little discussion of cost requirements and progress to reduce cost through the early deployment of FCs. The project plan also appears to be a bit soft, meaning not very aggressive.
- It is not clear why operation on hydrogen for 72 hours has been ruled out when many other backup power deployments are successfully using hydrogen fuel.
- The alternative hybrid approach that they have changed to (hydrogen/LPG) seems unnecessarily complicated because of the use of two different fuels, extra controls, etc. There was no analysis presented, technical or economic, to show why they chose this approach instead of simply using a battery during initial startup.

- They needed a better discussion of barriers and risks.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **2.5** for technical accomplishments and progress.

- Everything appears to be on target.
- The specific goals, product lifetime, and cost analysis are relatively vague. The progress is okay but is running against a plan that is not terribly aggressive.
- For a project that started more than a year ago (June 1, 2009), progress has been slow. The site selection still isn't completed for Warner Robins.
- The bulk of the effort to date appears to have been devoted to economic analyses, but no results on economic analyses were presented.
- This project shows good technical accomplishments and progress overall, but reporting accomplishments against target metrics would be very helpful.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **2.8** for collaborations.

- The DoD partners appear to be value added. However, there is no commercial partner, a relationship that should be developed to ensure a marketplace exists. Also, there is a statement of "automotive suppliers" being used for stacks, fuel processors, etc., in the opening section, but little follow-up on that fact.
- There are good collaborations, but they are on the weak side relative to some other ARRA projects.

Strengths and weaknesses

Strengths
- The potential benefits of the product are a strength.
- It seems like a good application. The demonstration should start early enough in the project to be useful.
- It draws on an existing technology base, which may minimize the technical barriers.

Weaknesses
- It is a new project.
- There are limited partnerships with commercial entities. There is also no clarity on cost performance and reduction over time.
- The definition of market and technical barriers needs to be better described, and specific approaches need to be more fully described.

Specific recommendations and additions or deletions to the work scope

- One needs to know the history of this unit, if there were any other units deployed in the past, and, if so, how those units performed.
- The PI should review the weaknesses listed here and work to address those areas.
- The team should re-examine the possibility of operation on hydrogen for 72 hours, since many other backup power deployments are successfully using hydrogen fuel. If that is not possible, examine the technical and economic trade-offs of using a battery for initial start-up versus going to a hybrid hydrogen/LPG system.
- This project needs to strengthen their economic justification and provide more detail on technical approach to specific barriers.

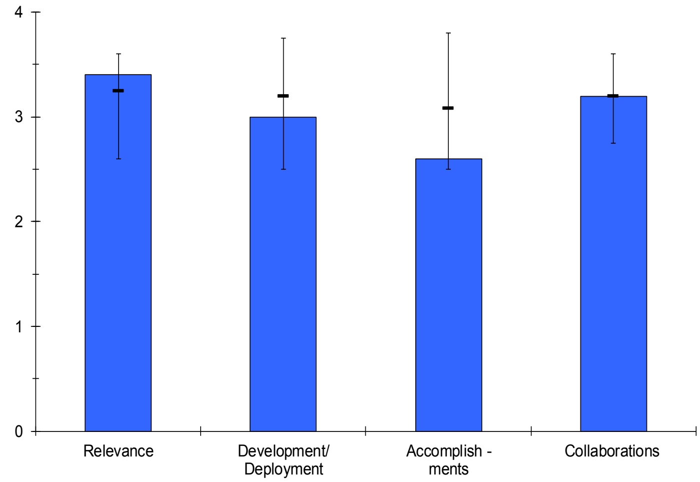
Project # ARRA-08: HEB Grocery Total Power Solution for Fuel Cell Powered Material Handling Equipment- Fuel Cell Hybrid Power Packs and Hydrogen Refueling

William Mitchell; Nuvera Fuel Cells.

Brief Summary of Project

The objectives of this project are to install one PowerTap hydrogen generation system with indoor refueling and 14 PowerEdge fuel cell systems (FCS) at an H-E-B (Here Everything's Better) grocery facility in San Antonio, Texas. Expected outcomes include: 1) periodic reports documenting system performance and any issues, 2) enable widespread adoption of hydrogen and fuel cell (FC) technology by employing this across the H-E-B fleet of 1,000 forklifts once verification of economic and/or operational advantages is determined, and 3) validation of the DOE market transformation activities.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.4** for its relevance to ARRA objectives.

- Making the conversion based on ARRA definition of jobs created and/or sustained is praised.
- The PI should continue to speak about additional economic impacts as well. It's good to hear what economic activity is stimulated outside the ARRA definition.
- Is there an ARRA definition of "economic activity" that has been spurred through this project?
- This will help FC volume for domestic manufacturers, which will lead to further cost reductions and commensurate market share in this new, growing technology. Indirectly, this also benefits the light-duty vehicle FC market since these are proton exchange membrane fuel cell (PEMFC) manufacturers.
- This project indicates that job saving is modest, one to two jobs. However, the prospect of stimulating long-term economic growth is viable.
- This project will definitely create or sustain jobs in one of the emerging commercial applications of FCs.
- Application of this project in a refrigerated warehouse environment will be a good test of FCs in this challenging project.

Question 1b: Relevance to overall FCT ARRA objectives

- It could be challenging to develop a 10-year life cycle within the first two years of this project. It would be interesting to note what they plan on doing for the next eight years. Hopefully, they are not completely dependent on what H-E-B decides!
- This is the hottest early market for FCs, but it still has a long way to go, both because of unfamiliarity and hydrogen infrastructure costs. Working with a grocery chain is a wise choice since they have a great chance for further adoption across the company and, ultimately, across the sector. They cite a 10% productivity gain.
- The proposed effort is consistent with the acceleration of these efforts. The question is whether the plan makes a significant impact. It would be useful to see more information on life cycle and durability, and how these results will be used to reduce customer risk of acquiring these devices.
- If H-E-B has a good experience with FC forklifts as a result of this project, the project team expects the grocery chain to employ to up to 1,000 forklifts across its fleet.

Question 2: Development and Deployment Approach

This project was rated **3.0** based on development and deployment approach.

- The PI should make sure to consider how larger power demand (more than 5kW capacity) on FC forklifts – because of larger demand in a particular section of the warehouse – will be addressed in a next project or contract. Moving all forklifts to another section is not always an option, especially when a larger fleet is ordered.
- The PI should consider reviewing how they evaluate the power needs for the forklifts. Moving the forklifts to another part of the warehouse could have been avoided.
- Installing 25 units is a large enough number from which to make an informed judgment on this technology from H-E-B's standpoint. H-E-B estimates they have 1,000 lift trucks, so there is room to grow. Nuvera's POWERTAP natural gas-to-hydrogen reformer allows for a turnkey solution. Two dispensers show the ability (compared to batteries) to reduce refueling trips and distances for the lift trucks. Two 10-hour shifts of employees provide enough operational tempo to make hydrogen FC lift trucks competitive. Texas is a good target market because they have low natural gas prices.
- The latter portion of the project plan could use a bit more detail to show data analysis cycles and lessons learned.
- The initial results of 10% total productivity gain versus batteries are impressive.
- Validating these findings with additional data will provide increased confidence in the added value of FC forklifts compared to batteries.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **2.6** for technical accomplishments and progress.

- Showing a numerical comparison with battery forklifts would be helpful in evaluating the significance of improvements and setting benchmarks.
- Input from H-E-B would help determine the project's progress, especially comments on what decision variables they will use to make the decision to keep the FC forklifts in continued operation.
- There is pretty good progress, although somehow the hydrogen FC lift trucks were unable to work the produce warehouse, and they had to move them to the grocery section. It wasn't clear what the specific issue was. The assumption is the FC power pack was undersized for the heaviest and highest of lifts (the presenter was a "pinch hitter" and quite unqualified to give the presentation).
- The presentation was not clear as to what has actually been accomplished to date from a technical point of view.
- There was no actual information about jobs created or sustained provided.
- The PI said that an example of measuring 10% of productivity could be described as starting with ten people, firing one person, and still getting the job done. This is definitely not the best way to argue that this project is creating jobs.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.2** for collaborations.

- It doesn't appear there are additional university or research organizations involved.
- There was little discussion about collaborations beyond H-E-B and providing NREL data. On the other hand, it would be challenging to suggest a list of collaborators in this area.
- They produced a good mix of partners to address the project tasks.
- H-E-B is a highly regarded grocer. Its progress with FCs will be keenly observed by the grocery industry. H-E-B may also have potential to be a customer of other FC products such as primary power. It is important to be developing a presence for FC products in Texas.
- The PI showed good collaboration with NREL in sharing (and then publishing) the data to be aggregated with other FC forklift projects.
- H-E-B appears to be an excellent project partner to grow the market share of FC forklifts, if this project is successful.

Strengths and weaknesses

Strengths
- It is valuable to have the H-E-B grocery stores involved.
- The potential to inform many more people (i.e., truckers and peripheral stakeholders) about FC forklift and related station projects is very beneficial.
- The involvement of a company that normally maintains battery- powered forklifts is extremely beneficial.
- This project helps a new technology move forward in a market well suited for it. Site selection in Texas where natural gas costs are low, client selection that utilizes a distribution warehouse with multiple shifts, and a turnkey approach, such as one-stop shopping for FC and hydrogen training and/or issues, increases the odds for success.
- The project is in a good market, and they are collaborating with good partners.
- The real world demonstration of the benefits of FC forklifts in a very demanding, critical-path application of moving groceries is useful.

Weaknesses
- The size of FC forklift fleet, presented as a percentage of total forklift fleet, which is 1,000, is a weakness.
- Another weakness is the limited project life. It is somewhat challenging to hear what the potential financial losses are if H-E-B decides not to continue this project.
- The presenter was not specific enough on details, and questions asked were generally not answered.
- There was a lack of focus on durability in a detailed way.
- There was no clear tabulation of jobs impact, which is key for ARRA activities.

Specific recommendations and additions or deletions to the work scope

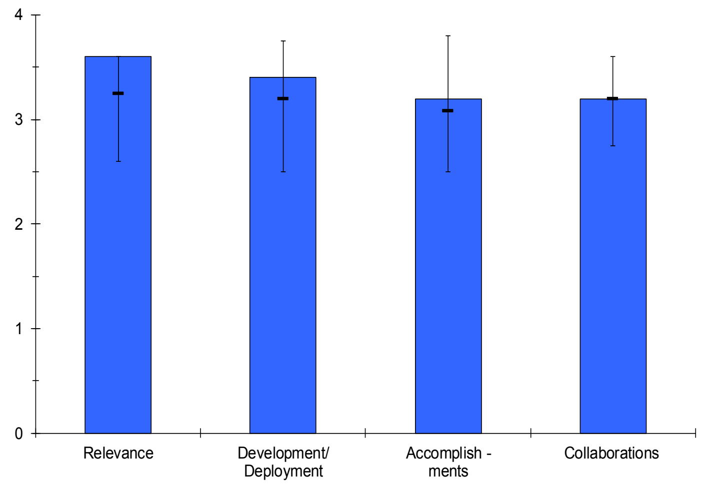
- Nuvera needs to make sure that cost of fuel projections are made for larger fleets of FC forklifts, in addition to the 10-year life cycle plan.
- A 10% productivity gain doesn't show how this translates in H-E-B’s overall warehouse benefits, jobs and productivity gains.
- The PI should specify how the fleet data will be analyzed. It is implied that NREL would do that.
- Nuvera should seek out opportunities for public outreach. For example, the Fuel Cell Seminar will be held this year in San Antonio, Texas, where H-E-B is headquartered. This would be a good opportunity for the grocery store chain to discuss publicly its interest in FCs.
- Questions about the details of the project could be more effectively answered if Nuvera had sent a technical presenter rather than a financial presenter. Many basic questions went unanswered.

Project # ARRA-09: 7B: Fuel Cell-Powered Lift Truck FedEx Freight Fleet Deployment
Curtis Cummings; FedEx Freight East

Brief Summary of Project

The objectives of this project are to: 1) convert an entire material handling equipment (MHE) fleet at the Federal Express (FedEx) facility in Springfield, Missouri, with fuel cell (FC)-powered forklifts, 2) demonstrate safe and reliable operations of hydrogen MHE, 3) demonstrate the economic benefits of conversion, 4) provide cost effective and reliable hydrogen, 5) spur further lift truck fleet conversions, and 6) establish proving ground for hydrogen MHE.

Overall Project Score: 3.3 (5 Reviews Received)



Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.6** for its relevance to ARRA objectives.

- If FedEx adopts FC-powered forklifts, even to a moderate degree, this could be a “shot in the arm” for the fledgling FC companies in this market space.
- The project could spur economic activity by creating a market for FC forklifts. The project will retrofit 35 FCs. Having a major third party evaluating this technology could open up a large market for FCs.
- They didn't address the relevance to overall ARRA objectives.

Question 1b: Relevance to overall FCT ARRA objectives

- FedEx is serious about exploring this technology. They clearly aren't doing it for the ARRA cost share. The fact that they want to convert their entire fleet of Class I forklifts in Springfield, Missouri, and are targeting a ”greenfield” site speaks volumes.
- This project could be the first step in a large deployment of this technology. If the testing is successful in this project, a large market segment could be opened up for FC technology.
- This is a good application to use this technology.

Question 2: Development and Deployment Approach

This project was rated **3.4** based on development and deployment approach.

- Working with Plug Power, arguably the leader in the FC lift truck market, and Air Products is a solid team. Thirty-five Class I lift trucks is sufficient volume from which to make an evaluation for future use within FedEx. Two indoor dispensers will keep the lift trucks from having to travel far for refueling purposes. The Plug Power units should last the entire shift, as they have at the New Cumberland Defense Depot Susquehanna. The 10-12 kW range is a good power range. FedEx will use this deployment to compare hydrogen fuel cell (HFC) lift trucks versus electric- and propane-powered lift trucks.
- There was a delay because of getting internal approval to lease the technology, because the status quo previously had been to purchase equipment outright. This delay is acceptable, because business procedures had to be changed to accept the new technology.
- It would be beneficial to know if there are any plans to release an economic analysis. It wasn’t identified as a milestone or deliverable.
- They had a vague schedule and overall plan. The emphasis on training operators is commended.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **3.2** for technical accomplishments and progress.

- There is good progress being made, despite a few bumps in the road. National Fire Protection Association standards versus international standards that the Springfield Fire Department uses caused adaptive process control (APC) to have to put in different dispensers than originally envisioned. FedEx had to put in a temporary battery charging infrastructure and use battery forklifts until hydrogen infrastructure was ready. As a result, HFC lift trucks were not in service at the time of the AMR. There does not appear to be any issues with Plug Power's GENDRIVE units being ready on time. They just need hydrogen in place to operate.
- They are making good progress toward their milestones. Units will be installed shortly and operation will begin.
- Much appears to have been accomplished in the last nine months. It would help if there were a schedule to judge against.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.2** for collaborations.

- There were only 10 slides in the briefing, and they didn't cover this area very well. They are sending data to NREL. It's difficult for a for-profit company putting up a lot of their own cost share to spread the word in this sector since that would be telling their competitors.
- There are three partners: Fedex, Plug Power, and Air Products. These are three effective partners that are likely to succeed in this type of project.
- Their stated cost of delivered hydrogen ($5/kg) seems unrealistic, especially considering that it is trucked in from either New Orleans or Sarnia, Ohio, to Springfield, Missouri.
- This was a good, but predictable, cast of characters.

Strengths and weaknesses

Strengths
- This project targets a major company with great potential for demand. A strong team was put together for this effort. Greenfield site selection and high operational tempo is an ideal deployment scenario. A company with a reputation as strong as FedEx would really boost the credibility of the HFC lift-trucks if they were to embrace them.
- A third-party evaluation of FC technology could jump start the FC market, if this test is successful.
- This is a real industrial test bed, and the project is making good technical progress.

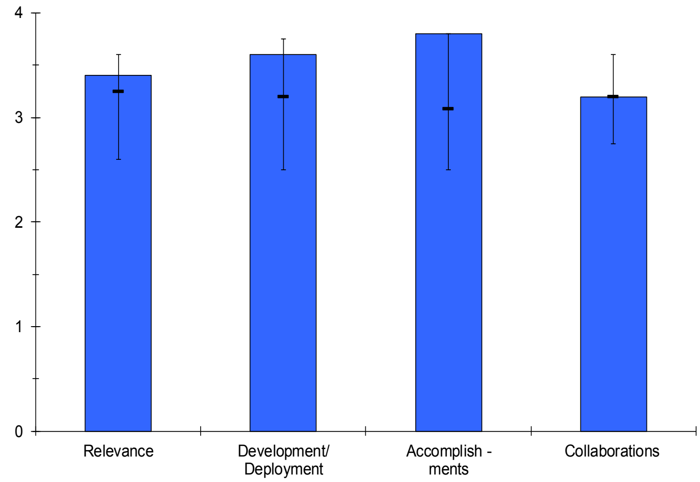
Weaknesses
- None.
- None to mention.
- This is a good project. It will be interesting to hear an unbiased assessment of these systems in operation from the user and customer perspectives.

Specific recommendations and additions or deletions to the work scope

- See if FedEx will share their comparison between FC forklifts with electric and propane. In particular, there is not a lot of comparison that has been done between HFC lift trucks and propane-powered lift trucks. The briefer said propane tends to have the most down time.
- It is recommended to continue the project as planned. It seems to be on track.
- It would be useful to show some economic analysis, with and without ARRA grant subsidies. Examine cases on both expected FC life and for the warranty life of the FC. Validate (or re-evaluate) the David Greene model that predicts that a market deployment of 1,000 units will drive FC costs from $3,600 per kilowatt to less than $2,000 per kilowatt.

Project # ARRA-10: Fuel Cell-Powered Lift Truck Sysco Houston Fleet Deployment
Scott Kleiver; Sysco of Houston

Overall Project Score: 3.6 (5 Reviews Received)



Brief Summary of Project

The objectives of this project are to: 1) support American Recovery and Reinvestment Act goals of long-term economic growth by successfully demonstrating a new technology, 2) establish a proving ground for expanded use of hydrogen fueling technology at Sysco and promoting future adoption of fuel cells (FC) in other applications to help drive their use in the U.S., and 3) promote the economic and environmental benefits of hydrogen fuel cell (HFC) technology. The tactics used are to: 1) convert the entire Class II and Class III lift truck fleet at Sysco Houston's greenfield distribution center to FC use, 2) demonstrate the economic benefits of large fleet conversions of lift trucks from lead-acid batteries to FC power units by measuring, analyzing, and reporting on the performance, operability, and safety of the systems, 3) demonstrate freezer operation, and 4) obtain affordable and reliable hydrogen.

Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.4** for its relevance to ARRA objectives.

- This project indicates good timing for the implementation of a hydrogen station and FC forklifts that are lined up. This is good to know for all potential future distribution centers beyond Sysco's Greenfield site. Lessons learned to be shared.
- There is a significant number of new jobs created compared to several of the other projects. This could also be because of new implementation instead of the replacement of a technology.
- They show good quantification of jobs impact and the financial benefit of the forklifts in the operation.
- The PI reported that 5.5 jobs are created. The cost savings have already been realized by the host site. This savings may affect other aspects of the business and keep additional jobs viable.
- The project stated that throughout the life of the project, 5.5 jobs have been created.

Question 1b: Relevance to overall FCT ARRA objectives

- Commercialization and deployment were shown, but the impact on the company that provided the fuel cells was not. It would also help if we knew what they learned from this project.
- The company appears to be genuine in looking to this application for more widespread deployment. The project could facilitate further expansion of FC use.
- This project can accelerate the commercialization and deployment of FCs and could open a large market for FC fork trucks. It also could affect the hydrogen fueling industry.
- Because of the size and reach of Sysco, a successful demonstration through this project could lead to large-scale adoption of FC forklifts at many sites.

Question 2: Development and Deployment Approach

This project was rated **3.6** based on development and deployment approach.

- The freezing capability was well addressed.
- Avoiding drive offs was well addressed by Plug Power. This should be required of all FC forklifts, because it saves significantly on cost and creates a safer work environment. A truck being driven away could require hose replacement that creates labor hours, costing up to several thousands of dollars to repair and/or replace it.
- They provided a good definition of key milestones, most of which are completed. The data collection intervals are crucial, and they are defined.
- The project is on track with FC forklifts operating at the site. The cost savings and productivity increases have been realized.
- This is a very large scale deployment, so it is great to see the contractor so well organized.
- The contractor appears to be ideally suited for a commercial evaluation of whether FC forklifts will assist their bottom line.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **3.8** for technical accomplishments and progress.

- The technical accomplishments and progress have both been excellent.
- The project is on track, and the plan is fairly aggressive.
- The units are in place and operating at a high level. Productivity has been increased.
- This project has moved very rapidly to deployment, with more than 2,500 hydrogen fills and a conversion of an entire fleet of Class II and Class III lifts to run on hydrogen at the Greenfield site.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.2** for collaborations.

- The involvement of a research institution (NREL) will hopefully result in the development of better models that can be used by the rest of the industry (i.e., the H2A Model or hydrogen delivery).
- There is no involvement of an academic institution.
- The project has good interactions with a FC manufacturer, a hydrogen supplier, and the FC customer interactions.
- Sysco, Plug Power, Air Products, and Big-D Construction are the appropriate players to participate in this project.
- It appears as though a good partnership has been forged with Air Products, who could easily support H-E-B, a Texas-based grocery store chain, as they move beyond the demonstration phase into a more full-blown commercial purchase and/or deployment without government incentives.

Strengths and weaknesses

Strengths

- Implementation of this project from a start facility is promising.
- They have a good number of FC forklifts.
- There is a good market for this application, an aggressive schedule, and a strong focus on life data.
- The schedule is on track and units are in the field. They are operating well.
- The project is being rapidly executed with many activities, such as fire marshal approval, ahead of schedule.

Weaknesses

- The distance to transport liquid hydrogen is a weakness.
- There are no warranties of FCs for the user.
- There is no indication that hydrogen cost per kilogram will come down. The hydrogen supplier appears to make profit on the sale of hydrogen. It would be interesting to know if that was the case.
- There are no obvious weaknesses here.
- No major project weaknesses were identified.

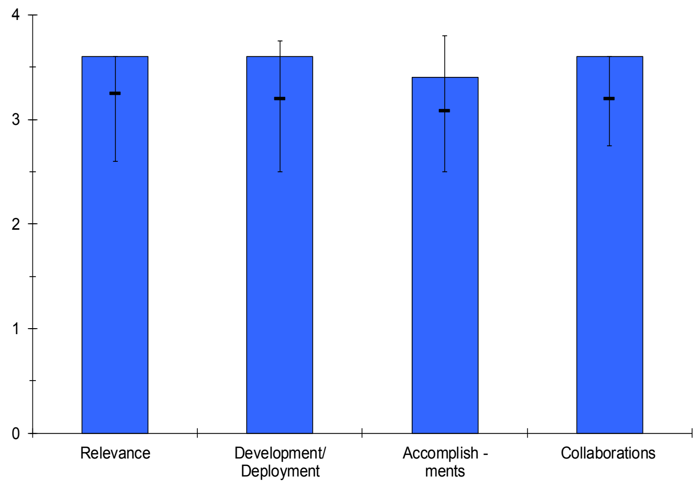
Specific recommendations and additions or deletions to the work scope

- Sysco's liability when something happens with the hydrogen station needs to be addressed.
- It is recommended that the team evaluate and/or consider what alternatives they have that would bring down the cost of hydrogen. If Sysco considers putting in similar fleets at other locations, the cost could come down significantly if the same station design is reproduced at each site.
- The PI should ensure that the data analysis is very robust. There will likely be some failures, and they should be reported in some detail in the future.
- It's good to continue operating the FC fork trucks and monitoring the progress.
- They could coordinate this project with the overall public outreach effort as part of the Education Subprogram. For example, invite Sysco to participate in a panel on market transformation at the 2010 Fuel Cell Seminar in nearby San Antonio, Texas.
- The PI should make sure that the project clearly identifies the value proposition for future deployments, based on the evaluation of costs and data from this project without future incentives being required.
- They are ready to publicly broadcast the success of this project at trade conferences so the word gets out beyond the community already aware of this technology.

Project # ARRA-11: 7B: GENCO Fuel Cell-Powered Lift Truck Fleet Deployment
Jim Klingler; GENCO.

Brief Summary of Project

Overall Project Score: 3.5 (5 Reviews Received)



The objectives of this project are to: 1) support American Recovery and Reinvestment Act goals of long-term economic growth by successfully demonstrating a new technology, and 2) promote the economic and environmental benefits of hydrogen fuel cell (HFC) technology. The tactics used are to: 1) demonstrate the economic benefits of large fleet conversions of lift trucks from batteries to fuel cell (FC)-powered units by measuring, analyzing, and reporting on the performance, operability, and safety of the systems that will spur further FC lift truck fleet conversions, 2) convert electric-drive fork lift truck fleets to FC use in five large distribution centers and manufacturing facilities, 3) provide affordable and reliable hydrogen, and 4) establish a proving ground for hydrogen fueling technology that will promote the future adoption of FCs in other applications, such as cars, and help drive the use of FC technology in the U.S.

Question 1a: Relevance to overall ARRA objectives

This project earned a score of **3.6** for its relevance to ARRA objectives.

- GENCO is a third-party distribution warehouse manager that can bring this technology to many major retailers who employ their services. These include Target, Best Buy, Johnson & Johnson, and Kimberly-Clark. The deployment itself will cover multiple original equipment manufacturers (OEMs): Wegmans Food Markets, Whole Foods Market Stores, Coca-Cola, and Sysco Food Services of Philadelphia. The productivity increases through use of these FC lift trucks should increase jobs at both the FC OEM level and the company that hires GENCO to operate the warehouses. Productivity precipitates lower costs, increasing profitability that should allow for further expansion and more jobs.
- This project could spur economic activity by investing in this new technology, and cost savings could be realized by these companies that, in turn, could save jobs and increase long-term economic growth.
- This project has the potential for many jobs to be created and/or saved given the large scale deployment of 357 lifts.
- Specifically, two jobs were maintained in the first quarter of 2010.

Question 1b: Relevance to overall FCT ARRA objectives

- GENCO is asking the question: "Can we run our customers' facilities more efficiently?" By exploring the use of FCs versus batteries in their lift trucks, GENCO is trying to become even more competitive. This underscores the relevance of this technology to this market.
- This deployment project gets this technology in the hands of the users, which could create a cascading effect for implementation of these FC fork lifts across the country. Projects like this are needed for commercialization of this technology.
- The number of FC units in this project is a large enough number to make a significant impact. With 357 lifts, this project has gravitas.
- The broad introduction to multiple users is commended. Trained operators and controlled conditions will help to achieve technical success.

- The large-scale potential of a nationwide rollout among GENCO partners is huge, which makes this project extremely relevant toward accelerating commercialization.

Question 2: Development and Deployment Approach

This project was rated **3.6** based on development and deployment approach.

- By having multiple warehouse and company deployments, GENCO is maximizing the impact that a successful pilot would have. As a third-party warehouse operator, GENCO can claim credit for a clean energy deployment and pass that credit on to their customers. Using two hydrogen suppliers – Air Products and Linde for the Whole Foods deployment – spreads out the risks and adds competition for hydrogen supply and infrastructure . Other great features of this project include the fact that these are multiple shift operations, and they are employing Class I, II and III lift trucks in the pilots.
- The deployment plan is well laid out, and good progress has been made toward the milestones. They have deployed 59 units and have logged significant run hours.
- It would be helpful to know if there is any plan to show and/or publish their economic analysis. It was not identified as a milestone or deliverable in the presentation.
- They offered a good definition of their approach. The reviewer recommends providing a better definition of what go/no-go means.
- Having two hydrogen suppliers, Linde and Air Products, is great because it will actually start to trigger some competition, resulting in lower hydrogen cost and increased choices in the marketplace.
- The location, type, and quantity of lifts to be deployed were clearly articulated.

Question 3: Technical accomplishments and progress toward project and ARRA objectives and milestones

This project was rated **3.4** for technical accomplishments and progress.

- Everything seems to be going pretty much on schedule. They are using the hydrogen station in Aiken, South Carolina, which lowers hydrogen costs at that site. They did have delays getting on contract and receiving all the environmental permits.
- Some of the units are in the field, and the remainder of the units are on schedule to be placed at the locations.
- Their technical progress to date has been good.
- There are 59 units already operating successfully.
- To date, there has been more than 2,000 hydrogen refuelings.
- The progress appears to be outstanding.
- They already have 59 GenDrive systems delivered – two months ahead of schedule – and have logged 24,000 operating hours.
- Good progress appears to be made at the other four sites on schedule.

Question 4: Collaborations with industry, universities and other laboratories

This project was rated **3.6** for collaborations.

- GENCO is doing a good job relaying information about this pilot to their customers. Their customers say they "want to be leaders, but not pioneers". This means a cautious, deliberative approach is in order, but it also means they want GENCO to keep them on the forefront of more productive and cleaner technologies. GENCO mentioned there have been no "Hindenburg references," as they have done their operator training and familiarization education with permitting officials. This suggests that they are collaborating early enough with these employees and officials to enable the project to go forward in a timely and effective fashion.
- The project has multiple partners including Plug Power, Linde, Air Products, and customers at each host site: Wegmans, Whole Foods, Coca-Cola, Sysco, and Kimberly-Clarke. These units are being evaluated by large and diverse companies that could create a significant market pull if this project is a success.
- Their collaboration includes two suppliers of hydrogen.
- Their stated cost of delivered hydrogen at $11 per kilogram seems realistic.
- The large number of users should really help accelerate and evaluate FCs in this marketplace.

- This project has assembled an excellent team with a cross-section of partners representing many different sectors of the U.S. retail economy.
- Important lessons will be learned from the execution of this project.

Strengths and weaknesses

Strengths
- Through strong collaborations, GENCO has a great target market to which they can deploy this technology.
- Many of GENCO's customers will see the benefits of this technology and take credit for having it used in their facilities.
- There are multiple hydrogen suppliers.
- The units have been deployed, and significant run hours have been logged. Multiple partners and hosts are involved in this deployment. The technology is performing as expected.
- The fact that they are going to deploy 357 units is impressive.
- This project provides for a relatively large quantity of delivered product, something that can help bring down production costs. The introduction of the product to multiple users and customers is commended.
- This project enables penetration of FC forklifts into a good cross-section of the U.S. economy, reflecting the intent and potential of the ARRA activity.

Weaknesses
- None.
- Set at one year, the warranty on the FC units is very limited. From the users' viewpoint, this could be a problem if the FCs start failing early.
- No weaknesses identified here.

Specific recommendations and additions or deletions to the work scope

- GENCO should continue monitoring progress until all units are sited and operating.
- The team can show some economic analysis, with and without ARRA grant subsidies, and examine cases on both expected FC life and warranty on FC life.
- The project should validate, or re-evaluate, the David Greene model that predicts a market deployment of 1,000 units that will drive FC costs from $3,600/kW to less than $2,000/kW.
- This is a good project. The PI should provide an unbiased account of lessons learned and feedback from the operators and customers.
- Keep completing milestones on time so that all of the project partners have a good experience with hydrogen and FCs.

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Energetics Inc.

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Abe, Yasuhiro
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Ahluwalia, Rajesh
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Ahmed, Sayeed
Infineon

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APPENDIX A: ATTENDEE LIST

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Bailey, Brent
CRC

Bailey, Carol
Deloitte Consulting

Bailey, Mark
Industry representative

Bailey, Owen
Umicore

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Balema, Viktor
Sigma_Aldrich Corp.

Ball, Sarah
Johnson Matthey Fuel Cells

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Ban, Chunmei
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Vencon

Barbier, Francoise
Air Liquide

Barclay, John
Heracles/Prometheus Energy

Barden, Dakota
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Barnes, James
DOE

Barnett, Brian
TIAX LLC

Barnett, Robb
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Bassett, Alexandria
New West Technologies

Bastnagel, Raymond
Solvay Solexis

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Belt, Jeffrey
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Taylor, Ralph
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Taylor, Robin
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Tejima, Go
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Tessier, Pascal
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Trunek, Andrew
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SUB-PROGRAM COMMENTS PROVIDED BY REVIEWERS

Education Sub-program Comments

1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (include information presented in the Plenary presentation of the sub-program if appropriate)

- Yes.

- The presentation provided the right level of detail for the sub-program. The education based challenges were clearly outlined. Progress was clearly outlined. The number of trained people was identified by category.

- The sub-program was covered well. The presentation did not always clearly indicate the progress from previous year since the totals were cumulative.

- Challenges and opportunities were clearly presented; as was the progress made over the last year (progress was clearly outlined by activity).

- I believe the sub-program was adequately covered. In terms of progress in comparison to the previous year, I believe this could have been better presented and discussed.

- The sub-program was covered adequately, and important issues were identified.

- Yes.

- DOE’s presentation showed that the Education program is covering several important areas from government to students to end users and even the general public. It's good to see such diversity. The challenges were clearly laid out and I appreciated the clarification that people who are negative towards hydrogen and fuel cells are usually negative because they don't have adequate information. Most people that interact with the technology have a positive outlook.

- An improvement could be to show how perceptions are being changed due to the various programs - data, quotes etc. that show that perceptions and acceptance of the technology is high after personal interactions with an education program.

- The sub-program was well covered in a congenial manner with good direction to the reviewers giving excellent background information on this important topic.

2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Yes - no gaps were apparent.

- Educating safety officials and codes and standards officials is a priority area. Efforts to develop meaningful, relevant, and innovative curricula for middle school and high school were clearly explained. State and local government outreach is an excellent area to be developing.

- There are no gaps in the project portfolio. It is hard to judge if anyone has been overlooked as classic categories for public education are being used.

- The challenges are being addressed via the training curriculum.

- There don't appear to be gaps in the portfolio, at least in relation to the available funding. More details on the future plans to address issues and challenges would be appreciated (presentation had a lot of detail on past accomplishments, but less detail on future plans).

- Now that commercial deployments are growing in a few early markets, I believe there is an opportunity for a targeted public information and outreach effort to enhance the progress being made in these early markets. The data collected and reported as part of the Recovery Act projects and the Market Transformation projects should contribute greatly to a body of information that could boost near-term sales. Examples would include validating the business case for fuel cell powering for MHE (material handling equipment), primary power for grocery stores, and backup power.

- Funding issues will continue to hamper the effectiveness of planned efforts.

- Yes.

- The current programs seem to be addressing the challenges, so to that extent, yes, it looks like the program is addressing them. But what would the sub-program like to do more of? It would be good to hear what new things the Education sub-program wants to do. Does it include greater volume of the same projects? New projects? The sub-program is budget restrained, so it is difficult do new things.

- The biggest challenge that this area faces is lack of budget. Finding funding for this important sub-program must be difficult, given the fact that there is no funding request for education efforts. Some education is paid for out of Codes & Standards funding, but the need is greater than that, and taking funds from the allied sub-program does neither sub-program justice.

3. Does the sub-program area appear to be focused, well-managed, and effective in addressing the DOE Hydrogen Program R&D needs?

- This appears to be a well-managed and focused sub-program meeting its goals and objectives.

- The sub-program appears to be focused, well-managed, and effective. The various audiences defined cover most everyone who has a need to know. It is difficult to know if the sub-program is well managed from this presentation. The sub-program is well organized.

- Yes. There are a multitude of on-going efforts that appear to be focused on the challenges and address them.

- The educational efforts do appear to be effective in addressing the Program's needs for outreach and education: they have reached a significant and wide audience with hydrogen information. I don't see any management issues at this point.

- Yes, I believe the sub-program is well positioned to support the needs of the DOE Hydrogen Program.

- Low or no funding makes it difficult or impossible to address DOE needs.

- Yes.

- The sub-program is addressing R&D needs. The DOE TDM is new and has hit the ground running, but I don't have enough history to determine how well-managed the sub-program is. So far, so good!

- The sub-program is done in a first-class manner.

4. Other Comments:

- We were not asked if we had any questions.

- Develop a method to understand if the overall perception of H_2 is changing as a result of these and industry efforts. Is H_2 becoming more acceptable in the minds of the public?

- Budget levels could impact the overall success of the program.

- I think the Education sub-program and maybe Vehicles and even EERE could be doing more outreach. Hydrogen is losing ground to other alternatives and it would be good to see hydrogen better included in the outreach of companion technologies like battery vehicles and other advanced technologies as part of a broad portfolio—much like the automakers are doing. These technologies are more complementary than competitive.

If the outreach from DOE was more visible and showed a portfolio approach, maybe it would dispel the issue in public opinion that the Administration believes more in battery vehicles than hydrogen vehicles. If they really are both a part of the important electric vehicle portfolio, maybe outreach projects could convey that. Education about these technologies and the way they support each other is key to consumer acceptance and also key to undoing the divide that's been created by the perception that one technology has been chosen over another. That perception needs to be fixed because it is slowing progress in both technology areas and that means it's slowing the progress that DOE could be making. I'd think that some cross-cutting programs like that would not only aid education, but help to increase the effectiveness of projects in several different DOE programs.

In addition, all education projects should be asked to present data on how their target audiences have been affected by the educational material presented to them (students, staffers, etc.). If some standard questions could be asked through different projects (like "What was your perception of hydrogen and fuel cells a year ago/before the project? What's your perception now?), it would provide great data on how the education program is effecting change.

- The lack of a funding request for this important sub-program is disappointing and a little depressing. I am reminded of the daughter who, when advised by her father to become a doctor, responds "But dad, without teachers, there would be no doctors.”

GENERAL PROJECT EVALUATION FORM

PeerNet Evaluation Criteria: General Evaluation Form

Provide specific, concise comments to support your evaluation -- and, write clearly please.

1. Relevance to overall DOE objectives – the degree to which the project supports the Hydrogen Program and the goals and objectives in the Multi-Year RD&D plan. (Weight = 20%)

4 - Outstanding. Project is critical to Hydrogen Program and fully supports DOE RD&D objectives.
3 - Good. Most project aspects align with the Hydrogen Program and DOE RD&D objectives.
2 - Fair. Project partially supports the Hydrogen Program and DOE RD&D objectives.
1 - Poor. Project provides little support to the Hydrogen Program and the DOE RD&D objectives.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: General Evaluation Form

2. Approach to performing the work – the degree to which technical barriers are addressed, the project is well-designed, feasible, and integrated with other efforts. (Weight = 20%)

4 - Outstanding. Sharply focused on technical barriers; difficult to improve approach significantly.
3 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1 - Poor. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments regarding approach:

PeerNet Evaluation Criteria: General Evaluation Form

3. Technical Accomplishments and Progress toward overall project and DOE goals – the degree to which progress has been made, measured against performance indicators and demonstrated progress towards DOE goals. (Weight = 40%)

4 - Outstanding. Excellent progress toward objectives; suggests that barrier(s) will be overcome.
3 - Good. Significant progress toward objectives and overcoming one or more barriers.
2 - Fair. Modest progress in overcoming barriers; rate of progress has been slow.
1 - Poor. Little or no demonstrated progress towards objectives or any barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: General Evaluation Form

4. Collaboration and Coordination with other institutions - the degree to which the project interacts with other entities and projects. (Weight = 10%)

4 - Outstanding. Close, appropriate collaboration with other institutions; partners are full participants and well coordinated.
3 - Good. Some collaboration exists; partners are fairly well coordinated.
2 - Fair. A little collaboration exists; coordination between partners could be improved.
1 - Poor. Most work is done at the sponsoring organization with little outside collaboration; little or no apparent coordination with between partners.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: General Evaluation Form

5. **Proposed Future Work** – the degree to which the project has effectively planned its future in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and, when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

4 - Outstanding. Plans clearly build on past progress and are sharply focused on barriers.
3 - Good. Plans build on past progress and generally address overcoming barriers.
2 - Fair. Plans may lead to improvements, but need better focus on overcoming barriers.
1 - Poor. Plans have little relevance toward eliminating barriers or advancing the program.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: General Evaluation Form

Project Strengths:

Project Weaknesses:

PeerNet Evaluation Criteria: General Evaluation Form

Recommendations for Additions/Deletions to Project Scope:

TECHNOLOGY VALIDATION PROJECT EVALUATION FORM

PeerNet Evaluation Criteria: Technology Validation (TV)

1. Relevance to overall DOE objectives - the degree to which the project supports the goals and objectives of the Technology Validation Section of the Multi-Year RD&D plan. (Weight = 20%)

4 - Outstanding. Project is critical to the DOE Hydrogen Program RD&D objectives and fully addresses the Technology Validation key technical targets.
3 - Good. Project strongly supports the DOE Hydrogen Program RD&D objectives and addresses Technology Validation key technical targets.
2 - Fair. Project only partially supports the DOE Hydrogen Program RD&D objectives or the Technology Validation key technical targets.
1 - Poor. Project provides little support to the Hydrogen Program RD&D objectives or the Technology Validation key technical targets.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Technology Validation (TV)

2. Approach to performing the work – the degree to which technical barriers are addressed, the project is well-designed, feasible, and integrated with other efforts. (Weight = 20%)

4 - Outstanding. Sharply focused on technical barriers; difficult to improve approach significantly.
3 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1 - Poor. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Technology Validation (TV)

3. Technical Accomplishments and Progress toward overall project and DOE Technology Validation goals – the degree to which progress has been made, measured against performance indicators and demonstrated progress towards DOE goals. (Weight = 40%)

4 - Outstanding. Excellent progress toward objectives; suggests that barrier(s) will be overcome.
3 - Good. Significant progress toward objectives and overcoming one or more barriers.
2 - Fair. Modest progress in overcoming barriers; rate of progress has been slow.
1 - Poor. Little or no demonstrated progress towards objectives or any barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Technology Validation (TV)

4. Collaborations with other institutions - the degree to which the project interacts with industry partners, universities and laboratories. (Weight = 10%)

4 - Outstanding. Close, appropriate collaboration with other institutions; partners are full participants.
3 - Good. Some collaboration exists; full/needed coordination could be accomplished easily.
2 - Fair. A little collaboration exists; full/needed coordination would take ***additional*** significant effort.
1 - Poor. Most work is done at the sponsoring organization with little outside interaction.

- [ ] 4 - Outstanding
- [ ] 3 - Good
- [ ] 2 - Fair
- [ ] 1 - Poor

Comments:

PeerNet Evaluation Criteria: Technology Validation (TV)

5. Proposed Future Activities – the degree to which the project has effectively planned its future work in a logical manner. (Weight = 10%)

4 - Outstanding. Plans clearly build on past progress and are sharply focused on barriers.
3 - Good. Plans build on past progress and generally address overcoming barriers.
2 - Fair. Plans may lead to improvements, but need better focus on overcoming barriers.
1 - Poor. Plans have little relevance toward eliminating barriers or advancing the program.

- [ ] 4 - Outstanding
- [ ] 3 - Good
- [ ] 2 - Fair
- [ ] 1 - Poor

Comments:

APPENDIX C: EVALUATION FORM

PeerNet Evaluation Criteria: Technology Validation (TV)

Project Strengths:

Project Weaknesses:

PeerNet Evaluation Criteria: Technology Validation (TV)

Recommendations for Additions/Deletions to Project Scope:

AMERICAN RECOVERY AND REINVESTMENT ACT PROJECT EVALUATION FORM

PeerNet Evaluation Criteria: Hydrogen: ARRA

1a. Relevance
Is the project effort relevant to the American Recovery and Reinvestment Act (ARRA) of 2009 goals: Create new jobs as well as save existing ones; spur economic activity and invest in long-term economic growth.(Weight = 20%)

4 - Outstanding. Project is very relevant and will make substantial contributions to the ARRA 2009 goals.
3 - Good. Project is relevant and will make moderate but significant contributions to the ARRA 2009 goals.
2 - Fair. Project is somewhat relevant and will make some contribution to the ARRA 2009 goals.
1 - Poor. Project is not relevant and is unlikely to contribute to the ARRA 2009 goals.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Hydrogen: ARRA

1b. Relevance
Does the project's technology development plan and/or deployment plan address the FCT ARRA project goals of accelerating the commercialization and deployment of fuel cells and fuel cell manufacturing, installation, maintenance, and support services?

4 - Outstanding. Project is very relevant and will make substantial contributions to FCT ARRA project goals.
3 - Good. Project is relevant and will make moderate but significant contributions to FCT ARRA project goals.
2 - Fair. Project is somewhat relevant and will make some contributions to FCT ARRA goals.
1 - Poor. Project is not relevant, and is unlikely to contribute to the FCT ARRA goals.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Hydrogen: ARRA

2. Development/Deployment Approach
Are the project's technical and deployment milestones and schedule clearly identified, appropriate, and feasible, and are technical and commercial barriers and risks adequately addressed? (Weight: 30%)

4 - Outstanding. Project team sharply focused on achieving milestones, overcoming barriers, and managing risks; difficult to improve approach significantly.
3 - Good. Appropriate milestones and schedule identified, and barriers and risks addressed. Effort likely to achieve project goals, but approach could be improved.
2 - Fair. Approach has significant weaknesses; but may contribute towards achieving most project goals.
1 - Poor. Unlikely to make progress towards project goals, and/or barriers, risks are not adequately addressed.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Hydrogen: ARRA

3. Technical Accomplishments and Progress
What is the overall progress towards project's objectives and milestones? Is progress adequately reported and quantified (e.g., number of jobs, installations, etc.) as required by ARRA? (Weight = 40%)

4 - Outstanding. Excellent progress toward objectives and milestones; barrier(s) likely to be overcome.
3 - Good. Significant progress towards objectives and overcoming one or more barriers.
2 - Fair. Rate of technical progress is slow, some progress made in overcoming barriers.
1 - Poor. Little or no demonstrated progress towards objectives, or towards overcoming barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

PeerNet Evaluation Criteria: Hydrogen: ARRA

4. Collaborations
Does the project team effectively use collaborations between partners and with other industrial, commercial, university or research organizations to achieve its objectives?

4 - Outstanding. Effective collaboration between partners and with other institutions enhance probability of success of effort.
3 - Good. Some collaboration exists; partners are fairly well coordinated.
2 - Fair. Minimal collaboration exists; coordination between partners could be improved.
1 - Poor. Little coordination between partners, or collaboration with other organizations exist.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

Comments:

APPENDIX C: EVALUATION FORM

PeerNet Evaluation Criteria: Hydrogen: ARRA

Project Strengths:

Project Weaknesses:

PeerNet Evaluation Criteria: Hydrogen: ARRA

Specific Recommendations:

LIST OF PROJECTS NOT REVIEWED

Project ID	Project Title	PI Name	Organization
ARRA-04	Advanced Direct Methanol Fuel Cell for Mobile Computing	Jim Fletcher	University of North Florida
ARRA-05	Jadoo Power Fuel Cell Demonstration	Ken Vaughn	Jadoo Power
ARRA-12	Demonstrating the Economic and Operational Viability of 72-Hour Hydrogen PEM Fuel Cell Systems to Support Emergency Communications on the Sprint - Nextel Network	Kevin Kenny	Sprint
BES-01	Fluoropolymers, Electrolytes, Composites and Electrodes	Stephen Creager	Clemson University
BES-02	Ab-initio Screening of Alloys for Hydrogen Purification Membranes	David Sholl	Georgia Institute of Technology
BES-03	Theory, Modeling, and Simulation of Ion Transport in Ionomer Membranes	Philip Taylor	Case Western Reserve University
BES-04	The Study of Proton Transport Using Reactive Molecular Dynamics	David Keffer	University of Tennessee
BES-05	Surface-Directed Fabrication of Integrated Membrane-Electrode Interfaces	Kane Jennings	Vanderbilt University
BES-06	Activity and Stability of Nanoscale Pt-based Catalysts	Yang Shao-Horn	Massachusetts Institute of Technology
BES-07	Cathode Catalysis in Hydrogen/Oxygen Fuel Cells: Mechanism, New Materials, and Characterization	Andrew Gewirth	University of Illinois
BES-08	Fundamental Studies of Electrocatalysis for Low Temperature Fuel Cell Catalysts	Nenad Markovic	ANL
BES-09	Engineering Catalytic Nanoporous Metals for Reactions Important to the Hydrogen Economy	Jonah Erlebacher	Johns Hopkins University
BES-10	Theoretical Insights Into Active and Durable Oxygen Reduction Catalysts	Matthew Neurock	University of Virginia

BES-11	An in situ Electrode-Potential-Controlled Nuclear Magnetic Resonance Investigation of Sulfur-Poisoning Effect on Pt-Based Mono- and Bi-metallic Nanoscale Electrocatalysts	YuYe Tong	Georgetown University
BES-12	Investigation of the Oxygen Reduction Reaction Activity of Heteroatom-containing Carbon Nano-structures	Umit Ozkan	Ohio State University
BES-13	In-Situ Studies of Active Sites and Mechanism for the Water-Gas Shift Reaction on Metal/Oxide Nanocatalysts	Jose Rodriguez	BNL
BES-14	Bio-Inspired Molecular Catalysts for Hydrogen Oxidation and Hydrogen Production	Morris Bullock	PNNL
BES-15	Structure/Composition/Function Relationships in Supported Nanoscale Catalysts for Hydrogen	Peter Stair	Northwestern University & ANL
BES-16	Fundamentals of Hydroxide Conducting Systems for Fuel Cells and Electrolyzers	Bryan Pivovar	NREL
BES-17	Transport Phenomena and Interfacial Kinetics in Planar Microfluidic Membraneless Fuel Cells	Hector Abruna	Cornell University
BES-18	High Performance Nano-Crystalline Oxide Fuel Cell Materials	Thomas O. Mason	Northwestern University
BES-19	Nanostructured, metal-modified oxide catalysts for steam reforming of methanol and the water-gas shift reactions	Maria Flytzani-Stephanopoulos	Tufts University
BES-20	Strategies for Probing Nanometer-Scale Electrocatalysts: From Single Particles to Catalyst-Membrane Architectures	Carol Korzeniewski	Texas Tech University
BES-21	Atomic-scale Design of a New Class of Alloy Catalysts for Reactions Involving Hydrogen: A Theoretical and Experimental Approach	Manos Mavrikakis	University of Wisconsin
BES-22	Multiscale Tailoring of Highly Active and Stable Nanocomposite Catalysts for the Production of Clean Hydrogen Streams	Gotz Veser	University of Pittsburgh
BES-23	Metal- and Metal Oxide-Supported Platinum Monolayer Electrocatalysts for Oxygen Reduction	Radoslav Adzic	BNL

BES-24	Development and Mechanistic Characterization of Alloy Fuel Cell Catalysts	Anders Nilsson	Stanford Linear Accelerator Laboratory
BES-25	Metal dissolution mechanisms in Pt-based alloys: Ideas for advanced PEM cathode design	Perla Balbuena	Texas A&M University
BES-26	Fundamental Studies of Electrocatalysis for Low Temperature Fuel Cell Catalysts (Poster of BES008 oral presentation)	Hoydoo You	ANL
BES-27	Theoretical Studies of Water-Gas Shift Reaction on Metal-Oxide Catalysts	Ping Liu	BNL
BES-30	Mechanism of Proton Transport in Proton Exchange Membranes: Insights from Computer Simulation	Greg Voth	University of Chicago
BES-31	Porous and Glued Ultrathin Membranes	Stephen Regen	Lehigh University
BES-32	The Development of Nano-Composite Electrodes for Solid Oxide Electrolyzers	Raymond Gorte	University of Pennsylvania
BES-33	Charge Transfer, Transport, and Reactivity in Complex Molecular Environments: Theoretical Studies for the Hydrogen Fuel Initiative	Michel Dupuis	PNNL
BES-34	Proton Conduction in Rare-earth Phosphates	Lutgard De Jonghe	LBNL
BES-35	The Dielectric Response of Hydrated PFSA Membranes – Measurements with Single Post Dielectric Resonators	Stephen Paddison	University of Tennessee
ED-01	Hydrogen Safety Training for First Responders	Steven Weiner	PNNL
ED-02	Education for Emerging Fuel Cell Technologies	Carl Rivkin	NREL
ED-16	Hydrogen Technology and Energy Curriculum (HyTEC)	Barbara Nagle	Lawrence Hall of Science at UC-Berkeley
ED-17	H2 Educate! Hydrogen Education for Middle Schools	Mary Spruill	NEED
ED-18	Hydrogen Knowledge and Opinions Assessment	Rick Schmoyer	ORNL

FC-49	Development of Micro-Structural Mitigation Strategies for PEM Fuel Cells: Morphological Simulations and Experimental Approaches	Silvia Wessel	Ballard
FC-53	Low Cost, Durable Seals for PEM Fuel Cells	Jason Parsons	UTC Power
FC-54	Transport Studies and Modeling in PEM Fuel Cells	Cortney Mittelsteadt	Giner Electrochemical Systems, LLC
FC-55	Subfreezing Start/Stop Protocol for an Advanced Metallic Open-Flowfield Fuel Cell Stack	Amedeo Conti	Nuvera Fuel Cells
FC-56	Visualization of Fuel Cell Water Transport and Performance Characterization Under Freezing Conditions	Satish Kandlikar	Rochester Inst. of Technology
FC-57	7C: Intergovernmental Stationary Fuel Cell System Demonstration	Richard Chartrand	Plug Power Inc.
FC-58	Research & Development for Off-Road Fuel Cell Applications	Mike Hicks	IdaTech, LLC
FC-61	Diesel Fueled SOFC System for Class 7/Class 8 On-Highway Truck Auxiliary Power	Dan Norrick	Cummins
FC-62	Solid Oxide Fuel Cell Development for Auxiliary Power in Heavy Duty Vehicle Applications	Gary Blake	Delphi
FC-63	Novel Materials for High Efficiency Direct Methanol Fuel Cells	Chris Roger	Arkema
FC-64	New MEA Materials for Improved DMFC Performance, Durability, and Cost	Jim Fletcher	University of North Florida
FC-65	The Effect of Airborne Contaminants on Fuel Cell Performance and Durability	Richard Rocheleau	University of Hawaii
FC-66	Development of Thermal and Water Management System for PEM Fuel Cell	Zia Mirza	Honeywell
FC-67	Materials and Modules for Low Cost, High Performance Fuel Cell Humidifiers	Will Johnson	W.L. Gore
FC-68	Center for Fundamental and Applied Research in Nanostructured and Lightweight Materials	Michael Mullins	Michigan Technological University

FC-69	Renewable and Logistics Fuels for Fuel Cells at the Colorado School of Mines	Neal Sullivan	Colorado School of Mines
FC-70	Development of Kilowatt-Scale Fuel Cell Technology	Steven Chuang	University of Akron
FC-71	Alternative Fuel Cell Membranes for Energy Independence	Kenneth Mauritz	University of Southern Mississippi
FC-72	Extended Durability Testing of an External Fuel Processor for SOFC	Mark Perna	Rolls Royce Fuel Cell Systems Inc.
FC-73	Hydrogen Fuel Cell Development in Columbia (SC)	Kenneth Reifsnider	University of South Carolina
FC-74	Martin County Hydrogen Fuel Cell Development	Jeffrey Bonner-Stewart	Martin County Economic Development Corporation
FC-75	Fuel Cell Balance of Plant Reliability Testbed	Vern Sproat	Stark State College of Technology
PD-01	Investigation of Reaction Networks and Active Sites in Bio-Ethanol Steam Reforming over Co-based Catalysts	Umit Ozkan	Ohio State University
PD-43	Developing Improved Materials to Support the Hydrogen Economy	Michael Martin	Edison Materials Tech Center
PD-44	Purdue Hydrogen Systems Laboratory	Jay Gore	Purdue University
PD-49	H2 Permeability and Integrity of Steel Welds	Zhili Feng	ORNL
PD-50	Coatings for Centrifugal Compression	George Fenske	ANL
PD-57	Photoelectrochemical Hydrogen Production	Malay Mazumder	University of Arkansas Little Rock
PD-60	Advanced Sealing Technology for Hydrogen Compressors	Hooshang Heshmat	Mohawk Innovative Technologies
PD-61	Photochemical System for Hydrogen Generation	Alexander Parfenov	Physical Optics Corporation
PD-62	Nanotube Array Photoelectrochemical Hydrogen Production	Rikard Wind	Synkera Technologies Inc.

PD-63	Aqueous Phase Base-Facilitated Reforming (BFR) of Renewable Fuels	Brian James	Directed Technologies, Inc.
PD-64	Advanced PEM Based Hydrogen Home Refueling Appliance	Michael Pien	ElectroChem, Inc.
PD-65	Unitized Design for Home Refueling Appliance for Hydrogen Generation to 5,000 psi	Timothy Norman	Giner, Inc.
PD-66	Design, Optimization and Fabrication of a Home Hydrogen Fueling System	Brian Hennings	Lynntech
PD-67	Hydrogen by Wire - Home Fueling System	Luke Dalton	Proton Energy Systems
PD-68	Modeling Hydrogen Dispensing Options for Advanced Storage	Kurtis McKenney	TIAX, LLC
PD-69	Development of a Hydrogen Home Fueling System	Greg Tao	Materials and Systems Research, Inc.
PD-70	One Step Biomass Gas Reforming-Shift Separation Membrane Reactor	Mike Roberts	Gas Technology Institute
PD-71	High Performance, Low Cost Hydrogen Generation from Renewable Energy	Katherine Ayers	Proton Energy Systems
PD-72	Development of Hydrogen Selective Membranes/Modules as Reactors/Separators for Distributed Hydrogen Production	Paul Liu	Media and Process Technology Inc.
PD-73	Zeolite Membrane Reactor for Water-Gas-Shift Reaction for Hydrogen Production	Jerry Y.S. Lin	Arizona State University
PD-74	Rapid Low Loss Cryogenic H2 Refueling	Salvador Aceves	LLNL
PD-75	Range Optimization for Fuel Cell Vehicles	Zhenhong Lin	ORNL
PD-76	Photoelectrochemical Generation of Hydrogen from Water Using Visible Light Sensitive Ferro-Electric BiFeO3 and Semiconductor Nanotubes	Mano Misra	University of Nevada Reno
SCS-11	Risk-Informed Separation Distances for H2 Facilities	Daniel Dedrick	SNL
ST-14	Overview of the DOE Hydrogen Sorption Center of Excellence	Lin Simpson	NREL

ST-16	Enhanced Hydrogen Dipole Physisorption: Henry's Law and Isosteric Heats in Microporous Sorbents	Channing Ahn	California Institute of Technology
ST-17	Single-Walled Carbon Nanohorns for Hydrogen Storage and Catalyst Supports	David Geohegan	ORNL
ST-20	Neutron Characterization in Support of the Hydrogen Sorption Center of Excellence	Dan Neumann	NIST
ST-29	5-Year Review of Metal Hydride Center of Excellence	Lennie Klebanoff	SNL
ST-33	Discovery and Development of Metal Hydrides for Reversible On-board Hydrogen Storage	Mark Allendorf	SNL
ST-34	Aluminum Hydride Regeneration	Jason Graetz	BNL
ST-35	Reversible Hydrogen Storage Materials - Structure, Chemistry, and Electronic Structure	Ian Robertson	University of Illinois
ST-36	2010 Overview and Wrapup: DOE Chemical Hydrogen Storage Center of Excellence	Kevin Ott	LANL
ST-39	Amineborane-Based Chemical Hydrogen Storage	Larry Sneddon	University of Pennsylvania
ST-42	Low-Cost Precursors to Novel Hydrogen Storage Materials	Suzanne Linehan	Dow Chemical Company
ST-43	Ammonia Borane Regeneration and Market Analysis of Hydrogen Storage Materials	David Schubert	U.S. Borax
ST-52	Best Practices for Characterizing Hydrogen Storage Properties of Materials	Karl Gross	H2 Technology Consulting, LLC
ST-56	Solutions for Chemical Hydrogen Storage: Dehydrogenation of B-N and C-C Bonds	Karen Goldberg	University of Washington
ST-57	Chemical Hydrogen Storage Using Ultra-High Surface Area Main Group Materials & The Development of Efficient Amine-Borane Regeneration Cycles	Philip Power	University of California Davis
ST-58	Electrochemical Hydrogen Storage Systems	Digby Macdonald	Pennsylvania State University

ST-59	Chemical Hydrogen Storage Using Aluminum Ammonia-Borane Complexes	Fred Hawthorne	University of Missouri
ST-60	Main Group Element and Organic Chemistry for Hydrogen Storage and Activation	David Dixon	University of Alabama
ST-61	Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage: Structure and Kinetics of Nanoparticle and Model System Materials	Bruce Clemens	Stanford University
ST-62	Discovery of H2 Storage Materials: LiMgN and Mg-Ti-H	Zak Fang	University of Utah
ST-63	Electrochemical Reversible Formation of Alane	Ragaiy Zidan	SRNL
ST-64	First-Principles Modeling of Hydrogen Storage in Metal Hydride Systems	Karl Johnson	University of Pittsburgh/ Georgia Institute of Technology
ST-65	Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage	Ping Liu	HRL Laboratories
ST-66	Catalyzed Nano-Framework Stabilized High Density Reversible Hydrogen Storage Systems	Xia Tang	UTRC
ST-67	Neutron Characterization and Calphad in Support of the Metal Hydride Center of Excellence	Terry Udovic	NIST
ST-68	Metal Borohydrides, Ammines, and Aluminum Hydrides as Hydrogen Storage Materials	Gilbert Brown	ORNL
ST-69	Development and Evaluation of Advanced Hydride Systems for Reversible Hydrogen Storage	Joe Reiter	NASA JPL
ST-70	Amide and Combined Amide/Borohydride Investigations	Don Anton	SRNL
ST-71	Effect of Trace Elements on Long-Term Cycling/Aging Properties and Thermodynamic Studies of Complex Hydrides for Hydrogen Storage	Dhanesh Chandra	University of Nevada Reno
ST-72	Synthesis of Nanophase Materials for Thermodynamically Tuned Reversible Hydrogen Storage	Channing Ahn	California Institute of Technology
ST-74	Hydrogen Storage Materials with Binding Intermediate between Physisorption and Chemisorption	Juergen Eckert	University of California Santa Barbara

ST-75	Optimization of Nano-Carbon Materials for Hydrogen Sorption	Boris Yakobson	Rice University
ST-76	Nanoengineered Graphene Scaffolds with Atom Substitution for H2 Adsorption	Jim Tour	Rice University
ST-77	Carbon Aerogels for Hydrogen Storage	Ted Baumann	LLNL
ST-78	Hydrogen Storage by Spillover	Ralph Yang	University of Michigan
ST-79	Characterization of Hydrogen Adsorption by NMR	Yue Wu	University of North Carolina
ST-80	Advanced Boron and Metal Loaded High Porosity Carbons	Mike Chung	Pennsylvania State University
ST-81	Optimizing the Binding Energy of Hydrogen on Nanostructured Carbon Materials through Structure Control and Chemical Doping	Jie Liu	Duke University
ST-82	Discovery of Materials with a Practical Heat of H2 Adsorption	Alan Cooper	Air Products
ST-83	Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage Vessels	Alex Ly	Quantum Fuel Systems Technologies Worldwide, Inc.
ST-84	Purdue Hydrogen Systems Laboratory	Jay Gore	Purdue University
ST-85	HGMS: Glasses and Nanocomposites for Hydrogen Storage	Kristina Lipinska-Kalita	University of Nevada Las Vegas
ST-86	The H-Prize	Jeffrey Serfass	Hydrogen Education Foundation
TV-03	Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project	Mike Veenstra	Ford
TV-04	Hydrogen to the Highways	Ron Grasman	Daimler

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2010 Annual Merit Review Survey Questionnaire Results

The 2010 DOE Hydrogen Program and Vehicle Technologies Program Merit Review and Peer Evaluation Meeting was held June 7–11, 2010, at the Marriott Wardman Park in Washington, D.C. A plenary session was held on Monday afternoon, and oral presentations were held in eight parallel sessions all day Tuesday, Wednesday, and Thursday, and a half day on Friday. There were 349 Hydrogen Program presentations—with 218 presented orally and 131 presented in poster sessions. Over 1,700 people attended the meeting. This report documents results from a survey questionnaire given to all participants.

1.) What is your affiliation?

	Number of Responses	Response Ratio
Government agency directly sponsoring the program under review	8	5.6%
National/government lab, private sector or university	65	45.7%
In an industry directly involved in the program under review	32	22.5%
In an industry with interest in the work under review	17	11.9%
Government agency with interest in the work	7	4.9%
Other	10	7.0%
No Response	3	2.1%
Total	142	100%

"Other" Responses
Support contractor
Not-For-Profit R&D
Academia
An observer of fuel cell industry and government policy
University
Consultant
NIST / TIP
German not for profit organization
International Government Agency

2.) Purpose and scope of the Annual Merit Review were well defined. Answer only if you attended the plenary on Monday.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	4	5	56	34
2%	4%	5%	55%	34%

12 Comment(s)

The presentations seemed to be a self-lauding exercise for the presenters. But we, the reviewers, seemed most interested in details, such as what the presenters learned and how they plan to expand their work.

The fact that it is a joint review of two programs seemed to be lost on some of the plenary speakers.

The plenary was a meeting for battery electric and plug-in hybrids and did not address hydrogen other than superficially. The HFCV (hydrogen fuel cell vehicle) program has met all its targets (from competitive storage targets to expected battery targets) and is treated apathetically by DOE management. That is not rational management.

The status was well reviewed; however, unlike in the past, there was not much mention of future programs and solicitations. This could be due to the fact that future programs have not been decided on yet, but mentioning that would be better than saying nothing on the subject at all.

Honestly, the plenary session was not very useful. Overviews of the research were not detailed enough. If one is going to spend time at the plenary session, please present something important and useful. Research presented from higher level officials usually means the presentation lacks details, with generic statements and opinions being substituted for said details. Those aforementioned substitutions are meaningless.

The plenary talks, primarily the initial section of high-level talks, were a disappointment this year. Some speeches were poorly prepared and the talks in general did not sufficiently address the DOE’s Hydrogen Program.

The intent of the plenary session was clear, but the process by which the review comments will be used going forward is not completely understood.

The meeting provided insufficient descriptions of overall program goals and how the review results are going to be used.

A few presentations seemed to be primarily concerned with the format of the review rather than providing an actual purpose.

With a little fine tuning, this program would excel on most fronts.

It was surprising to hear that only 85% of the presentations would be reviewed. How does one find out if a particular project was reviewed, and if so, what the results of that review are?

The plenary session was very good.

3.) The plenary presentations were helpful to understanding the direction of the Hydrogen and Vehicle Technologies Programs. Answer only if you attended the plenary on Monday.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
7	9	16	42	13
8%	10%	18%	48%	15%

21 Comment(s)

The plenary presentations varied in quality and direction. Pat Davis' presentation was particularly good at explaining the VTP (Vehicle Technologies Program) direction. Other presentations were not as successful.

Rick Farmer's and Pat Davis' presentations were helpful in understanding the direction of the Hydrogen and Vehicle Technologies Program.

Several of the talks were poor. Some presentations could have been deleted without losing anything.

It is well understood that the Administration is not supportive of fuel cell development. Nonetheless, it is not helpful that the plenary presentations did not pay much attention to fuel cell issues. The DOE leaders at least should pay respect to the efforts and sacrifices that are made for fuel cell development and give equal time to fuel cell speakers.

While DOE managed a $2.5 billion ARRA (American Recovery and Reinvestment Act of 2009) program, there were no funds devoted to a HFCV (hydrogen fuel cell vehicle) market transformation program succeeding a technology validation effort. One senior official mentioned forklifts but there was no real recognition of where the Hydrogen Program has progressed or any recognition or support for the CA ZEV (California's Zero Emission Vehicle) mandate.

The status was well reviewed. However, unlike in the past, there was not much mention of future programs and solicitations. This could be due to the fact that future programs have not been decided on yet, but mentioning that would be better than saying nothing on the subject at all.

This year included talks from EPA (Environmental Protection Agency) and TARDEC. While interesting and clearly relevant to the program, it did little to give a better understanding of the program direction, particularly the TARDEC talk. It's understandable that the successes and failures of the partnership could affect the future regulatory landscape, for example, EPA and NHTSA (National Highway Traffic Safety Administration).

The goals and directions of the programs were clearly presented.

It was interesting that hydrogen was included as a part of a broader focus on diversity along with energy systems as a whole, especially for the transportation sector, but was de-emphasized in the early plenary presentations.

Comments from the first speakers largely ignored hydrogen and fuel cells. I am concerned that one senior official seemed unconcerned about his audience's position.

A timeline displayed per program would have been helpful to understand where the program is in its maturity.

The non-DOE speakers did not even mention hydrogen and barely said anything about fuel cells. The plenary room needed larger screens, louder sound, and more aisles (shorter rows) to accommodate audience seating.

The presentations overlapped quite a bit.

There was almost no mention of hydrogen, and the speakers said "fuel cells" three times in total.

As a member of the hydrogen and fuel cell program, it was very obvious that this program was only mentioned as an afterthought. I suggest that all plenary talks be removed, with the exception of the Program Manager Overview presentations.

The direction of the Hydrogen Program is unclear, and the plenary presentations did not help clarify the direction and goals of the Hydrogen Program.

This comment would be different from those made by people at the AMR for Office of Vehicle Technologies purposes. For those who were there for FCT (fuel cell technology) purposes, the message that was conveyed (by omission) was that hydrogen and FCT are of no importance or value to the DOE or the community at large.

Some presentations seemed to be aimed at a more general audience, not an audience of people who are very knowledgeable about the industry. Also, it was almost completely battery-focused with very little mention of the fuel cell and hydrogen program.

The presentations were too generic.

Is there access to the presentations? They were not found on the CD provided during registration. Are they online somewhere?

The plenary session was somewhat light on details regarding the overall planning of the programs.

4.) Sub-program overviews were helpful to understanding the research objectives. Answer only if you attended one or more sub-program overviews.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	1	9	69	39
1%	1%	8%	58%	33%

8 Comment(s)

Hearing from the sub-program leaders is important to set the stage for the detailed work discussions that follow.

Presentations were consistent and to the point between several technology team sessions. The presentations provided clear targets, funding, status, etc., for all in attendance.

The overviews were somewhat "business as usual." Many presentations simply listed what occurring with a very limited number of "success stories." The presenters were good.

The overviews were short but focused.

It might be an improvement if these sessions were 30 minutes long. Many sessions run over, which makes the rest of the day more difficult with respect to keeping time.

Some presentations were helpful, while some were not as clear.

It is often difficult to attend all presentations within a specific sub-program, which makes the overview session very beneficial in understanding the overall program objectives, progress, and budget.

The program reviews were excellent. It's challenging for reviewers to remain mentally focused having to go through so many advanced research presentations back to back. The PIs are at a disadvantage when asked to present their work without the full benefit of time on their side.

5.) What was your role in the Annual Merit Review? Check the most appropriate response. If you are both a presenter and a reviewer and want to comment as both, complete the evaluation twice, once as each.

	Number of Responses	Response Ratio
Attendee, neither Reviewer nor Presenter	63	44.3%
Presenter of a project	53	37.3%
Peer Reviewer	23	16.1%
No Response	3	2.1%
Total	142	100%

6.) The quality, breadth, and depth of the following were sufficient to contribute to a comprehensive review:

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Presentation	0	1	5	30	24
	0%	2%	8%	50%	40%
Question and answer periods	1	2	6	34	17
	2%	3%	10%	57%	28%
Answers provided to programmatic questions	0	2	10	31	17
	0%	3%	17%	52%	28%
Answers provided to technical questions	0	3	5	35	17
	0%	5%	8%	58%	28%

7 Comment(s)

More time was needed for the question and answer session. More technical details should be available before the review so one can investigate and ask more detailed questions.

The reviews for the national labs were very well done with good comments, as well as focused challenges and direction. The reviews of the industry presentations on Thursday (section V) were poor. The reviewers asked few questions, and nothing was really challenged.

The sessions were very well run. Reviewers were generally objective with just one or two having an overly negative and confrontational attitude.

The auto companies were never asked about how the DOE funding applied to their respective programs; instead, they just talked about their vehicles.

Many of the presentations lacked depth.

Some of the presenters were not very knowledgeable about their topic. It was apparent they were more business-related and less technically informed.

It is understood that the Q&A had to be limited to 10 minutes due to the number of projects being reviewed, but in some cases this was an insufficient amount of time.

7.) Enough time was allocated for presentations.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	3	7	35	15
0%	5%	12%	58%	25%

7 Comment(s)

In some cases it would have made more sense to allot presentation time commensurate with project size.

Some of the presentations needed more time to be effective.

Time allocation was perfect!

As always, 20 minutes is not much time to properly present some of the complex projects, but it is the best that can be done when you factor in the number of projects and the length of the event.

There was not enough allocated time for a few projects where the scope was larger and more complex than average.

There was enough time allocated, yet many presenters were not prepared for the time limitation.

Some presenters did not do a sufficient job managing their limited time.

8.) The questions asked by reviewers were sufficiently rigorous and detailed.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	5	16	31	6
2%	8%	27%	53%	10%

8 Comment(s)

Some reviewers were better at this than others. Are guidelines provided to reviewers?

Many reviewers were not specialized enough in certain fields to fully understand the ramifications of the projects and ask good questions.

Some review panels were not up to the task.

Reviewers did not ask sufficiently detailed questions to the automakers or to the presenters using Clean Cities funds for education and outreach. At all other sessions attended, the reviewers asked great questions.

The reviewers need to be tougher. We allocate money from taxpayers to support the industrial research, and we need to know exactly where the money has been spent and what exactly the program status is.

The panel questions were usually not very probing. It seemed more like an exercise and less like a review.

The reviewers and their questions were great this year for the fuel cell track.

Some reviewers use the Q&A sessions to get on their own "soap box" while displaying little to no objectivity in their review. Note that this is a minority of the reviewers, but the few that do it take away from an objective review process.

9.) The frequency (once per year) of this formal review process for this Program is:

	Number of Responses	Response Ratio
About right	53	37.3%
Too frequent	2	1.4%
Not frequent enough	3	2.1%
No opinion	2	1.4%
No Response	82	57.7%
Total	142	100%

6 Comment(s)

The frequency is good, though the timing was a bit off, considering the presentations were all outdated and next year's budget is already in place.

One consideration may be to review each project every other year. In other words, the AMR would be held each year but at about half the current size. The quality may improve as a result.

Having the event take place more frequently makes no sense as it takes time to make significant progress on something so complex. About 18 months would probably be okay, while 24 months is too long.

Every other year would be sufficient.

I suggest a bi-weekly review showcasing different tracks.

It is difficult to attend the full AMR as it is necessary to miss an entire week of work to do so. Is it practical, feasible, and convenient for the DOE to split the AMR into two meeting events (spring and fall)?

10.) Logistics, facilities and amenities were satisfactory.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	5	26	28
0%	2%	8%	43%	47%

13 Comment(s)

The DOE should require that all of the facilities in use during the AMR (including the hotel) offer recycling.

Great work on the facilities!

The conference rooms were spread out too far and some were oddly shaped, which was not conducive to PowerPoint presentations.

Everything was outstanding!

The only glitches that occurred involved issues with room sizes.

Please consider cheaper venues. Paying $250 or more a night for a hotel is very expensive for many average companies. There are many locations available for around $100 a night.

The Wardman Hotel facilities were terrific.

The hotel was great and was also an improvement over Crystal City.

This aspect of the event was highly impressive and deserves my congratulations.

Judi Abraham did an amazing job by moving the entire Fuel Cells Session at a moment's notice when the session was overcrowded and the air conditioning was not working well on the first day! The hotel in general was much nicer than Crystal City.

The hotel was very expensive.

The DOE might consider moving the review out of D.C. While this location is convenient for the few DOE participants located in D.C., the expense for the rest of the participants is significant. This review will end up costing roughly $2,500 on one particular company's DOE contracts. This is money that is no longer available to conduct research.

The initial room for the fuel cell session was much too small. The session was moved to a larger room in a less prominent section of the hotel.

11.) The visual quality of the presentations was adequate. I was able to see all of the presentations I attended.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	12	8	25	13
2%	20%	14%	42%	22%

17 Comment(s)

The screens were placed too low. The heads of the people often blocked text. Most presenters used small fonts and lots of text, which created more viewing trouble.

Long, narrow rooms made viewing difficult.

The text and graphs on slides should be larger.

Some projector screens seem to be a little small and hard to read if one sat in the back rows.

In some of the rooms, it was difficult to see slides.

In some rooms the screens were blocked by the heads of attendees. If the screens were up a few more feet, it would have been much better. Either that, or select a location with tiered seating. But lifting screens a few feet would be much more practical.

The plenary talks were an issue as the screen was not readable for half of the audience. The technical sessions were better, but the screens were still too small.

The screens were too small in the grand ballrooms.

For such big conference rooms, the font was too small on most of the presentations.

The visual quality was adequate in the tracks. But, the screens were too small for Monday evening's plenary.

Some rooms were well laid out for viewing the presentations, but some, particularly the balconies, were not. It was very hard to read the slides from the back of the room in the long, skinny balcony rooms.

In some of the venues, the presentation screens were too low.

The overhead lights in the FC session could not be adjusted over the screen. We improvised (by turning off most of the lights) when the presenter needed the presentation photos to be seen clearly. At the plenary, the slides were unreadable from the back of the massive hall.

Many of the screens were too small to see the details on the screen. Either larger screens should be available, or the PIs need to be instructed not to put so much material on one slide.

It would be great to have the screens larger and higher. Most of the conference was spent trying to look through someone's head at the presentation. Please find a way to make the screens visible to everyone in the audience.

It was sometimes hard to see the screens. This was due to a combination of views being obstructed by people's heads and/or too much lighting above the screen.

It was very difficult to see the slides due to people's heads being in the way and the way the chairs were set up.

12.) The audio quality of the presentations was adequate. I was able to hear all the presentations I attended.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	3	32	23
0%	0%	5%	55%	40%

3 Comment(s)

Sometimes the audio/visual person let the microphone “hum” with some feedback that was annoying.

It was sometimes hard to hear during the Q&A. Next time, the moderator just needs to make sure that he/she forces the audience to use microphones.

Although the presenters could be heard, it was often difficult to hear questions from the audience.

13.) The meeting hotel accommodations (sleeping rooms) were satisfactory.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	14	12	19
0%	0%	31%	27%	42%

6 Comment(s)

This hotel location (Adams-Morgan) is much better than the Crystal City location of past reviews.

Overall, it would be preferable to stay at a hotel with free Internet access in the rooms and/or free Internet access in the business center, neither of which this hotel had. They did a great job with everything else, and the accommodations were thoroughly satisfying.

The hotel was nice but expensive!

The hotel was expensive.

The hotel was full when trying to reserve a room.

A different hotel was used.

14.) The information about the Review and the hotel accommodations sent to me prior to the Review was adequate.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	3	28	25
0%	2%	5%	49%	44%

2 Comment(s)

The dates should be locked in sooner for attendees. Thank you for sending along the dates for 2011 with this survey invitation.

The agenda needs to have meeting rooms on it and a hotel map. If the agenda could all fit on two sides of one sheet, that would be perfect.

15.) What was the most useful part of the review process?

33 Response(s)

The most useful part of the review process was personal interactions and having a place where everyone is present to conduct conversations and meetings.

Learning about each participant's role was most useful.

The Q&A sessions were the most productive part of the review process.

The Q&A sessions after the presentations and talking with folks in between sessions proved to be very useful.

It was extremely productive getting to know the major players, such as researchers and companies with related products.

The individual presentations proved to be the most effective part of the review process. The presentations were extremely helpful compared to anything else.

Attending a few of the technical sessions on lithium ion batteries was useful.

There's been very impressive progress! The overview of projects supported by the DOE provided industry an opportunity to consider future collaboration areas with national labs.

The most useful aspect was one place to learn about all the DOE projects. The most detrimental was the fact that there was a limited opportunity to ask questions.

There were great technical presentations as well as questions and answers.

The most effective aspect of the conference was the questions from the audience.

It was very fruitful hearing about the technical progress and plans for the projects.

The fact that there was an ability to meet with nearly everyone who is active in the field in the U.S. was incredibly useful.

Making all presenters use the same slide format proved to be useful, as it made the presentations very easy to understand.

It was really beneficial getting to see related work beyond one's own project.

It was very helpful having great opportunities to meet with people at the breaks and lunches. Both were very beneficial.

The electronic copies of the presentations were very useful.

The technical information proved to be very worthwhile. The posters and poster process were better than most other conferences with posters.

The number of topics covered, poster sessions, and networking opportunities were all extremely constructive.

The presentation materials were interesting.

The AMR gave a larger vision to these areas. It also gave people an opportunity to communicate with each other.

Not being a reviewer and therefore not being tied to any particular topic or presentation, one participant was able to move around among various topics and presentations in order to cover many bases.

The process itself was highly informative and appropriate for its purpose. The technical part of the presentations proved to be the most advantageous.

The presentations and the Q&A sessions were both gainful.

It's great to see the real progress that is being made in the hydrogen vehicle technologies program and all the other DOE programs. Also, the forum serves as a good networking event among hydrogen industry colleagues.

The most valuable aspect was using PeerNet. And, as an organizing committee member, using Sharepoint was great. Having the presentations ahead of time helped the reviewers.

The ability to make contact with PIs and others across many organizations is really important. Also, the ability to see in a quick nutshell what is going on across the board is good.

An understanding of the scope of the DOE work and the ability to identify key programs of merit proved to be the most profitable.
The presentations were most useful.

The most advantageous facet was the enhanced understanding of the DOE research progress and direction in the vehicle technologies area that the conference afforded.

The fact one could oversee the overall scope of effort, or lack thereof, proved to be the most useful.

Discussing research with colleagues was very beneficial.

Poster sessions and presentations proved to be the most valuable facets of the conference.

16.) What could have been done better?

21 Response(s)

There needs to be longer time for Q&A and more time for audience questions. Ask speakers to be available after the review for questions, cluster meeting rooms, and choose meeting rooms conducive to PowerPoint presentations.

There need to be better plenary session speakers.

Nothing could be done better.

Not much could be done better. It was pretty good.

The initial sessions were the ones participants wanted to review, but they overlap, so it is impossible to attend battery and fuel cell overview sessions, which are needed together for a balanced understanding. Perhaps some sessions can be moved to the afternoon. Make presentations available earlier online and allow questions to be sent online. Consider different venues at a lower cost (e.g., Oak Ridge National Laboratory). Consider Webex or online options. Make more technical details (papers) also available online.

The review of the industry presentations on Thursday (section V) was poor.

Since this is a technical review to assess and provide guidance for each specific project, it may be useful to only include projects that still have more than a year or so to go before completion. Therefore, a suggestion is to eliminate presentation of projects that are nearing completion.

In regards to the plenary session initial section talks, only invite people who are committed to providing a good talk. The screens need to be larger.

A suggestion is to ensure power outlets are in a location where the chords cannot be accidentally displaced.

Tough reviewers are needed.

Poster sessions did not seem to be part of the review; rather, they were more of an excellent networking event.

The plenary session was pitiful. The breaks ran out of food and coffee every time.

The reviewer questions could have been more in depth.

It will be much easier for the audience to see the slides if more screens were put in the conference room.

A couple of the track rooms were remote and hard to find, but I’m unsure what could have been be done about that given the size of the event.

The presentation screens could have been higher.

Everything was well coordinated.

It is recommended that there be some means for the TDMs to know if the reviewer actually attended the presentations, such as a check in on PeerNet. Perhaps a list of which reviewers are supposed to be in the session, with a “reviewer” ribbon on their badge, to make sure reviewers get to ask questions first.

In general, most session presenters adhered to their allotted time. However, in some sessions where presenters got done quickly and there were minimal questions, the session got ahead of time. That caused participants to miss some presentations, because the session chair did not wait until the allotted time to start.

There needs to be a coaching session for the presenters to help them better summarize their presentations and limit the number of slides to fit within the 20 minutes allotted.

It is recommended they minimize side meetings that distract from central purpose of the review.

17.) Overall, how satisfied are you with the review process?

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	2	37	18
0%	2%	3%	64%	31%

2 Comment(s)

The review is good, as it brings transparency to the DOE-funded projects, providing many with an opportunity to learn about DOE activities.

It is suggested to hold separate reviews for hydrogen and vehicles. From a TDM's perspective, there's no advantage (technical, logistic, or political) in holding a combined review. However, there is an immense logistical, cost, technical, and timing advantage to having them separate! It is very difficult to focus everyone's effort on the actual review. It is simply too large to be efficient and effective, although the organizers did a great job with it.

18.) Would you recommend this review process to others, and should it be applied to other DOE programs?

	Number of Responses	Response Ratio
Yes	54	38.0%
No	2	1.4%
No Response	86	60.5%
Total	142	100%

4 Comment(s)

It is very important, however, to consider cost reduction options such as a less expensive location, no meals, etc.

Every DOE program should have a similar review process!

Maybe, but it is expensive.

It is too expensive to attend unless you are obligated by a DOE contract.

19.) Information about the program(s)/project(s) under review was provided sufficiently prior to the review session.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	2	1	11	6
9%	9%	5%	50%	27%

9 Comment(s)

The assignments, arrangements, and presentations were all provided with sufficient time to review.

There was no confidentiality in the review process. Reviewers were fully identified to the presenters, and evaluations were done online with the audience in full view of our computer screens. Both practices are inappropriate.

It would have been preferred to have the information in advance to review, consider, and prepare appropriate questions and comments.

The organizers were highly proficient and gave full access to the presentations and descriptions well before the event.

One participant's assignments were not received until a week before the review.

It was difficult to get the password since one participant's secretary was not regarded as being authorized to claim it. Hence, this participant could claim it only on-site, which was too late to print the presentations for review. This might be a particular issue for reviewers from overseas.

The schedule for projects assigned to each reviewer should be sent out at least 30 days in advance (rather than about 14 days) so that travel can be scheduled accordingly.

It would have been helpful to receive the assignments earlier. The website was useful, however.

Initially, one reviewer was asked to review, but one week before the review was informed that his/her services were "no longer required." Then, at the review itself, he/she was again asked to review. Some other colleagues did not receive their reviewer assignments until the weekend before the review.

20.) Review instructions were provided in a timely manner.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	2	0	12	7
5%	9%	0%	55%	32%

4 Comment(s)

All were provided in time. This participant has done this for several years, so not much coaching was required. Overall though, good effort was made to provide reviewers instructional sessions (online and in person) at several occasions. Keep up the good work.

This was all done well. As a returning reviewer who more or less knew the drill, the instructions were clear.

The Webinar training was useful.

The DOE has a list of research targets for fuel cells, such as durability, cost, etc. It would be helpful to distribute this shortlist with the instructions. A search for them on the Internet was required to finally find them.

21.) The information provided in the presentations was adequate for a meaningful review of the projects.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	2	6	10	3
5%	9%	27%	45%	14%

10 Comment(s)

Some presentations were able to convey complex information a layman could understand, yet other presentations were so abstract that only someone with an extensive background in the field could follow the information. It is suggested that they are given clear instructions and that each presentation must stand on its own, tell a complete story, and be understandable by a congressional staffer.

The reviewers should have access to additional materials, because presented (perceived!) state-of-the-art information on the materials are being presented, as well as metrics describing the final goal(s) of the research and the Gantt chart. Only then can we appreciate whether the project is on track.

This is very presentation-specific. Most presentations were good and have gradually improved over previous years. Presenters are getting better at delivering the right amount of slides, as there was never too much or too little. In general, most presentations could still benefit from putting one-bullet takeaway messages on each slide. That simple but small effort goes a long way, especially when reviewing in absence of the presenter (either as preparation before the presentation or post-presentation).

The projects are becoming larger and more complex. Obviously, only a small fraction of results can be presented. There is no way out of this situation. The question of value for money also seems off the table, which is again a difficult thing to approach.

Some presentations were lacking in technical details and results due to intellectual property issues, which made it difficult to give a meaningful review.

Most presenters followed the required format, but not all. It is certainly easier to review a project that had all the information in the appropriate slides, since it is generally really difficult to review as one is hearing the presentation for the first time. This is especially true when little time is available to look at the materials before the meeting.

In most cases, the presentations provided enough material to review the projects. In one particular case, one company provided no data and very little information about their approach. The lack of information made it impossible to review their project.

The PIs need more time to do an effective job of communicating their work. Having 20 minutes to present a year's worth of work is too little.

Economics is a fundamentally important aspect of evaluation for feasibility of each technology being developed. Most programs reviewed did not do an adequate job of presenting current achievements, as well as targeting economics, including providing comparisons to current state-of-the-art technologies.

The poster's printed information was not sufficient. Some discussion with the presenter was needed.

22.) The evaluation criteria were adequately addressed in the presentations.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	1	1	3	12	4
	5%	5%	14%	57%	19%
Approach	0	1	4	13	3
	0%	5%	19%	62%	14%
Accomplishments & Progress	0	2	4	10	5
	0%	10%	19%	48%	24%
Collaboration & Coordination	0	0	6	12	3
	0%	0%	29%	57%	14%
Proposed Future Work	0	2	5	12	3
	0%	5%	24%	57%	14%

4 Comment(s)

Technical accomplishments and progress need to be stated more clearly and related directly to program goals. Accomplishments need to include a discussion of economics for the proposed technology. Proposed future research was universally obtuse. This also should be clarified with, perhaps, a bulleted list with connection to the goals.

Some presentations adequately addressed the evaluation criteria; others did not.

Some presentations were better than others, of course.

Some presentations were well organized and addressed the criteria adequately, while others were poorly written and some had sections completely missing, such as future plans.

23.) During the Annual Merit Review, reviewers had adequate access to the Principal Investigators.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	1	15	4
0%	5%	5%	71%	19%

4 Comment(s)

There were not enough places reserved for reviewers to sit. There should be two rows of seats for them and one row of seats for presenters.

Most everyone was available to speak with as needed. The poster sessions were better than in years past. The increased space improved accessibility to poster presenters and the availability of food in the same room relaxed the environment substantially. Previously, Crystal City poster sessions were “brutally” claustrophobic.

There was not enough time, and I’m not sure there will ever be! I advocate having two, meetings a year. The AMR jams too much into one meeting, and there’s a lot of information and people. It is preferable to have smaller meetings separating electrochemical storage devices and two or three smaller meetings a year where the presentations can be staggered. For example, include half of the PIs in the first meeting and the other half in the second meeting.

One participant reviewed two poster sessions for a total of four hours of presentations and was assigned 38 presentations to review. This amounted to reviewing about 10 presentations per hour, or having six minutes per presentation review. It was not possible to cover all of the reviews, especially considering there were more in the second evening than the first. It would have been helpful to have some of the food served, particularly to reviewers and presenters, before the sessions, as it was quite tiring to do the work without food until 8:30 p.m. or 9 p.m..

24.) Information on the location and timing of the projects was adequate and easy to find.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	1	13	8
0%	0%	5%	59%	36%

3 Comment(s)

It was a well-organized review.

It wasn’t clear from the materials which reviews were presentations and which were poster sessions. It was necessary to ask the review helpers, and they had to look into it for a bit before they figured it out. Maybe a different number series would help to differentiate between the two.

It was organized, except for the obvious need to move the Fuel Cells session. Some planning should be put into room size in the future.

25.) The number of projects I was expected to review was:

	Number of Responses	Response Ratio
Too many	5	3.5%
Too few	1	<1%
About right	16	11.2%
No Responses	120	84.5%
Total	142	100%

5 Comment(s)

Twelve projects were adequate. It allowed time to review other sessions and dedicate appropriate time and effort to the write-up of each without being overly daunting.

Twenty eight reviews are a bit much. There is no thought given to the overlapping assignments given to reviewers with multiple areas of expertise.

One participant could have reviewed projects on Thursday since he/she was there all week.

One participant selected the ones he/she could handle given time constraints and interests.

There were too many reviews for the time allowed.

26.) Altogether, the preparatory materials, presentations, and the question and answer period provided sufficient depth for a meaningful review.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	1	16	5
0%	0%	5%	73%	23%

4 Comment(s)

From a reviewer standpoint, the relevant and critical information was missing. There is a need to depart from the presentation format for the reviewers to provide the DOE with better insight as to the value of the project.

Many presenters went “overboard” on the presentation, which truncated Q&A sessions. But generally, they went well.

In general, yes. However, some projects did not have adequate information on the technical approaches due to intellectual property issues. Any government-funded programs or work done at the national labs should not be restricted for general disclosure.

This answer does not mean it cannot and should not improve. A lot of PIs seemed to spend their time on the materials and concepts of the past rather than sharpening their focus on the future.

27.) The request to provide a presentation for the Annual Merit Review was provided sufficiently prior to the deadline for submission.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	4	27	18
0%	2%	8%	54%	36%

7 Comment(s)

There needs to be more advanced warning of the AMR presentation deadline.

There was a problem receiving the original e-mail but I was able to get it in sufficient time. One reviewer's company's spam filter rejected it.

The request to provide a presentation could have been sent earlier. Three weeks was not sufficient.

The deadline should be strictly enforced. Not meeting the deadline should disqualify PIs from presenting at the AMR.

Communication for the event and the power point templates worked just fine.

Our issue was internal to our company. All directors, principal investigators, and program managers who are listed on documentation should receive announcements.

The company representatives were not aware of the annual meeting, as this was their first DOE activity. It seemed like a short window, especially for an inexperienced project team.

28.) Instructions for preparing the presentation were sufficient.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	2	22	26
0%	0%	4%	44%	52%

6 Comment(s)

The posters were very available, with some containing far more material than requested. Either give all more latitude or enforce your guidelines.

There are too many slides and a lot of redundant information required. It would be better to have required information, rather than a strict requirement on slide format.

The instructions were very clear, and the example templates were nice and provided several format options.

Every aspect was clearly communicated. After attending the first one, the next one should be better as the expectations are now known.

It is recommended that all notices specifically state that the approved format be used and include a hyperlink to the format. This participant's contract administrator did not know about the required information.

Rich Bechtold was very helpful with suggestions.

29.) The audio and visual equipment worked properly and were adequate.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	3	23	23
0%	0%	6%	47%	47%

4 Comment(s)

Many presenters had difficulty with the projection equipment or microphones.

This is not applicable for posters. For oral presentations, there were some glitches.

At times, one reviewer couldn't hear the speakers. Perhaps a collar mike could have been provided.

There were no issues during one participant's presentation, but there was an interruption in power which disabled the microphones during one of the sessions. Also, a projector stopped working for a presenter during a session.

30.) The evaluation criteria upon which the Review was organized were clearly defined and used appropriately.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	1	2	3	26	15
	2%	4%	6%	55%	32%
Approach	1	1	2	27	16
	2%	2%	4%	57%	34%
Accomplishments & Progress	1	1	2	28	15
	2%	2%	4%	60%	32%
Collaboration & Coordination	1	4	3	27	12
	2%	9%	6%	57%	26%
Proposed Future Work	1	3	4	25	14
	2%	6%	9%	53%	30%

8 Comment(s)

One presenter's project was a little unique, since they are a proving ground for a fuel cell already designed and being manufactured.

The organization of the presentation made sense. It was the actual review results that were unclear.

There were times when the given evaluation criteria were not sufficient. Perhaps a chart numbered 1-10 for each functional area would be more beneficial.

The technical accomplishments and progress can only be judged appropriately if reviewers have access to the additional information requested above.

Relevance is confusing. Is it relevant to the technology or relevant to the partnership goals? It's supposed to be the partnership goals, but reviewers and presenters have both misinterpreted this. Many of us don't support the plan of cutting H_2 funding, so how can premature termination of a project be marked as being relevant? All the projects are supposed to represent the president's goals. So, what is the point of asking us about relevance if there are no partnership goals being met?

All projects are relevant because that metric is necessary to get funded. They are relevant by design. The transfer and collaboration is very broad and varied. Much of it seems like service work. The transfer often is not meaningful because the work is commercialization, and the intellectual property is not revealed. The future research is also reasonably apparent, except in the rare case where the project is ending. Certainly in a three-year project, the future research is simply the tasks assigned through the next year.

The question about relevance is odd. How could the projects have received funding unless they were relevant? This question should be eliminated from future reviews, as it seems irrelevant.

Relevance at the topical level was right on point. But the PIs didn't quite follow or sometimes even understand the topic. In one instance the PI confused high energy with high capacity on the cathode material and found materials that increased capacity by 20% but dropped the average voltage to 75% with almost no net change in energy!

31.) Explanation of the questions within the criteria was clear and sufficient.

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	1	1	3	26	12
	2%	2%	7%	60%	28%
Approach	1	1	4	25	12
	2%	2%	9%	58%	28%
Accomplishments & Progress	1	1	3	26	12
	2%	2%	7%	60%	28%
Collaboration & Coordination	0	2	4	26	10
	0%	5%	10%	62%	24%
Proposed Future Work	1	2	3	25	12
	2%	5%	7%	58%	28%

0 Comment(s)

32.) The right criteria and weightings were used to evaluate the project(s)/program(s).

Top number is the count of respondents selecting the option. Bottom % is percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	1	2	2	25	11
	2%	5%	5%	61%	27%
Approach	1	1	3	24	12
	2%	2%	7%	59%	29%
Accomplishments & Progress	1	1	2	25	12
	2%	2%	5%	61%	29%
Collaboration & Coordination	1	2	3	24	11
	2%	5%	7%	59%	27%
Proposed Future Work	1	1	2	27	10
	2%	2%	5%	66%	24%

4 Comment(s)

Research on enabling technologies, like improving materials properties and performance for specific component parts, cannot always be translated into exact efficiency gains. But such work remains an essential component to enabling progress on larger systems. Those formulating review criteria should understand this role more clearly.

By default, the DOE would not fund projects that are not relevant to the DOE's mission, so relevance should not be a review criterion. Presumably, the approach has also been reviewed during the proposal review process. It should either not have a high weighting or should be eliminated completely as a review topic but included in the presentations as background material.

How can one see the review results from projects?

As stated earlier, relevance is a given. The DOE only funds relevant projects. The approach is also a foregone conclusion. The technology transfer and collaboration patterns vary from project to project. It seems obvious that good collaboration within a large organization can be as useful as good collaboration between two organizations. Basically the question is teaming, getting the right talents on board for success. The future research in most multi-year projects is to do the next year's tasks.

33.) Please provide additional comments:

38 Response(s)

As a PI, this was his/her first time at the AMR. All in all, this is a very well-organized event. The venue, the agenda, and presentation format were all communicated to the presenters well ahead of schedule. Very well done!

The venue was AWFUL. It was outrageously expensive and the quality of service was very poor. The Internet was poor (at $15 a day!!) and the rooms were noisy (toilet and shower use of neighbors was very clear).

It is essential in future reviews to have, say, a 30-minute poster session with just the reviewers and those being reviewed, then open up to all.

The inflexible presentation format asks for similar information on more than one slide. For example, there is no need for a summary slide since it repeats just-presented information. Allow more flexibility as long as the required items are covered. The early deadline (60 days!) excludes key progress. That especially hurts new projects. Can it be shortened to 30 days?

This process is fine as is.

The presentations are too short (approximately 20 minutes), and the reviewers do not obtain all the information from such a short time. One participant attended presentations from three topics, and it was very common to catch reviewers that were not aware about the work done previously in the project.

The presentations are requested too far ahead. More recent and important data obtained after the presentation is sent in is left out. The review meeting is too large, and this handicaps its scientific value. Discussions with reviewers are limited and diluted by the presence of outsiders.

Very few posters and presentations included references from other work. There were several presentations with claims of novel work that were simply not true. This was not a single instance, but was observed repeatedly throughout the meeting. This must be corrected as other researchers were not acknowledged. It was especially obvious for HSECoE (Hydrogen Storage Engineering Center of Excellence) presentations.

It was disappointing that the hotel did not have free wireless Internet service. This would have helped one reviewer to provide input into the system during the week. Also, the cell phone service (for Sprint phones) was poor in the hotel. These small items can make a significant impact on the productivity of and convenience for those attending.

The room for balcony IX was very far away and hard to find.

The space allocated for breaks was crowded, and because of the crowds, it was difficult to locate participants for meetings. Also, supplies of food and/or coffee often ran out.

Please make sure the AMR is at the same location next year. The venue is far better than in previous years.

This is a very informative and useful event to see how industry peers are doing and also see that there is vision on this project outside of our company.

It was very well organized. Thank you for the meals as well!

In a “world of hype and outright deception,” the AMR stands in stark contrast. The peer review function assures honest data and serious evaluation. Overseas colleagues commented with admiration on the entire meeting. Congratulations for creating such a valuable venue.

It was excellent and very informative.

This is a very well-organized event, and thank you very much for holding it and allowing attendees not directly involved with the process to participate.

The presentation room was too hot and not air conditioned. There were problems with the sound system sometimes. The reviews of the industry on Thursday were poor.

Some of the research presented was not “grounded in the real world.” I suggest having some real-world, non-technical people as reviewers.

Argonne National Laboratory (ANL) seemed to have a disproportionate share of the presentations.

The process works for the presenters and reviewers, but it does not work very well for the audience.

The overall process is okay.

The service at lunch on Tuesday was excellent. The hotel employees were numerous and engaged.

I suggest that all assistance (i.e., providing meeting space and logistic support) provided for side meetings should be cancelled, and the practice of scheduling them in conjunction with AMR should be discouraged. The actual review becomes a secondary priority to the side meetings.

Please try to get the plenary session presenters more focused. These “high-level” staff members at the DOE and other agencies really do not have an understanding of the programs being reviewed. Some of their commentary was not useful at all.

It was appreciated that the diligence of the session chairs kept the program moving along and on time. There was a lot of information in a short period of time, but it was worth the trip.

There were too many parallel sessions.

Overall, the review process was very smooth this year.

Too much time is wasted in presenting the standard material that is required. More time should be spent by the presenters on the research and substance of the project with more technical details.

It’s a good process.

AMR improves every year. It is incredibly well organized, and the location was outstanding and far more relaxing than the Crystal City location. Please continue at this facility despite presumably higher costs.

This meeting has grown huge. It is hard to see why the two programs are combined into the same time and place. There should be some thought given to making it possible for one person to attend both conferences, and that means a different time and maybe a different place.

The splitting of the fuel cell session into two parallel sessions on Friday morning was not good. This reviewer was the only representative from one particular company (an auto OEM), and would have liked to have seen the projects in both sessions.

It would be very helpful to the reviewers to have three to five minutes at the end of each presentation Q&A session to complete our reviews before the next presentation.
This is a great event which helps to raise the quality level of the research activities.

We need to elevate the energy level in the cell to substantially higher values to make EVs and even PHEVs a market reality. There are PIs who are still working on NCA, LFP, etc., which are commercial materials today and are best left up to the industry to perfect.

Of the six projects reviewed by this particular reviewer, two should have never been funded, but given that they were, the funding should be terminated since there is no logical reason to believe they will be successful. One was too expensive. It has been funded for the last three years and it's a wonder why attention was not directed to the economics of it earlier.

It would be helpful to follow more rigorous milestones and go/no-go decision points.