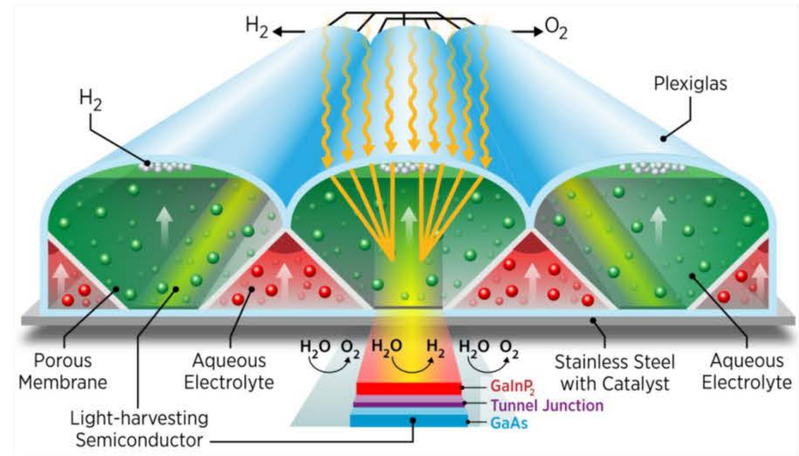
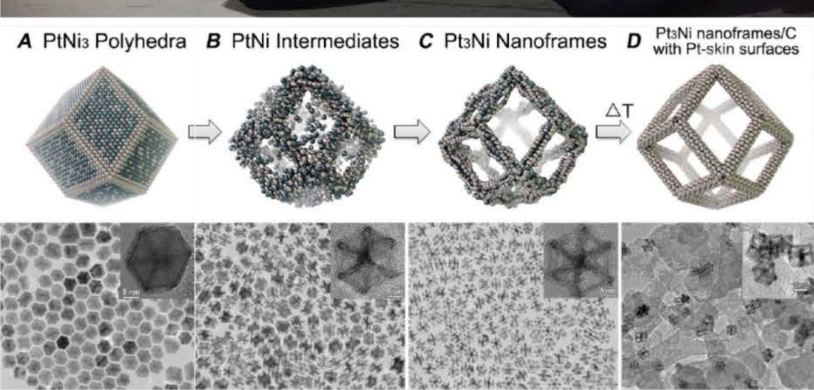


H2FIRST



U.S. DEPARTMENT OF
ENERGY

Hydrogen and Fuel Cells Program

2014 Annual Merit Review and Peer Evaluation Report

June 16–20, 2014
Washington, DC

DOE/GO-102014-4503 October 2014

About the Cover

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

Innovative reactor design for photoelectrochemical hydrogen production. *Illustration by Alfred Hicks, National Renewable Energy Laboratory (NREL).*

In-line QC development for fuel cell electrodes on NREL's research web-line. *Photo courtesy of Michael Ulsh and Guido Bender, NREL.*

"Hex-Cell" is a passive heat exchanger loaded with sorbent powder for hydrogen storage and flow-through hydrogen gas for cooling. *Photo courtesy of Savannah River National Laboratory*.

Launched by the U.S. Department of Energy (DOE), the Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) project is a new effort being run by NREL and Sandia National Laboratories to support H_2USA, a public-private partnership co-launched by industry and DOE, and will work to address hydrogen infrastructure component R&D. *Image courtesy of DOE.*

FedEx, Plug Power, and partners are designing, building, and deploying 15 baggage tow tractors to be operated at the airport in Memphis, Tennessee. *Photo courtesy of FedEx.*

A new catalyst synthesized in 2014 by research groups led by Vojislav Stamenkovic (Argonne National Laboratory) and Peidong Yang (Lawrence Berkeley National Laboratory), which consists of a platinum-nickel alloy nanoframe covered by a thin platinum skin, has a performance more than 30 times higher than that of conventional platinum on carbon catalysts. *Photo courtesy of Vojislav Stamenkovic, Argonne National Laboratory, also printed in* Science *(343: 6177), 2014; pp. 1339–1343.*

Photo on right:

U.S. Capitol Building. *Photo courtesy of www.istock photo.com.*

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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 fiscal year (FY) 2014 U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program Annual Merit Review and Peer Evaluation Meeting (AMR), held in conjunction with DOE's Vehicle Technologies Office Annual Merit Review on June 16–20, 2014, in Washington, DC. In response to direction from various stakeholders, including the National Academies, this review process provides evaluations of the Hydrogen and Fuel Cells Program's projects in applied research, development, demonstration, and analysis of hydrogen and fuel cells. A joint plenary session opened the meeting with a keynote address from Alan Taub, Professor of Material Science and Engineering at the University of Michigan, followed by overview presentations from the Hydrogen and Fuel Cells Program, the Vehicle Technologies Office, and the Basic Energy Sciences Program. A plenary for Hydrogen and Fuel Cells Program participants included overviews on each of the eight sub-program areas: Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Technology Validation; Safety, Codes and Standards; Market Transformation; and Systems Analysis.

DOE values the transparent, public process of soliciting technical input on projects from relevant experts. The recommendations of the reviewers are taken into consideration by DOE technology managers in generating future work plans. The table that follows lists the projects presented at the review, evaluation scores, and the major actions to be taken during the upcoming fiscal year (October 1, 2014–September 30, 2015). The projects have been grouped according to sub-program area and reviewed according to the appropriate evaluation criteria. The weighted scores for all of the projects are based on a four-point scale, with half-point intervals. To furnish principal investigators (PIs) with direct feedback, all of the evaluations and comments are provided to each presenter; however, the authors of the individual comments remain anonymous. The PIs are instructed by DOE to fully consider these summary evaluation comments, along with any other comments by DOE managers, in their FY 2015 plans. In addition, DOE managers contact each PI individually and discuss the comments and recommendations as future plans are developed.

In addition to thanking all participants of the AMR, I would like to express my sincere appreciation to the reviewers for your strong commitment, expertise, and interest in advancing hydrogen and fuel cell technologies. 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 2015 AMR, which is presently scheduled for June 8–12 in Arlington, Virginia. Thank you for participating in the FY 2014 AMR.

Sincerely,

Sunita Satyapal
Director
Hydrogen and Fuel Cells Program
U.S. Department of Energy

Hydrogen Production and Delivery

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-014	Hydrogen Delivery Infrastructure Analysis *Amgad Elgowainy; Argonne National Laboratory*	3.3	X			Reviewers commended the analytical work and collaboration with industry and suggested extending the collaboration to other federal agencies. Recommendations included improving the analysis to address the cost of the pump at stations and the high-pressure tube trailer valves. Reviewers also suggested including multiple pathway analysis to provide ideas for new/modified pathways to reach U.S. Department of Energy (DOE) cost targets.
PD-022	Fiber-Reinforced Composite Pipelines *George Rawls; Savannah River National Laboratory*	3.6	X			Reviewers were pleased with the performance and progress of the project, noting that fiber-reinforced-polymer pipelines demonstrate great potential for long-term development of hydrogen fuel technology. Reviewers suggested that additional fluid dynamic analyses for each proposed joint concept are needed to study the choked flow through reduced internal diameters, and that testing should include pressurized hydrogen.
PD-025	Hydrogen Embrittlement of Structural Steels *Brian Somerday; Sandia National Laboratories*	3.4	X			Reviewers complimented the project team's ability to address DOE technical barriers and maintain a continued understanding of the problem and scientific challenges. Reviewer suggestions included conducting analyses of installation costs and detailed cost savings. They also recommended conducting more testing on fatigue crack growth measurements in steel pipe exposed to hydrogen from other sources.
PD-028	Solarthermal Redox-Based Water Splitting Cycles *Al Weimer; University of Colorado*	3.0	X			Reviewers applauded the innovative, high-quality efforts and progress made with this project as well as the effective use of collaboration. They expressed concern about the technical challenges of moving solid materials at high temperatures and low pressures and recommended industrial or other expert input for the reactor system design and modeling. Reviewers also recommended a stronger focus on the materials development aspects of the project and continued updating of the Hydrogen Analysis (H2A) model, with particular attention on capital and operations and maintenance (O&M) costs.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-035	Semiconductor Materials for Photoelectrolysis *Todd Deutsch*; *National Renewable Energy Laboratory*	3.5	X			Reviewers commended the project for its depth of understanding of the III/V semiconductor materials class, which offers one of the most promising pathways to achieving DOE goals for cost-effective photoelectrochemical (PEC) solar hydrogen production. The project's extensive collaborations with the broader PEC Working Group were viewed as a particular strength. Some concerns were expressed related to the project team's limited access to dedicated III/V semiconductor fabrication equipment, and related to the limited scope of materials characterizations—particularly related to durability investigations. Reviewers recommended expanding the project scope and bringing in new cross-office and cross-institute research and development (R&D) partnerships to leverage the relevant materials innovations.
PD-037	Biological Systems for Hydrogen Photoproduction *Maria Ghirardi; National Renewable Energy Laboratory*	2.9		X		Reviewers noted that the project is focused on addressing DOE barriers related to oxygen accumulation and hydrogen production rates. They stated that the project uses a logical approach. They noted the changes in scope and delays to the project, but they approved of plans to combine multiple mutations into a single strain as a logical completion and laudable goal. This project will be discontinued for programmatic reasons in early fiscal year (FY) 2015.
PD-038	Fermentation and Electrohydrogenic Approaches to Hydrogen Production *Pin-Ching Maness; National Renewable Energy Laboratory*	3.3	X			Reviewers recognized the progress the project has made in improving hydrogen production from the fermentation of cellulose and commended the project for its strong collaborations in the areas of feedstock sources, microbial electrolysis cell work, and genetic engineering. They noted the lack of technoeconomic analysis, and would have appreciated more information about Task 4, which involved developing a case study. Reviewers suggested adding analysis of the area of metabolic flux; potential uses for other components such as C5 sugars, lignin, and proteins; and chemical engineering.
PD-048	Electrochemical Hydrogen Compressor *Ludwig Lipp; FuelCell Energy, Inc.*	3.4			X	According to reviewers, the project has an excellent approach and has made good, steady progress. Reviewers found the project to be highly relevant to DOE goals because of its potential for achieving better operating cost and reliability than mechanical compressors. Reviewers suggested that the project assess the economics of high-volume manufacturing, and the potential for compressor variability in high-volume production. Additionally, while reviewers commended the partnership between FuelCell Energy and Sustainable Innovations, they recommended that the project add partners to help with research initiatives such as optimizing the compressor membrane. This project will be completed in FY 2014.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-058	Characterization and Optimization of Photoelectrode Surfaces for Solar-to-Chemical Fuel Conversion *Tadashi Ogitsu; Lawrence Livermore National Laboratory/National Renewable Energy Laboratory*	3.5	X			Reviewers commented that the PEC theoretical tools and expertise developed through this project are outstanding and extremely valuable to broader PEC R&D efforts. The coordination of theoretical model development with experimental validation work based on spectroscopic results was highly commended. There was some concern that the project scope covered too many topics, given the project budget. It was recommended that the project team establish broader ties with the semiconductor and catalysts R&D communities to leverage synergistic theoretical and computational resources.
PD-081	Solar Hydrogen Production with a Metal-Oxide-Based Thermochemical Cycle *Tony McDaniel; Sandia National Laboratories*	3.1	X			Reviewers remarked on the outstanding approach to materials discovery and characterization, innovative reactor concept development, and systems analysis, as well as the excellent credentials and facilities of the project team. They expressed concern about the complexity of the reactor design and the high efficiencies and large decrease in capital cost required to meet the cost targets. Reviewers recommended a stronger emphasis on materials R&D, including screening methods prior to synthesis, characterization of materials durability during thermal cycling, and continued technoeconomic analysis.
PD-088	Vessel Design and Fabrication Technology for Stationary High-Pressure Hydrogen Storage *Zhili Feng; Oak Ridge National Laboratory*	3.0	X			Reviewers complimented this project’s technical approach and progress to date in proving technical viability. The concept of vent ports to allow for hydrogen to diffuse out of the steel was particularly well received. The project was criticized for not considering the viability of vessels’ installation at forecourt stations with respect to their size, the potential for diffusion paths being plagued by on-site moisture, and installation costs. Reviewers recommended conducting a cost comparison of the vessels with respect to existing fiber-wound storage technologies.
PD-094	Economical Production of Hydrogen through Development of Novel, High-Efficiency Electrocatalysts for Alkaline Membrane Electrolysis *Katherine Ayers; Proton OnSite*	3.1	X			According to reviewers, this project takes a novel approach to reducing the capital costs of electrolysis by developing alkaline membrane technology with the potential to move electrolyzers to a new, lower-cost curve. Despite the reduction in capital costs, reviewers indicated that this is a small percentage of the levelized cost of hydrogen. It was noted that degradation and stability need to be better characterized. Some other reviewer recommendations included focusing on improving cell efficiency, optimizing the catalyst chemistry, and performing an H2A model analysis to evaluate the ability of this work to reduce the cost of hydrogen.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-095	Improving Cyanobacterial O_2-Tolerance Using CBS Hydrogenase for Hydrogen Production *Pin-Ching Maness; National Renewable Energy Laboratory*	3.3	X			According to reviewers, the project is well organized and focused on addressing oxygen inhibition in synechocystis, though it was noted that the work is only part of what would be required to meet the ultimate DOE goals. Reviewers noted the strong collaborations and the progress made, but also the lack of hydrogen production data. They felt that the proposed future work is logical and has the potential to meet the project goals. They recommended better definition of the pathway and the potential for hydrogen production, as well as better understanding of protein function and activity.
PD-096	Electrolyzer Component Development for the Hybrid Sulfur Thermochemical Cycle *William Summers; Savannah River National Laboratory*	2.9	X			Reviewers recognized the electrolyzer performance as the critical barrier to the hybrid sulfur cycle and commended the progress made in electrocatalyst screening, electrolyte membrane development, and the design and fabrication of a pressurized button cell for higher-temperature and pressure testing. They were concerned about the emphasis on polybenzimidazole membranes, noting that they are known to have stability issues. It was recommended that this issue be addressed and that other membrane and catalyst candidates be investigated.
PD-098	Low-Noble-Metal-Content Catalysts/Electrodes for Hydrogen Production by Water Electrolysis *Katherine Ayers; Proton OnSite*	3.1	X			Reviewers were generally satisfied with the progress made in reducing platinum group metal (PGM) loading of electrolyzer electrodes through leveraging core-shell catalyst technology developed at Brookhaven National Laboratory. However, several reviewers noted that PGM loading is only a small percentage of the system cost and is therefore unlikely to have a large impact on the cost of hydrogen. Some reviewers felt that the future work was not well defined and that project tasks were not integrated well. It was recommended that the team consider performing the H2A cost analysis earlier in the project to assess the potential impact of the work, rather than waiting until the end.
PD-100	700 bar Hydrogen Dispenser Hose Reliability Improvement *Kevin Harrison; National Renewable Energy Laboratory*	3.5	X			This project was well received by reviewers because of its relevance to enabling low-cost hydrogen delivery and its technical approach. In particular, reviewers appreciated the use of a robot to simulate fueling. Reviewers recommended that future research include exposure to realistic service conditions such as sunlight, environmental contaminants, and hysteresis. They also recommended that the project collaborate with station owners in California, Yokohoma Rubber in Japan, and/or other domestic hose manufacturers. Reviewers felt that such collaborations would ensure that the project accounts for fueling abnormalities that occur in service, such as breakaway events.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-102	Hydrogen Pathways Analysis for Polymer Electrolyte Membrane Electrolysis *Brian James; Strategic Analysis, Inc.*	3.1	X			Reviewers appreciated the project team's inclusion of two national laboratories and commended the involvement of four electrolyzer companies in developing polymer electrolyte membrane (PEM) case studies. The results of the studies were seen as useful, especially the capital cost breakdown and sensitivity analysis. The reviewers commented that the correlation between the project results and relevant DOE targets should be made clearer. Recommendations included extending the PEM case studies to include alternative electrolyzer operating conditions (e.g., in current density), and establishing quantifiable limits to electrolytic hydrogen production achievable through capital and operating cost improvements.
PD-103	High-Performance, Long-Lifetime Catalysts for Proton Exchange Membrane Electrolysis *Hui Xu; Giner, Inc.*	3.3	X			Reviewers were pleased with the progress made toward developing lower-PGM-loading, high-performing electrocatalysts for PEM water electrolysis. They also commented on the strength of the team and the very good collaboration between the team members. Reviewers recommended placing more emphasis on longer-term durability testing. They also suggested considering possible down-selection among the different catalysts being developed.

Hydrogen Storage

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-001	System-Level Analysis of Hydrogen Storage Options *Rajesh Ahluwalia; Argonne National Laboratory*	3.4	X			Reviewers noted that the project is an important tool for the Hydrogen Storage sub-program and provides useful understanding of the impact of new technologies. The project was commended for its good overall accomplishments; in particular, for defining the sorbent storage property requirements. Reviewers commented that the resin additive study results need to be validated because other researchers showed different results. Reviewers also recommended that the project be careful in generalizing suitable high-density polyethylene (HDPE) operating temperatures because different grades of HDPE can operate at lower temperatures.
ST-004	Hydrogen Storage Engineering Center of Excellence *Don Anton; Savannah River National Laboratory*	3.5	X			Reviewers commended SRNL for its overall management of the Hydrogen Storage Engineering Center of Excellence (HSECoE) because of its effectiveness in focusing the coordination and collaboration between Center partners on the objectives. The use of spider charts to show performance against targets, the use of detailed milestone charts to track progress, and carrying out a lessons learned activity were specifically cited as practices future collaborative efforts should adopt. It was recommended that greater emphasis be directed toward the more challenging targets for sorbent systems (e.g., loss of usable hydrogen).
ST-005	Systems Engineering of Chemical Hydrogen, Pressure Vessel, and Balance of Plant for Onboard Hydrogen Storage *Kriston Brooks; Pacific Northwest National Laboratory*	3.2	X			The project, as part of the HSECoE, was commended for its extensive collaboration with other Center partners. Reviewers commented on the high relevance of the project activities. With completion of the work on chemical hydrogen (CH) storage systems within the HSECoE, reviewers recommended completion and dissemination of the system models and other project results. They also emphasized that the cost analyses for the chemical and sorbent systems should be completed.
ST-006	Advancement of Systems Designs and Key Engineering Technologies for Materials-Based Hydrogen Storage *Bart van Hassel; United Technologies Research Center*	3.2	X			This project is part of the HSECoE. The reviewers commended United Technologies Research Center (UTRC) for its weight/volume reduction and integration of the gas liquid separator and ammonia filters into the CH storage system, allowing the CH system to meet the U.S. Department of Energy's (DOE's) 2017 target for volumetric capacity. In addition, reviewers acknowledged the importance of UTRC's role in developing graphical user interfaces for the publicly available Simulink models, which will allow material researchers to understand the effect of material properties on system-level performance. There was some concern related to the potential premature development of auxiliary systems for material that may not be commercialized.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-007	Chemical Hydrogen Rate Modeling, Validation, and System Demonstration *Troy Semelsberger; Los Alamos National Laboratory*	3.2			X	The work by LANL on chemical hydrogen storage systems, as part of the HSECoE, was considered to be highly relevant to the sub-program, even though the system was not selected for continuation as an HSECoE Phase III activity. The determination of chemical hydrogen storage material property requirements for a system to meet the DOE performance targets was noted as being highly valuable. Reviewers recommended publishing the material requirements and system models in peer-reviewed journals. This effort is being wrapped up as a result of chemical hydrogen storage system activities not being continued in Phase III of the HSECoE.
ST-008	System Design, Analysis, and Modeling for Hydrogen Storage Systems *Matthew Thornton; National Renewable Energy Laboratory*	3.2	X			Reviewers commended this NREL project, as part of the HSECoE, on the integrated framework model that couples vehicle, fuel cell, and hydrogen storage system models for system/materials performance evaluation. Reviewers commented that these models should be extremely useful to the research community in the future, and that making them available to the public on the Internet should be a high priority.
ST-010	Ford/BASF-SE/UM Activities in Support of the Hydrogen Storage Engineering Center of Excellence *Mike Veenstra; Ford Motor Company*	3.3	X			This project is part of the HSECoE. Overall, reviewers were impressed with the project's accomplishments, ranging from its role as sorbent system architect to metal-organic-framework-5 (MOF-5) scale-up, failure mode and effects analysis coordination, and performance/cost modeling. Reviewers applauded the principal investigator (PI) and his overall leadership, noting that this project "seems to be a nerve center for the entire HSECoE." Reviewers were encouraged by the identification of several new, promising MOF materials, but there were still concerns regarding the inability of MOF-5 to meet DOE's volumetric capacity targets. As a result, the reviewers said that the project provides a very valuable original equipment manufacturer perspective on the practicality of adsorbents as onboard hydrogen storage materials.
ST-019	Multiply Surface-Functionalized Nanoporous Carbon for Vehicular Hydrogen Storage *Peter Pfeifer; University of Missouri*	2.5			X	The reviewers applauded the efforts the University of Missouri has taken to correct or remove previous results that were deemed unsubstantiated, and they suggested further collaboration with the validation group at NREL to ensure that future results are valid. The reviewers questioned the progress to date, as well as the results related to the reported amount of sp^2 bonded boron in the carbon lattice, which is the inherent key to the proposed approach. When these results are combined with the overall uncertainty in the hydrogen uptake measurements, most reviewers concluded that this project should end as scheduled in November 2014.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-044	Savannah River National Laboratory Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Adsorbent Storage *Bruce Hardy; Savannah River National Laboratory*	3.3	X			The technical efforts on modeling and evaluating hydrogen sorbent systems by SRNL, as a partner in the HSECoE, were considered critical for the overall success of the HSECoE. Reviewers commented that the approach and activities were well planned and highly relevant to the sub-program. Reviewers suggested that the project team should produce recommendations on the materials' requirements needed to meet DOE system targets, as well as look at system balance-of-plant (BOP) components. It was also recommended that the project team put more emphasis on improving performance against the remaining challenging targets, such as loss of usable hydrogen.
ST-046	Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage *Kevin Drost; Oregon State University*	3.0	X			This project is part of the HSECoE. Reviewers were generally pleased with the progress of the project and acknowledged the promise of the novel modular adsorbent tank insert (MATI) design to meet the unique challenge of optimizing the amount of hydrogen stored in a given volume while also accounting for challenging heat transfer requirements compounded by adsorbent media densification. There were concerns noted regarding whether the MATI was sufficiently robust to operate reliably for the necessary lifetime under the variable pressure and temperature conditions present in an adsorbent system.
ST-047	Development of Improved Composite Pressure Vessels for Hydrogen Storage *Norman Newhouse; Hexagon Lincoln*	3.3	X			This project is part of the HSECoE. In addition to the importance of having a tank manufacturer on the HSECoE team, the reviewers acknowledged several positive contributions from Hexagon Lincoln (HL), including the development of flexible tanks for system testing, the development of lower-cost/lighter-weight tanks, and the demonstration of improved vessel subsystem capabilities (i.e., operation at cryogenic temperatures and isolation bottle approach). The main weaknesses identified relate to the lack of correlation between HL's results and the DOE targets, and the perception that HL's proposed Phase III work will not benefit the ultimate outcome of the HSECoE Phase III effort.
ST-063	Reversible Formation of Alane *Ragaiy Zidan; Savannah River National Laboratory*	3.1	X			Overall, the reviewers commented favorably on the progress this SRNL project has made in the past year. They especially commended the project on focusing on process cost reduction and addressing key technical barriers that have been identified. It was recommended that the team remain focused on addressing reaction kinetics and understanding the cause of the required overpotential. The addition of Ardica as a potential commercialization partner was considered positive.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-093	Melt-Processable PAN Precursor for High-Strength, Low-Cost Carbon Fibers *Felix Paulauskas; Oak Ridge National Laboratory*	3.1	X			Reviewers commended the project’s move toward using polyacrylonitrile with methyl acrylate as the precursor formulation material because it allows the possibility of meeting the required carbon fiber (CF) mechanical properties. Reviewers suggested that the project obtain industrial confirmation of manufacturing and feasibility assumptions as it develops a cost model for the project. Reviewers also suggested that in future presentations, the PI include as much information as possible to allow a better understanding of how technical issues were addressed without invoking intellectual property issues.
ST-099	Development of Low-Cost, High-Strength Commercial Textile Precursor (PAN-MA) *Dave Warren; Oak Ridge National Laboratory*	3.2			X	Reviewers commented that the project has a good overall approach to decrease CF cost, and that there is good cooperation between FISIPE, the precursor manufacturer, and ORNL. Reviewers noted that there is a need to clarify the types of final CF testing to be conducted by a tank manufacturer. Reviewers also stressed the importance of completing a cost analysis that accounts for yield loss and product quality. This project has been completed.
ST-100	Ongoing Analysis of Hydrogen Storage System Costs *Brian James; Strategic Analysis, Inc.*	3.1	X			The project was praised for focusing on analyzing the cost of BOP—the highest cost component of the compressed storage system at low manufacturing volumes. Reviewers also commended the approach of using compressed natural gas BOP cost analysis to validate the cost model for compressed hydrogen storage at low production volumes, as well as the good efforts in gaining data from and vetting results with BOP component manufacturers. Reviewers recommended that the project investigate the cost impacts of higher inspection demands and safety verification.
ST-101	Enhanced Materials and Design Parameters for Reducing the Cost of Hydrogen Storage Tanks *Kevin Simmons; Pacific Northwest National Laboratory*	3.1	X			Reviewers commented that the project has done a great job of validating models with empirical studies in composite properties, as well as demonstrating properties of matrix modifications, nanofillers, and catalysts for curing. Reviewers also commended the project’s advances in addressing gas dormancy of cold gas versus other cryogenic storage approaches. However, reviewers commented that the robustness of the resin, liner, and fibers being considered is either unknown, or highly uncertain, at cold temperatures and should be sufficiently evaluated.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-103	Hydrogen Storage in Metal-Organic Frameworks *Jeffrey Long; Lawrence Berkeley National Laboratory*	3.2	X			The reviewers acknowledged the methodical approach of the highly qualified team that appears to be well aligned with the DOE targets. They reported that the project appears to be correctly focused on developing framework materials with increased binding energies at ambient temperatures and conclusively showing multiple hydrogens bonded per open metal site, noting that this is a lofty goal. However, the reviewers also noted several project weaknesses, including a general lack of progress in synthesizing new materials and concern that the modeling, neutron, and high-pressure work carried out by project subcontractors is not properly guiding or benefiting the core material development task of the project.
ST-104	Novel C-B-N-Containing Hydrogen Storage Materials *Shih-Yuan Liu; Boston College*	3.4	X			Reviewers commended the comprehensive approach and active down-selection to systematically investigate novel compounds, as well as the accumulation of a rather large library of CBN compound data as hydrogen storage materials. Reviewers also praised the synthesis of difficult-to-make CBN heterocycle compounds and the characterization of dehydrogenation reaction products. Reviewers recommended that the project team place more focus on exploring new compounds with higher capacities and that can be recharged with hydrogen onboard the vehicle.

Fuel Cells

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-007	Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes *Bryan Pivovar; National Renewable Energy Laboratory*	3.0	X			Reviewers thought the approach of developing extended thin-film cathode catalysts has merit for improving catalyst activity and durability. They commended the project for its significant progress over the last year in developing highly active catalysts based on rotating disk electrode (RDE) measurements, and for translating some of them into membrane electrode assemblies (MEAs), albeit with lower fuel cell performance. Reviewers commented on the high quality of the team and its collaborative efforts. Recommendations included increasing efforts in electrode development to translate the high-activity RDE results into MEA performance. Also, reviewers saw transition metal leaching as an issue impacting durability that needs to be further addressed.
FC-008	Nanosegregated Cathode Catalysts with Ultra-Low Pt Loading *Vojislav Stamenkovic; Argonne National Laboratory*	3.6	X			According to reviewers, the synthetic results, characterization effort, and achievements in specific activity and mass activity are excellent. The reviewers noted that ANL's contribution to the structure-performance of catalyst structures is of great importance to the whole fuel cell community. They also noted that the project team is suitable, with industry, universities, and national laboratories represented. Reviewers suggested that greater emphasis be placed on integrating the catalysts into an MEA, followed by fuel cell testing, versus RDE testing.
FC-009	Contiguous Pt Monolayer O_2 Reduction Electrocatalysts on High-Stability, Low-Cost Supports *Radoslav Adzic; Brookhaven National Laboratory*	3.4	X			According to reviewers, the project is very well managed and continues to produce excellent results. Reviewers felt that the development of core-shell catalysts constitutes one of the most promising pathways to the reduction of Pt usage in polymer electrolyte membrane fuel cells (PEMFCs). They applauded the project's record on practical invention and efforts toward commercialization. They did request, if possible, for an update on development progress at the licensees (e.g., N.E. ChemCat Corporation) of the patents from this project to be given at DOE reviews. Some reviewers questioned the use of platinum group metals (PGMs) for a sufficiently stable core.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-013	Durability Improvements through Degradation Mechanism Studies *Rod Borup; Los Alamos National Laboratory*	2.9	X			Reviewers stated that LANL has made great progress in defining MEA degradation mechanisms and, to some extent, providing mitigation conditions. They felt that the approach is generally good and addresses the known issues of durability in PEMFCs. However, they noted that the project overlaps with activities (e.g., cathode carbon degradation) being pursued by automotive and fuel cell stack original equipment manufacturers (OEMs). They also noted a lack of automotive OEM collaboration. They suggested that LANL minimize efforts to explore the impact of catalyst layer cracks on membrane durability, because of technical advances that have eliminated the membrane cracks.
FC-016	Accelerated Testing Validation *Rangachary Mukundan; Los Alamos National Laboratory*	3.4	X			Reviewers lauded the project's excellent detail and accomplishments over the last year. Reviewers stated that the team is varied and experienced, with good characterization capabilities. They noted that analyzing accelerated stress tests (ASTs) to determine which conditions and tests are too aggressive and which are too passive, based on real data, is an important step. They felt that defining gaps in ASTs and working to develop ASTs to fill those gaps was also a great accomplishment. Reviewers suggested that more work should be done to determine how the gas diffusion layer aging affects performance. Reviewers also encouraged ANL to offer solutions to enhance the durability and performance of the materials.
FC-017	Fuel Cells Systems Analysis *Rajesh Ahluwalia; Argonne National Laboratory*	3.3	X			Reviewers noted that ANL has looked at a number of material configurations that are relevant to next-generation catalysts, heat rejection constraints, and optimization studies related to the system cost/catalyst metal loadings. They stated that these are all high-impact areas for fuel cell manufacturers, and that a validated system model that provides guidance for optimization in these areas is highly valuable. Reviewers applauded the inclusion of the fuel cell heat rejection requirement (Q/ΔT), and they found the new results intriguing and challenging because the fuel cell will have to operate at higher temperatures. Reviewers suggested including a turbo compressor in the model and completing a cost study that also considers end-of-life performances.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-018	Fuel Cell Transportation Cost Analysis *Brian James;* *Strategic Analysis, Inc.*	3.5	X			Reviewers viewed the analyses performed by this project as well designed, comprehensive, consistent, rigorous, sharply focused, and providing value to DOE decision makers. Reviewers suggested that more emphasis be placed on alternative systems and technologies, such as low-pressure PEMFC systems and transportation systems with different degrees of hybridization and fuel cell sizes. Reviewers also suggested the following: an increased focus on BOP, consideration of lower manufacturing volumes, comparisons of projected cost estimates with real-world fuel cell prices, evaluation of the use of dispersed Pt/C catalyst layers (instead of nanostructured thin film), and examination of portable power and low-temperature stationary fuel cell applications. Reviewers also questioned the potential of analyzing the limit of cost reduction.
FC-020	Characterization of Fuel Cell Materials *Karren More;* *Oak Ridge National Laboratory*	3.6	X			Reviewers remarked that this project contributes significantly to the fundamental understanding of degradation mechanisms, and that the team is developing characterization methods that help address critical needs of the fuel cell research community. Collaborations were found to be numerous and of high quality, and reviewers noted that they included international collaborations that provided access to unique imaging/analysis (microscopy) capabilities. Reviewers considered the recent work in adapting conditions to allow characterization of the ionomer dispersion in the catalyst layer a major accomplishment. However, they believed more work needs to be done for this methodology to be used as a quantitative measure for ionomer degradation.
FC-021	Neutron Imaging Study of the Water Transport in Operating Fuel Cells *David Jacobson;* *National Institute of Standards and Technology*	3.5	X			Reviewers noted that the team has developed a very effective approach to achieving continual improvement of the characterization techniques and testing infrastructure, while also allowing a substantial amount of time for user access to benefit the community at large. They stated that NIST has achieved impressive spatial resolution in water imaging and sped up the time frame over which measurements can be made. Reviewers lauded the project's progress to increase resolution to <10 microns, but they noted that the signal-to-noise ratio and the time resolution must be improved.

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FC-026	Fuel Cell Fundamentals at Low and Subzero Temperatures *Adam Weber; Lawrence Berkeley National Laboratory*	3.3	X			Most reviewers felt that connecting diagnostic data and materials characterization to the cell model was a solid approach. Further, reviewers stated that the focus on nanostructured thin film (NSTF) performance at low temperatures would have broad value to the community, yet some reviewers noted that the project would benefit from a more even split between NSTF and conventional dispersed catalyst electrodes. Reviewers also felt that the project team would benefit from closer collaboration with system integrators or an OEM to provide insight into issues with applying the model to stacks.
FC-065	The Effect of Airborne Contaminants on Fuel Cell Performance and Durability *Jean St-Pierre; Hawaii Natural Energy Institute*	3.2	X			Reviewers reported that this project featured a thorough approach to testing fuel cell performance with selected contaminants. They stated that the principal investigator (PI) has developed an extensive database of contaminants and identified electrochemical and chemical reaction pathways. They felt that the PI’s description of the mechanism of the increase in peroxide yield as a function of catalyst contamination was convincing. The reviewers suggested that an overview slide be provided that identifies where the selected contaminants are likely to be encountered.
FC-083	Enlarging Potential National Penetration for Stationary Fuel Cells through System Design Optimization *Genevieve Saur; National Renewable Energy Laboratory*	3.3	X			Reviewers praised the model developed by this project as a flexible and valuable tool with the potential to have broad applicability. The addition of emissions control benefits to the model structure was regarded as useful and important. Reviewers cited as a key strength the fact that the tool is developed in open-source software, and they recommended further efforts to make the model more readily available. Reviewers encouraged validation through existing installed fuel cell systems. They also identified additional collaboration with industry—especially the involvement of fuel cell producers and end users—as a key need to help validate the model. They suggested that the researchers consider model performance assessments (particularly a sensitivity analysis) around the different system elements and input parameters as a part of, or in place of, the model validation effort.

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FC-085	Synthesis and Characterization of Mixed-Conducting Corrosion-Resistant Oxide Supports *Vijay Ramani; Illinois Institute of Technology*	2.6		X		According to reviewers, the approach of using metal oxides as a replacement for conventional carbon supports is worthwhile, but the results with the selected metal oxide systems do not yet meet performance requirements for use in fuel cells. Reviewers noted good progress in preparing indium-tin-oxide (ITO) supports as a lower-cost alternative to the ruthenium-titanium-oxide supports investigated earlier in the project. However, they identified serious issues with the Pt/ITO catalyst, including low platinum surface area and poor MEA performance. Reviewers agreed that scale up of ITO-based MEAs is not worthwhile, because of the low performance thus far. Some reviewers indicated that further R&D on Pt/ITO would be worthwhile, while others indicated that the system is unlikely to have sufficient performance and stability in MEAs to justify further investment.
FC-086	Development of Novel Non-Platinum-Group-Metal Electrocatalysts for Proton Exchange Membrane Fuel Cell Applications *Sanjeev Mukerjee; Northeastern University*	3.0	X			The reviewers remarked that significant progress, especially with respect to catalyst activity and scale up, has been made in the development of non-PGM catalysts for PEMFC cathodes. However, they also expressed that there is still a long way to go before non-PGM catalysts become a practical replacement for Pt-based cathodes, especially for automotive fuel cell applications. Reviewers commended the very good collaboration between the strong team, which, they noted, consists of a good mix of academic, industry, and national laboratory partners. Also, they noted that there was a lack of clarity on future work. Recommendations included further characterization of the catalyst active site and site density.
FC-087	High-Activity Dealloyed Catalysts *Anusorn Kongkanand; General Motors*	3.2			X	For this project, reviewers commended the technical progress achieved and the level of collaboration. They noted the advances in meeting catalyst mass activity and durability milestones, as well as in transitioning the advanced catalyst to MEAs. Some reviewers commented on the lack of control of materials homogeneity limiting the ability to interpret results. Reviewers also suggested that MEA developmental work with the advanced catalyst needs to be continued, and that long-term stability needs to be assessed.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-088	Development of Ultra-Low Doped-Pt Cathode Catalysts for Polymer Electrolyte Membrane Fuel Cells *Branko Popov; University of South Carolina*	2.7		X		The reviewers saw value in the hybrid catalyst concept, where some catalytic activity is derived from the support material, but they stated that characterization and understanding of the synergies need improvement. Questions were also raised about cost and durability. Some reviewers noted an apparent lack of progress from last year. They also noted that while many of the DOE catalyst targets have been met by one or more University of South Carolina formulations, no one formulation meets all the targets. They suggested broadening and strengthening the collaborations to include more direct participation of a catalyst manufacturer. Reviewers recommended providing a better comparison with state-of-the-art catalysts and presenting as-measured data in addition to iR-free data.
FC-091	Advanced Materials and Concepts for Portable Power Fuel Cells *Piotr Zelenay; Los Alamos National Laboratory*	3.1			X	According to reviewers, incremental but significant progress has been made on many fronts relating to the use of liquid alcohol and ether fuels in portable fuel cells. Reviewers felt that the team is strong, well organized, highly capable, and very good at generating new materials. Some reviewers thought that there was limited cooperation between partners and that the interaction between the research groups needs to be improved. Reviewers also suggested that LANL address the scalability of production of the new catalysts and membranes.
FC-096	Power Generation from an Integrated Biomass Reformer and Solid Oxide Fuel Cell (SBIR Phase III Xlerator Program) *Patricia Irving; InnovaTek, Inc.*	3.3			X	According to reviewers, good technical progress has been made, especially in terms of simplifying the system design and reducing part count and cost. They were impressed by the estimated cost of $1,722/kW at 50,000 units per year, and they thought this cost would allow for good market penetration. Reviewers found the use of additive manufacturing to be beneficial, and some thought this accelerated the rate of progress. Some reviewers applauded the use of renewable biofuel for hydrogen production, but others cautioned that hydrogen from biofuel may not be cost competitive. Reviewers noted that progress toward the 2015 goals for hydrogen production and combined heat and power (CHP) systems has been impressive; however, they also reported that durability still needs to be demonstrated.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-097	Stationary and Emerging Market Fuel Cell System Cost Analysis—Auxiliary Power Units *Vincent Contini; Battelle*	3.2	X			The reviewers generally endorsed system cost analysis as a tool for assessing potential market size and identifying high-cost components where R&D funding should be applied. They reported that analysis clearly identifies BOP as the major cost driver. Reviewers stated that collaboration is broad but seems to lack input from fuel cell system integrators such as material handling equipment or truck OEMs and/or users. The reviewers recommended more sensitivity analysis; frequent updating of relevant systems, fuel cell types, and manufacturing approaches; and comparison with other cost analysis efforts.
FC-098	A Total Cost of Ownership Model for Design and Manufacturing Optimization of Fuel Cells in Stationary and Emerging Market Applications *Max Wei; Lawrence Berkeley National Laboratory*	3.1	X			Reviewers noted that the project considered a broad spectrum of fuel cell types and applications. They felt that cost breakdowns provide valuable insight into R&D needs and that health and environmental costs provide additional support for fuel cell use. The reviewers also noted that the team included a good cross section of national laboratories and private industry, but they expressed concern that the one OEM consulted has limited experience with systems integration of CHP systems. Reviewers recommended adding more industry partners to provide needed expertise in CHP systems.
FC-103	Roots Air Management System with Integrated Expander *Dale Stretch; Eaton Corporation*	3.1	X			Reviewers regarded the focus of this project—component development—as very important. Strong product knowledge and collaboration with significant fuel cell partners were viewed as strengths. While reviewers characterized the upstream (i.e., sub-system and system-level) partnerships as strong, they suggested that the project could benefit from a motor/controller partner, noting that these components seem to be the primary barriers to meeting the cost target. Reviewers lauded the progress on modeling and improving designs, as well as the hardware testing, but they saw overall progress as relatively slow. Reviewers noted that it is not clear how the proposed technology plans to achieve the DOE targets, and they indicated that it would be useful to see trade-off analysis on critical parameters. Reviewers also suggested using accelerated testing so that potential failure modes could be determined more quickly and possibly mitigated. They also recommended scaling back the plastic rotor development work because it did not seem to significantly benefit either cost or efficiency gaps.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-104	High-Performance, Durable, Low-Cost Membrane Electrode Assemblies for Transportation Applications *Andrew Steinbach; 3M*	3.0	X			Reviewers commented that the project achieved significant technical progress during the past year, and that it has good collaboration with well-coordinated partners. Some reviewers noted that the current approach may not be sufficient to further improve the developed MEA's operational robustness to allow for practical application. Some reviewers also stated that catalyst-layer architecture modification and catalyst development beyond Pt-Ni are required to improve durability and performance.
FC-106	Rationally Designed Catalyst Layers for Polymer Electrolyte Membrane Fuel Cell Performance Optimization *Deborah Myers; Argonne National Laboratory*	3.2	X			Reviewers stated that the project has a good team that includes expertise in advanced catalyst materials and fabrication, catalyst layer characterization, and modeling and diagnostics. They felt that this project addresses an essential topic for successful fuel cell commercialization. They were impressed that the project has already successfully accomplished the modification of catalyst powder with proton conducting groups. Reviewers suggested that ANL carry out durability studies and increase activities to address mass transport losses.
FC-107	Non-Precious-Metal Fuel Cell Cathodes: Catalyst Development and Electrode Structure Design *Piotr Zelenay; Los Alamos National Laboratory*	3.2	X			Reviewers stated that the project team is excellent and features a great breadth of collaborators as well as a highly experienced PI and supporting institution. They noted that this project has the potential to provide many answers about the complex electrochemistry of non-PGM catalysts. They applauded LANL's strong technical improvements over the current state of the art. Reviewers suggested that LANL address durability and increase mechanistic studies.
FC-108	Advanced Ionomers and Membrane Electrode Assemblies for Alkaline Membrane Fuel Cells *Bryan Pivovar; National Renewable Energy Laboratory*	2.9	X			Most reviewers noted that the approach to developing novel AEMs was reasonable, albeit with a relatively high degree of risk. They commented that the assembled team was excellent and a very good mix of national laboratories, academia, and industry. The reviewers viewed the project as being in an early stage of development, with progress, perhaps, being a bit slow. There was concern that the hydroxide form of the membrane had not yet been characterized sufficiently (especially for OH^- conductivity), and reviewers recommended that more focus be placed in this area.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-109	New Fuel Cell Membranes with Improved Durability and Performance *Michael Yandrasits; 3M*	3.2	X			Reviewers indicated that the first-year project has made good progress. They reported that the approach of using multiple acid sites per side chain, including sulfonate and sulfonimide sites, was a viable pathway toward preparing ionomers that combine high conductivity with low swelling. Reviewers felt that the use of inert electrospun nanofibers to provide mechanical stability was a promising route to achieving a high degree of stabilization with a minimal increase in resistance. However, they highlighted the lack of information about the scalability and expected manufacturing cost as a concern. Reviewers suggested cost analysis of the ionomer and electrospinning process as a future task. Increased reliance on larger-scale testing was also suggested as a way to identify possible problems and down-select materials earlier in the process.
FC-110	Advanced Hybrid Membranes for Next-Generation Polymer Electrolyte Membrane Fuel Cell Automotive Applications *Andrew Herring; Colorado School of Mines*	2.9	X			Reviewers noted that the project does address the critical barriers and builds on the PI's previous projects developing HPA-containing polymers. It is mentioned, however, that much of the work so far has been reviewing work previously completed. Reviewers noted that the approach is fundamentally sound and may lead to a large improvement in performance, as opposed to smaller incremental gains. However, reviewers felt that progress so far has been limited, while noting that the project is in its early stages. Reviewers felt that appropriate partners are identified; however, there does not appear to be much collaboration to date.

Manufacturing R&D

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
MN-001	Fuel Cell Membrane Electrode Assembly Manufacturing Research and Development *Michael Ulsh; National Renewable Energy Laboratory*	3.4	X			The reviewers noted that the relevance of the approach to actual manufacturing practices and to industry is implicit in the collaborations with membrane electrode assembly and membrane suppliers. They commended the implementation of the quality control (QC) techniques in industry and recommended bringing the QC techniques to additional original equipment manufacturers. Reviewers applauded the decision to stop work on an ionomer/carbon ratio diagnostic, because it showed that the team is grounded in the practical development of techniques and not wasting time on tasks that are not feasible. The reviewers would like to see a correlation established between defect or defect size and fuel cell performance. This issue should be addressed in fiscal year 2015.

Technology Validation

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-008	Fuel Cell Bus Evaluations *Leslie Eudy;* *National Renewable Energy Laboratory*	3.7	X			Tangible results from this project were found to provide a consistent history of technology performance and cost improvements over time while also delivering value to decision makers. Reviewers noted that thorough evaluations, quality information, and active collaboration with stakeholders have led to a better understanding of the status of technology development relative to DOE's goals. Reviewers reported that changes in fleet management and some buses going out of service have posed challenges and questions related to the potential impact. Reviewers suggested evaluating more transit agencies while also comparing findings with similar data from other countries.
TV-016	Stationary Fuel Cell Evaluation *Genevieve Saur;* *National Renewable Energy Laboratory*	3.4	X			While the project was noted to have useful market data and analyses, some data-related concerns were raised. The collection of data on a voluntary basis led to reviewers questioning whether there could potentially be bias (poor performers may be less willing to share data compared to better performers). It was noted that there is a lack of disaggregation of data according to technology categorizations and applications. The reviewers recommended that the data evaluation process be more clearly linked to key research or technology deployment questions, and that feedback should be given to DOE about the gaps in technology performance and market status. Reviewers also suggested expanding collaborations and obtaining more state partners in order to provide geographic variability.
TV-019	Hydrogen Component Validation *Kevin Harrison;* *National Renewable Energy Laboratory*	3.2	X			Because compressors are a key reliability issue in hydrogen stations, this project's evaluation of compressor failure mechanisms was seen to have the potential for a large impact. Reviewers indicated that the project has a well-structured approach and addresses a key area that has not had much transparency in the past. They expressed concern that the "accelerated" test program does not follow the right approach. Reviewers suggested that, initially, specific failure modes of the compressor should be explored, followed by a repetition of those factors. Reviewers also highly recommended obtaining input from other compressor suppliers. Other recommendations included the development of a "generic" tool to also be used by other compressor technologies, and conducting technoeconomic analysis of the impact of the project.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-020	Validation of an Advanced High-Pressure Polymer Electrolyte Membrane Electrolyzer and Composite Hydrogen Storage, with Data Reporting, for SunHydro Stations *Larry Moulthrop; Proton OnSite*	3.4	X			The project was perceived as well designed, with a real-world strategy and the potential to lower costs. The coordination between the four project partners was noted to be a true partnership, with each bringing its strength to the team. Reviewers praised the attention given to safety, codes, and standards. An area identified as needing more attention was cost targets and estimation, along with the evaluation of the economic impact of installing high-pressure electrolysis. Reviewers recommended considering scale-up of the station; integrating the containers/components into one pallet (to enable ease of shipping); and comparing power consumption values from this project to those of the California State University, Los Angeles, hydrogen station project (TV-024). Reviewers also noted that they would like to see more detailed data on total electrolyzer energy consumption.
TV-021	Forklift and Backup Power Data Collection and Analysis *Jennifer Kurtz; National Renewable Energy Laboratory*	3.8	X			The project was observed as adding value to the commercialization of niche market hydrogen and fuel cell technologies while also addressing barriers and giving appropriate attention to the types of metrics required to confront these barriers. Reviewers viewed outstanding sensitivity analysis and active interaction with manufacturers and users as key strengths of the project. While reviewers did not identify any major weaknesses, they did make several recommendations for enhancement; for example, they suggested that the project team should further gauge whether industry would be willing to continue to provide data, and that encouragement to do so would be beneficial. Reviewers also noted that obtaining qualitative verbal feedback from operators of these systems could provide better insight.
TV-024	California State University, Los Angeles, Hydrogen Refueling Facility Performance Evaluation and Optimization *David Blekham; California State University, Los Angeles*	3.0	X			The project was seen as having the potential to identify optimization potentials for components of electrolysis-based hydrogen fueling stations while having the added benefit of an educational aspect. However, reviewers noted a lack of clarity in plans regarding how the station will be optimized, as well as a lack of a technoeconomic plan to evaluate the economic advantages of the proposed solutions. While collaboration with California Weights and Measures was seen as valuable, increased collaboration with other entities that have developed hydrogen stations was highly recommended. Further reviewer suggestions included developing measurable goals for addressing barriers and meeting targets, and providing feedback on how to reduce capital and operating costs. Reviewers also suggested comparing the electrolyzer power consumption with that of the electrolyzer used in project TV-020.

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TV-025	Performance Evaluation of Delivered Hydrogen Fueling Stations *Michael Tieu; Gas Technology Institute*	3.1	X			The project was viewed by reviewers as having the potential to provide valuable data on hydrogen stations while also enabling comparisons across stations and helping to expand the network of stations. Reviewers noted that they expected to see more details on the performance parameters that are being validated and a clearer expression of how barriers are being addressed. The partnership with Linde was perceived as a key strength of the project because these partners have a good working relationship and bring vast experience to the table. Because the project timeline is dependent on factors outside the control of the project investigators (e.g., permitting and construction delays), reviewers suggested that the project team reevaluate the feasibility of implementing all five stations as well as perform risk analysis and planning. Reviewers also noted that addressing costs targets should be a part of project goals.
TV-026	Hydrogen Fueling Infrastructure Research and Station Technology *Brian Somerday; Sandia National Laboratories*	2.5	X			Reviewers viewed the project as having the potential to contribute to the deployment of hydrogen stations and to address real-time technology performance and operation issues. However, the project was perceived as too new to be comprehensively evaluated, and some reviewers were uncertain about the value proposition of such an effort. Reviewers suggested that further attention be devoted to characterizing H2FIRST, and that indicators of project success be measurable. Reviewers also cautioned that care should be given to effectively manage the entities involved while specifying objectives within each project team.

Safety, Codes and Standards

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SCS-001	National Codes and Standards Deployment and Outreach *Carl Rivkin; National Renewable Energy Laboratory*	3.1	X			Reviewers recognized the importance and potential impact of this project and the extensive list of collaborators involved, noting the scope and breadth of the work as a clear strength. However, they also noted that stronger engagement with and more substantive feedback from industry stakeholders, national laboratories, and trade associations is needed. Reviewers commented on the lack of a cohesive approach or strategy to codes and standards deployment and outreach. They suggested that the work would benefit from increased coordination with related projects active at other laboratories or programs. Reviewers stated that deployment efforts should include the international community and that project activities should focus on more substantive outputs and accomplishments.
SCS-002	Component Standard Research and Development *Robert Burgess; National Renewable Energy Laboratory*	2.5	X			Reviewers recognized that this work is critical to the advancement of regulations, codes, and standards for hydrogen and fuel cell technologies. The project was commended for its collaboration with manufacturers and system installers to ensure that certified products are commercially available. Project collaborations with codes development organizations (CDOs), standards development organizations (SDOs), and industry were cited as a strength, as was the project's work to focus standards development efforts at the component level. However, reviewers noted that collaboration and/or teaming with SNL could avoid duplication of effort. Reviewers recommended that the project team craft a more strategic approach and scope for the project. They also suggested that the project team better leverage the expertise of other national laboratories (e.g., SNL). In addition, reviewers noted that rather than focusing on a single component, the project team should focus on assessing multiple components for a given set of infrastructure hardware.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SCS-004	Hydrogen Safety, Codes, and Standards: Sensors *Eric Brosha; Los Alamos National Laboratory*	2.9	X			Reviewers acknowledged the significant progress made in developing a hydrogen-specific sensor; however, they also noted the lack of a commercial manufacturing partner to demonstrate the feasibility and questioned if commercially available, economically viable sensors would result. Reviewers praised the project's potential for deploying improved sensors that include wireless communications and backward compatibility with relatively low operating costs. In addition to the lack of a mainline sensor manufacturer partner, reviewers noted the lack of analysis of sensor performance versus International Organization for Standardization 26142: Hydrogen Detection Apparatus as a weakness. It was recommended that the project team expand the field tests to better replicate real-world conditions. The researchers also noted that the project team should better define the life cycle cost and market price of a deployment-ready sensor, as well as how the market price compares to commercially available products.
SCS-005	Research and Development for Safety, Codes and Standards: Materials and Components Compatibility *Chris San Marchi; Sandia National Laboratories*	3.5	X			Reviewers noted that the project team demonstrated a sound and valuable approach to addressing key technical gaps. Reviewers also commended the project team for its engagement with CDOs and SDOs and its progress, noting that it is relevant and aligned with that of industry. Cited project strengths included the direct impact on current and near-term standards development activities and the valuable input to industry regarding lower-cost steels and the benefits of automated welding over manual welding. Reviewers recommended an increased focus on communicating results and lessons learned to station builders and design engineers as well as improving efforts to address actual service conditions. Reviewers noted that while future work advances logically from the achievements to date, it is important to ensure that the end goal supports the development of American National Standards Institute standards.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SCS-007	Hydrogen Fuel Quality *Tommy Rockward; Los Alamos National Laboratory*	3.6	X			Reviewers recognized the progress made in developing an in-line fuel quality analyzer to enable the commercialization of fuel cell electric vehicles. The participation of international and domestic CDOs and SDOs, and the project's strong technical data development, were seen as strengths that are positively contributing to the international harmonization of fuel quality standards and addressing barriers to the deployment of hydrogen infrastructure. However, reviewers noted a lack of reference to and support of SAE J2719, as well as the project's limited national outreach and feedback activities. Reviewers recommended that future work extend testing from the membrane electrode assembly to the stack level. J2719 compliance testing was also recommended for future work.
SCS-011	Hydrogen Behavior and Quantitative Risk Assessment *Katrina Groth; Sandia National Laboratories*	3.4	X			Reviewers commended the progress made in developing the Hydrogen Risk Assessment Models tool and how the project team addressed the previous years' feedback. The development of a performance-based approach to risk assessment and the establishment of a benchmark metric for station readiness were noted as major accomplishments. However, reviewers commented on the lack of engagement with code officials—the ultimate end users of the quantitative risk assessment toolkit. Recommendations included developing an approach to educate code officials and expanding collaborations with international entities, hydrogen suppliers, and car manufacturers.
SCS-015	Hydrogen Emergency Response Training for First Responders *Monte Elmore; Pacific Northwest National Laboratory*	3.1	X			According to reviewers, this project has demonstrated continued progress through the provision of online and in-person training and education. However, reviewers identified a lack of urgency in conducting training events and a need for a more creative means of educating first responders. They recommended that the project team develop an improved strategy to engage targeted stakeholders such as local fire and police departments. Reviewers also suggested seeking increased feedback from key stakeholders and participants in training activities.
SCS-017	Hands-On Hydrogen Safety Training *Salvador Aceves; Lawrence Livermore National Laboratory*	3.1	X			According to reviewers, this project fills an important knowledge gap but could have a greater impact if larger audiences were targeted. Cited project strengths included LLNL's ability to leverage its expertise to provide practical training courses—particularly, the provision of both Internet-based and hands-on safety classes. Reviewers noted the lack of significant progress and the heavy focus on high-pressure systems, rather than on hydrogen gas and hydrogen-specific applications, as weaknesses. It was recommended that the project team identify a long-term plan to engage broader audiences or hand off the training course for industry to continue.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SCS-019	Hydrogen Safety Panel and Hydrogen Safety Knowledge Tools *Nick Barilo; Pacific Northwest National Laboratory*	3.8	X			Reviewers commended the project's flexibility in developing tools and resources that keep pace with the changing stages of technology commercialization. They also recognized the Hydrogen Safety Panel for doing outreach to project developers and for being involved in the early design stages. Cited strengths included the outreach to insurance groups and authorities having jurisdiction to better understand user needs and the innovative approaches for providing informational tools and resources. Reviewers recommended collaborating with the Fuel Cell & Hydrogen Energy Association (FCHEA) and NREL to avoid duplication of effort and to provide more robust products.
SCS-021	National Renewable Energy Laboratory Hydrogen Sensor Testing Laboratory *William Buttner; National Renewable Energy Laboratory*	3.2	X			This project was recognized for its focus on addressing technical barriers in terms of reliability, durability, and the cost of deploying hydrogen sensors. Reviewers acknowledged the project's potential impact in terms of supporting stationary applications and vehicle repair facilities, its strong international collaborations on basic research, and its efficient use of resources. Reviewers questioned the lack of collaboration with automotive original equipment manufacturers (OEMs) and the Asian hydrogen communities, given their resources and expertise. Reviewers recommended an increased focus on stationary applications rather than vehicles; an industry workshop to identify the needs of OEMs, code requirements, and a path to listing hydrogen-specific sensors in UL 2075; and increased alignment of hydrogen fuel quality detection activities with related work in other Safety, Codes and Standards and Hydrogen Delivery sub-program projects.
SCS-022	Fuel Cell & Hydrogen Energy Association Codes and Standards Support *Karen Hall; Fuel Cell & Hydrogen Energy Association*	3.1	X			Reviewers acknowledged FCHEA's collaboration with international and domestic organizations and SDOs and its focus on multiple technology applications (e.g., transportation, stationary power, and portable power). Reviewers praised the depth of expertise and experience that FCHEA demonstrated in its role in coordinating efforts with industry to support DOE activities. Reviewers also noted weaknesses in the project, such as the lack of a cohesive strategy and the lack of identification of specific contributions to optimize the project's relevance and potential impact on industry. It was recommended that the project pursue a more proactive approach to engaging SDOs to help accelerate the standards development process and improve the quality of promulgated standards. Reviewers also suggested collecting more feedback from key stakeholders regarding barriers to commercialization to better leverage the associations' member bases.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SCS-023	Hydrogen Leak Detector for Hydrogen Dispenser *Igor Pavlovsky; Applied Nanotech*	3.0			X	Reviewers praised this project for its progress, demonstrated repeatability, and high accuracy (at up to 2%) over a wide temperature range. It was also recognized for its collaboration with NREL. Reviewers noted that project strengths such as the simple sensor design and low cost could benefit station developers and automotive OEMs. The project's identified weaknesses included the lack of data on interference, sensor drift, and long-term durability. Reviewers recommended that the project continue to Phase II only if the NREL testing shows promise for the technology. They stated that if the project advances to Phase II, at least one partner (e.g., a fueling station or an OEM) should be added.

Market Transformation

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
MT-006	Fuel Cell Combined Heat and Power Commercial Demonstration *Kriston Brooks; Pacific Northwest National Laboratory*	3.3	X			Reviewers commented that this project will help introduce combined heat and power systems at consumer locations. Also, reviewers stated that the data collected from these applications has provided valuable insight into the system's effectiveness and reliability. It was suggested that feedback is needed from host organizations about their experiences with the system, the system's costs/benefits, the worthiness of using the system without U.S. Department of Energy support, and any system changes needed.
MT-007	Landfill Gas to Hydrogen *Shannon Baxter-Clemmons; South Carolina Hydrogen and Fuel Cell Alliance*	3.2			X	Several reviewers commented that this project showcases an opportunity to produce hydrogen that is viable for use in fuel cells from landfill gas, which is often an unrealized asset. Reviewers noted that the project lacks cost information on the impact of the revised gas cleanup standards. This project will be completed in FY 2015.
MT-008	Hydrogen Energy Systems as a Grid Management Tool *Mitch Ewan; Hawaii Natural Energy Institute*	3.6	X			Reviewers stated that this project will enhance the ability to use renewables by mitigating the grid instability caused by those renewables. Reviewers suggested that it would be beneficial for the project team to work with utility companies to monetize grid benefits from electrolysis and to install electrolyzers in distributed locations.
MT-011	Ground Support Equipment Demonstration *Jim Petrecky; Plug Power*	3.1	X			Reviewers reported that the plan to complete this project is reasonable, with a number of go/no-go decisions that will help mediate the risk of this project. However, they also stated that the summer 2014 schedule seems very aggressive and will need to be monitored.
MT-013	Maritime Fuel Cell Generator Project *Joe Pratt; Sandia National Laboratories*	3.6	X			Reviewers stated that the project has done an outstanding job of coordinating between the fuel cell supplier, the fuel cell customer, the infrastructure support, and the relevant regulatory agencies. However, reviewers also stated that perhaps Ballard should be added to the project team so that the project team includes fuel cell expertise and not just electrolysis.
MT-014	Fuel-Cell-Based Auxiliary Power Unit for Refrigerated Trucks *Kriston Brooks; Pacific Northwest National Laboratory*	3.0	X			Reviewers stated that this project could meet a need of the trucking industry, save fuel, reduce greenhouse gases, and create a market for fuel cell technology. However, they stated that the funding/time does not seem sufficient for full integration (e.g., electrical integration with the truck refrigeration unit), and that the reason for 400-hour demonstrations was not defined.

Systems Analysis

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
AN-033	Analysis of Optimal Onboard Storage Pressure for Hydrogen Fuel Cell Vehicles *Zhenhong Lin; Oak Ridge National Laboratory*	3.2	X			Reviewers observed that the project approach was sound but needs to include multi-objective optimization because of the importance of different vehicle parameters. They recommended that the project expand the collaboration to automobile manufacturers. Reviewers noted that the project provides a useful analysis of clustered deployment strategies compared to a region-wide infrastructure deployment. They also stated that the project could be strengthened by improving the validation of the underlying assumptions, including the consumer's value of time.
AN-035	Employment Impacts of Infrastructure Development for Hydrogen and Fuel Cell Technologies *Marianne Mintz; Argonne National Laboratory*	3.4	X			Reviewers agreed that the project is useful, and that the identification of the economic benefits and job creation impacts of hydrogen fuel cell electric vehicles and the associated infrastructure will be valuable for policy considerations and decisions. They stated that the project team should consider including a comparative analysis with other conventional and alternative fuels and refueling infrastructure. They recommended that additional station developers should peer review the technical and economic sections of the model, and that the project scope should be expanded to include displaced jobs.
AN-036	Pathway Analysis: Projected Cost, Life Cycle Energy Use, and Emissions of Future Hydrogen Technologies *Todd Ramsden; National Renewable Energy Laboratory*	3.1	X			Reviewers observed that this project was well developed and utilized a highly structured approach. They commended the project for making excellent progress and noted that the analysis is instrumental for the DOE, industry, and other stakeholders in defending the merits of hydrogen fuel. Reviewers stated that the project could be strengthened by updating key assumptions. In addition, they suggested performing sensitivity cases relative to a decarbonized U.S. economy.
AN-039	Life Cycle Analysis of Water Consumption for Hydrogen Production Pathways *Amgad Elgowainy; Argonne National Laboratory*	3.4	X			Reviewers acknowledged that the project addresses a critical need by adding water consumption to the life cycle analysis of the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET) model. They commended the project on its excellent progress made to date and its excellent approach for the comparative analysis. They recommended that the project team consider regional analysis of water issues in the future.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
AN-044	Impact of Fuel Cell System Peak Efficiency on Fuel Consumption and Cost *Aymeric Rousseau; Argonne National Laboratory*	3.4	X			Reviewers observed that the project is well designed and that the assumptions are realistic. They acknowledged that the use of probability of targets was a good approach and provided a good understanding of the interaction of fuel stack efficiency, cost, and hydrogen storage cost. They encouraged the project team to increase its collaboration with the automobile manufacturers.
AN-045	Analysis of Incremental Fueling Pressure Cost *Amgad Elgowainy; Argonne National Laboratory*	3.6	X			Reviewers commented that the project took an excellent approach to understanding the hydrogen storage and dispensing configurations and the costs for stations. They mentioned that the project provided a thorough understanding of the cost of the station components and the resulting cost for hydrogen dispensed at various pressures. The reviewers encouraged the project team to pursue more in-depth collaboration and consultation with hydrogen component suppliers.
AN-046	Hydrogen Station Economics and Business (HySEB)—Preliminary Results *Zhenhong Lin; Oak Ridge National Laboratory*	3.0		X		It was acknowledged that the analysis provided information and findings on how the station size and deployments can affect the net present value of the station. Reviewers stated that the project goals were too broad, and that the results were difficult to interpret. They noted that a simple definition is needed of the objective function that the consumer/investor seeks to optimize. The reviewers remarked that the project should define the assumptions embedded in the model, and that the analysis could be strengthened by comparing the parameters and assumptions to existing modeling of hydrogen cost, station cost, and infrastructure locations.
AN-047	Tri-Generation Fuel Cell Technologies for Location-Specific Applications *Brendan Shaffer; University of California, Irvine*	3.0	X			According to reviewers, the project's approach was reasonable but limited. They stated that the scope should be expanded to consider other sources of fuel, such as natural gas from pipelines, and economic analysis of the tri-generation system. They also felt that project collaborations should include fuel cell companies with expertise in tri-generation and stakeholders in the Northeast.
AN-049	Electricity Market Valuation for Hydrogen Technologies *Joshua Eichman; National Renewable Energy Laboratory*	3.4			X	Reviewers commented that this analysis project was very relevant to the Fuel Cell Technologies Office goals and objectives and made significant progress. They stated that the project's findings will contribute to understanding of the application of hydrogen for energy storage and the associated costs and financial benefits. They noted that project collaboration should be expanded to include other industry stakeholders and countries that are installing energy storage projects.

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Introduction

The fiscal year (FY) 2014 U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program) Annual Merit Review and Peer Evaluation Meeting (AMR), in conjunction with DOE's Vehicle Technologies Office Annual Merit Review, was held June 16–20, 2014, at the Washington Marriott Wardman Park Hotel in Washington, DC. This report is a summary of comments by AMR peer reviewers about the hydrogen and fuel cell projects funded by DOE's Office of Energy Efficiency and Renewable Energy (EERE). Projects supported by other DOE offices (including the Office of Science [Basic Energy Sciences] and ARPA-E) in areas relevant to hydrogen and fuel cells were also presented at the FY 2014 AMR. DOE uses the results of this merit review and peer evaluation, along with additional review processes, to make funding decisions for upcoming fiscal years and help guide ongoing performance improvements to existing projects.

The objectives of this meeting include the following:

- Review and evaluate FY 2014 accomplishments and FY 2015 plans for DOE laboratory programs; industry/university cooperative agreements; and related research, development, and demonstration (RD&D) efforts.
- Provide an opportunity for stakeholders and participants (e.g., fuel cell manufacturers, component developers, and others) to provide input to help shape the DOE-sponsored RD&D program in order to address the highest-priority technical barriers and facilitate technology transfer.
- Foster interactions among the national laboratories, industry, and universities conducting RD&D.

The peer review process followed the guidelines in the *Peer Review Guide* developed by EERE. The peer review panel members, listed in Table 1, provided comments about the projects presented. Panel members included experts from a variety of backgrounds related to hydrogen and fuel cells, and they represented national laboratories; universities; various government agencies; and manufacturers of hydrogen production, storage, delivery, and fuel cell technologies. Each reviewer was screened for conflicts of interest 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	Abdel-Baset, Tarek	Chrysler Group LLC
2	Adzic, Radoslav	Brookhaven National Laboratory
3	Afzal, Kareem	PDC Machines, Inc.
4	Ahmed, Shabbir	Argonne National Laboratory
5	Ainscough, Chris	National Renewable Energy Laboratory
6	Antoni, Laurent	Commissariat A l'Energie Atomique (CEA)
7	Antos, George	National Science Foundation
8	Araghi, Koorosh	National Aeronautics and Space Administration
9	Artyushkova, Kateryna	University of New Mexico
10	Atanasiu, Mirela	European Commission, Fuel Cells and Hydrogen Joint Undertaking
11	Autrey, Thomas	Pacific Northwest National Laboratory
12	Ayers, Katherine	Proton OnSite
13	Balema, Viktor	Sigma-Aldrich
14	Barbosa, Nicholas	National Institute of Standards and Technology
15	Barilo, Nick	Pacific Northwest National Laboratory
16	Baturina, Olga	U.S. Navy, Naval Research Laboratory
17	Benjamin, Thomas	Argonne National Laboratory
18	Birdsall, Jackie	Toyota Engineering and Manufacturing America
19	Bonner, Brian	Air Products and Chemicals, Inc.
20	Bordeaux, Christopher	Bordeaux International Energy Consulting LLC
21	Borup, Rod	Los Alamos National Laboratory
22	Bouwkamp, Nico	California Fuel Cell Partnership
23	Bowman, Robert	Oak Ridge National Laboratory

No.	Name	Organization
24	Boyd, Robert	Boyd Hydrogen LLC
25	Brink, Andy	Michelman
26	Brown, Craig	National Institute of Standards and Technology
27	Bunnelle, Eric	ExxonMobil
28	Burgunder, Albert	Praxair, Inc.
29	Burke, Kenneth	National Aeronautics and Space Administration, Glenn Research Center
30	Busby, F. Colin	W.L. Gore & Associates, Inc.
31	Butsch, Hanno	NOW GmbH
32	Cairns, Julie	CSA Group
33	Centeck, Kevin	U.S. Army, TARDEC (Tank Automotive Research, Development and Engineering Center)
34	Choudhury, Biswajit	DuPont Fuel Cells
35	Christiansen, Katy	U. S. Department of Energy, American Association for the Advancement of Science Fellow
36	Co, Anne	Ohio State University
37	Cole, Brian	U.S. Army RDECOM/CERDEC (Research, Development and Engineering Command/Communications-Electronics Research, Development and Engineering Center)
38	Cole, Vernon	CFD Research Corporation
39	Collins, William	Consultant
40	Contini, Vince	Battelle
41	Creager, Stephen	Clemson University
42	Cullen, David	Oak Ridge National Laboratory
43	Curry-Nkansah, Maria	Argonne National Laboratory
44	Dale, Nilesh	Nissan USA
45	Datye, Abhaya	University of New Mexico
46	De Castro, Emory	Advent Technologies, Inc.
47	Debe, Mark	Consultant (formerly 3M)
48	Dedrick, Daniel	Sandia National Laboratories
49	Dinh, Huyen	National Renewable Energy Laboratory
50	Eckerle, Tyson	Zero Emissions Vehicle Infrastructure Project Manager, State of California
51	Elrick, William	California Fuel Cell Partnership
52	Erdle, Erich	EFCECO, Erdle Fuel Cell & Energy Consulting
53	Erlebacher, Jonah	Johns Hopkins University
54	Eudy, Leslie	National Renewable Energy Laboratory
55	Ewan, Mitch	University of Hawaii, Manoa
56	Faldi, Alessandro	ExxonMobil
57	Fan, Chinbay	Gas Technology Institute
58	Farese, David	Air Products and Chemicals, Inc.
59	Felter, Tom	Sandia National Laboratories
60	Fenske, George	Argonne National Laboratory
61	Funk, Stuart	LMI
62	Gangi, Jennifer	Fuel Cells 2000
63	Garzon, Fernando	Los Alamos National Laboratory
64	Ge, Qingfeng	Southern Illinois University
65	Gennett, Thomas	National Renewable Energy Laboratory
66	Gittleman, Craig	General Motors, Research & Development Center
67	Grassilli, Leo	Consultant - Office of Naval Research
68	Greene, David L	Oak Ridge National Laboratory / University of Tennessee
69	Gross, Tom	Energy Planning and Solutions (Consultant)

No.	Name	Organization
70	Grot, Stephen	Ion Power
71	Gu, Wenbin	General Motors
72	Hall, Karen	Fuel Cell and Hydrogen Energy Association
73	Hamdan, Monjid	Giner, Inc.
74	Hamilton, Jennifer	California Fuel Cell Partnership
75	Han, Taehee	Nissan USA
76	Hancock, Dave	Plug Power, Inc.
77	Hardis, Jonathan	National Institute of Standards and Technology
78	Harris, Aaron	Air Liquide
79	Harvey, David	Ballard Power Systems
80	He, Wensheng	Arkema, Inc.
81	Hennessey, Barbara	U.S. Department of Transportation
82	Herring, Andy	Colorado School of Mines
83	Hirano, Shinichi	Ford Motor Company
84	Holladay, Jamie	Pacific Northwest National Laboratory
85	Houle, Frances A	Lawrence Berkeley National Laboratory
86	James, Brian	Strategic Analysis, Inc.
87	Jaramillo, Thomas	Stanford University
88	Jensen, Craig	University of Hawaii, Honolulu
89	Josefik, Nicholas	U.S. Army Corps of Engineers (USACE-DOD)
90	Junge, Axel	General Motors, Research & Development Center
91	Keller, Jay	Consultant (formerly Sandia National Laboratories)
92	Kim, Sangtae	University of California, Davis
93	Knights, Shanna	Ballard Power Systems
94	Kocha, Shyam	National Renewable Energy Laboratory
95	Kongkanand, Anusorn	General Motors Corporation
96	Kopasz, John	Argonne National Laboratory
97	Kraigsley, Alison	National Institutes of Health
98	Kurtz, Jennifer	National Renewable Energy Laboratory
99	Lakshmanan, Balsu	General Motors Corporation
100	Levy, Michael	Aaqius & Aaqius S. A.
101	Lewis, Michele	Argonne National Laboratory
102	Liu, Di-Jia	Argonne National Laboratory
103	Madden, Tom	Consultant
104	Maes, Miguel	National Aeronautics and Space Administration
105	Markovic, Nenad	Argonne National Laboratory
106	Maroni, Victor	Argonne National Laboratory
107	McGuire, Tim	Daimler AG
108	McKone, Thomas	Lawrence Berkeley National Laboratory
109	McWhorter, Scott	Savannah River National Laboratory
110	Melaina, Marc	National Renewable Energy Laboratory
111	Mergel, Jürgen	Forschungszentrum Jülich GmbH
112	Merritt, James	U.S. Department of Transportation
113	Miller, James	Argonne National Laboratory
114	Minh, Nguyen	GE Global Research Center
115	Mittelsteadt, Cortney	Giner, Inc.
116	Mohtadi, Rana	Toyota Engineering and Manufacturing America
117	Moulthrop, Larry	Proton OnSite
118	Mukerjee, Sanjeev	Northeastern University
119	Mukundan, Rangachary	Los Alamos National Laboratory
120	Myers, Charlie	Trenergi Corporation
121	Myers, Deborah	Argonne National Laboratory

No.	Name	Organization
122	Nicholas, Mike	University of California, Davis
123	O'Brien, James	Idaho National Laboratory
124	Ohma, Atsushi	Nissan (Japan)
125	Olson, Gregory	Consultant – SRA International, Inc.
126	O'Malley, Rachel	Johnson Matthey
127	Ott, Kevin	Los Alamos National Laboratory (retired)
128	Owejan, Jon	State University of New York, Alfred State
129	Pallasch, Johannes	NOW GmbH
130	Parks, George	FuelScience LLC / Phillips 66
131	Patel, Pinakin	FuelCell Energy, Inc.
132	Penev, Michael	National Renewable Energy Laboratory
133	Perret, Robert	Nevada Technical Services LLC
134	Perry, Mike	United Technologies Research Center
135	Petrovic, John	Petrovic and Associates
136	Pietrasz, Patrick	Ford Motor Company
137	Pivovar, Bryan	National Renewable Energy Laboratory
138	Podolski, Walt	Argonne National Laboratory
139	Polevaya, Olga	Nuvera Fuel Cells, Inc.
140	Ramsden, Todd	National Renewable Energy Laboratory
141	Resende, William	BMW
142	Richards, Mark	FuelCell Energy, Inc.
143	Rinebold, Joel	Connecticut Center for Advanced Technology, Inc.
144	Rorrer, Greg	National Science Foundation
145	Rufael, Tecle	Chevron
146	Sandrock, Gary	Oak Ridge National Laboratory
147	Schlasner, Steven	University of North Dakota, EERC
148	Shaffer, Brendan	University of California, Irvine
149	Shaw, Suzanne	European Commission, Fuel Cells and Hydrogen Joint Undertaking
150	Shenoy, Dev	U.S. Department of Energy, Advanced Manufacturing Office
151	Siegel, Don	University of Michigan, Ann Arbor
152	Sievers, Robert	Teledyne Energy Systems
153	Simnick, James	BP America
154	Simpson, Lin	National Renewable Energy Laboratory
155	Skolnik, Ed	Energetics Incorporated
156	Sofronis, Petros	University of Illinois, Urbana-Champaign
157	Soto, Herie	Shell Hydrogen LLC
158	Stamenkovic, Vojislav	Argonne National Laboratory
159	Steen, Marc	European Commission, Joint Research Centre
160	Steinbach, Andy	3M
161	St-Pierre, Jean	University of Hawaii, Manoa
162	Swider-Lyons, Karen	U.S. Navy, Naval Research Laboratory
163	Tamhankar, Satish	Linde
164	Thomas, C.E. (Sandy)	Clean Car Options
165	Trabold, Tom	Rochester Institute of Technology
166	Trocciola, John	SRA International, Inc.
167	Turner, John	National Renewable Energy Laboratory
168	Valdez, Thomas	National Aeronautics and Space Administration, Jet Propulsion Laboratory
169	van der Vliet, Dennis	3M
170	Veenstra, Mike	Ford Motor Company
171	Verduzco, Laura	Chevron LLC

No.	Name	Organization
172	Wagner, Frederick T.	General Motors Corporation (retired)
173	Waldecker, James	Ford Motor Company
174	Walk, Alex	SGL Group
175	Wang, Conghua	TreadStone Technologies, Inc.
176	Warren, Dave	Oak Ridge National Laboratory
177	Weber, Adam	Lawrence Berkeley National Laboratory
178	Wegrzyn, Jim	Brookhaven National Laboratory
179	Wei, Max	Lawrence Berkeley National Laboratory
180	Wen, Jennifer	University of Warwick
181	Wessel, Silvia	Ballard Power Systems
182	Wheeler, Douglas	DJW Technology LLC
183	Williams, Mark	National Energy Technology Laboratory
184	Woods, Stephen	National Aeronautics and Space Administration
185	Xu, Qiang	National Institute of Advanced Industrial Science and Technology (AIST)
186	Zelenay, Piotr	Los Alamos National Laboratory
187	Zhao, Ji-Cheng	U.S. Department of Energy, Office of Energy Efficiency and Renewable Energy

Summary of Peer Review Panel's Crosscutting Comments and Recommendations

AMR panel members provided comments and recommendations regarding selected DOE hydrogen and fuel cell projects, overall management of the Hydrogen and Fuel Cells Program, and the AMR peer evaluation process. The project comments, recommendations, and scores are provided in the following sections of this report, grouped by sub-program area. Comments about sub-program management are provided in Appendix B.

Analysis Methodology

A total of **100** FCTO projects were reviewed at the meeting. As shown in Table 1, **187** review panel members participated in the AMR process, providing a total of **664** project evaluations. These reviewers were asked to provide numeric scores (on a scale of 1–4, including half-point intervals, 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 not reviewed, is provided in Appendix D.

For the Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Safety, Codes and Standards; and Systems Analysis sub-programs, scores were based on the following five criteria and weights:

Score 1: Approach to performing the work (20%)
Score 2: Accomplishments and progress toward overall project and DOE goals (45%)
Score 3: Collaboration and coordination with other institutions (10%)
Score 4: Relevance/potential impact on DOE Program goals and RD&D objectives (15%)
Score 5: Proposed future work (10%)

For each project, individual reviewer scores for each of the five criteria were weighted using the formula in the box below to create a final score for each reviewer for that project. The average score for each project was then calculated by averaging the final scores for individual reviewers. The individual reviewer scores for each question were also averaged to provide information on the project's question-by-question scoring. In this manner, a project's final overall score can be meaningfully compared to that of another project.

Final Overall Score = [Score 1 x 0.20] + [Score 2 x 0.45] + [Score 3 x 0.10] + [Score 4 x 0.15] + [Score 5 x 0.10]

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

For the Market Transformation and Technology Validation sub-programs, scores were based on the following five criteria and weights:

Score 1: Relevance/potential impact on DOE Program goals and RD&D objectives (15%)
Score 2: Strategy for technical validation and/or deployment (20%)
Score 3: Accomplishments and progress toward overall project and DOE goals (45%)
Score 4: Collaboration and coordination with other institutions (10%)
Score 5: Proposed future work (10%)

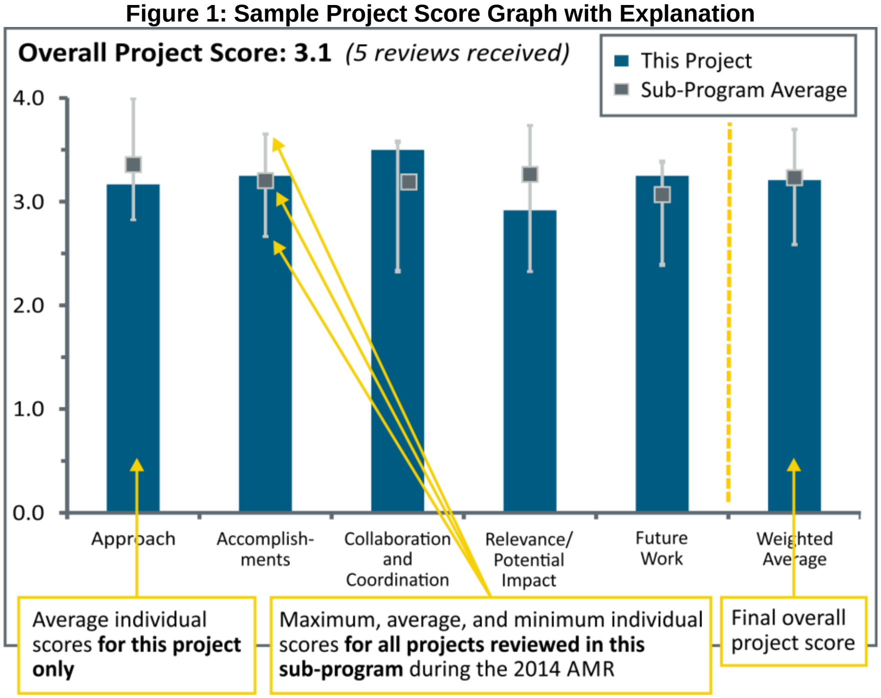
For all sub-programs, reviewers were also asked to provide qualitative comments regarding the five criteria, specific strengths and weaknesses of the project, and any recommendations relating to the work scope. These comments were also entered into the online, private database for easy retrieval and analysis.

Organization of the Report

The project comments and scores are grouped by sub-program area (Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Technology Validation; Safety, Codes and Standards; Market Transformation; and Systems Analysis) in order to align with the Fuel Cell Technologies Office's planning scheme. Each of these sections begins with a brief description of the general type of R&D or other activity being conducted. Next are the results of the reviews of each project presented at the 2014 AMR. The report also includes a summary of the qualitative comments for each project, as well as a graph showing the overall project score and a comparison of how each project aligns with all of the other projects in its sub-program area. A sample graph is provided in Figure 1.

Projects are compared based on a consistent set of criteria. Each project report includes a chart with bars representing that project's average scores for each of the five designated criteria. The gray vertical hash marks that overlay the blue bars represent the corresponding maximum, average, and minimum scores for all of the projects in the same sub-program.

Figure 1: Sample Project Score Graph with Explanation



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

Table 2: Sample Project Scores

	Approach (20%)	Accomplishments (45%)	Collaboration and Coordination (10%)	Relevance/ Potential Impact (15%)	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
Maximum	3.6	3.7	3.5	3.4	3.4
Average	3.3	3.2	3.1	3.0	3.1
Minimum	3.0	2.6	2.7	2.7	2.9

Using this data, the chart for Project A would contain five bars representing the values listed for that project in Table 2. A gray hash mark indicating the related maximum, minimum, and average values for all of the projects in Project A's sub-program area (the last three lines in the table above) would overlay each corresponding bar to facilitate comparison. In addition, each project's criteria scores would be weighted and combined to produce a final, overall project score that would permit meaningful comparisons to 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 0.45] + [3.3 x 0.10] + [3.2 x 0.15] + [3.1 x 0.10] = 3.3

2014 — Hydrogen Production and Delivery
Summary of Annual Merit Review of the Hydrogen Production and Delivery Sub-Program

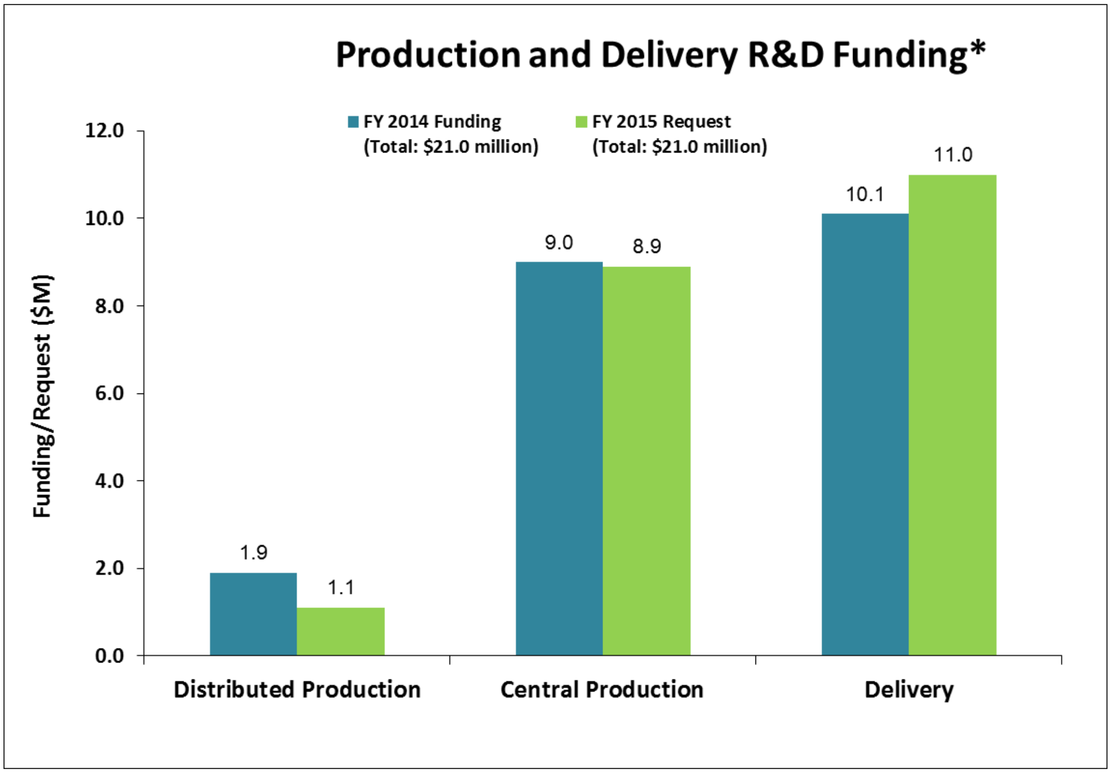
Summary of Reviewer Comments on the Hydrogen Production and Delivery Sub-Program:

This review session evaluated hydrogen production and delivery research and development (R&D) activities in the U.S. Department of Energy (DOE) Fuel Cell Technologies Office (FCTO) in the Office of Energy Efficiency and Renewable Energy. The hydrogen production projects reviewed represented a diverse portfolio of technologies to produce hydrogen from renewable energy sources. Production project sub-categories included water electrolysis, solar-driven thermochemical cycles, photoelectrochemical (PEC) direct water splitting, biological hydrogen production, and hydrogen production pathway analysis. The hydrogen delivery projects reviewed included R&D for low-cost pipeline materials, pipeline and forecourt compression, forecourt storage and dispensing components, and delivery cost analyses.

The reviewers recognized the Hydrogen Production and Delivery sub-program as focused, effective, well managed, and having a clear strategy to achieve DOE goals and objectives. Reviewers commented positively on the high quality of the R&D performed in the past year and the sub-program's engagement with industry. They encouraged continued coordination with the DOE Office of Science and the overall scientific community in leveraging hydrogen research, development, and demonstration (RD&D). They also emphasized the need for continued cost modeling of production and delivery technologies to identify and address cost barriers. In addition, reviewers stressed the need for balance between short-, mid-, and long-term technologies in the portfolios, and for more attention to near- and mid-term goals, targets, and deployments in order to meet the DOE cost goal in 2020.

Hydrogen Production and Delivery Funding:

The fiscal year (FY) 2014 appropriation for the Hydrogen Production and Delivery sub-program was $21 million. Funding was distributed approximately evenly between hydrogen production and hydrogen delivery, representing an increase in funding to delivery relative to past years, when funding was distributed with approximately two-thirds to production and one-third to delivery, and reflecting the current FCTO priority emphasis on hydrogen infrastructure technology development. Production funding is focused on long-term, renewable pathways such as PEC, biological, and solar-thermochemical hydrogen production. While this emphasis will continue in FY 2015 as part of the $21 million budget request, short- and mid-term technologies in production and delivery will be addressed through competitively selected new starts initiated in FY 2014. The delivery portfolio emphasis in FY 2014 was on reducing near-term technology costs, such as those associated with tube trailers and forecourt compressors, and on identifying additional low-cost early market delivery pathways that are viable. This emphasis will continue in FY 2015.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area.

Majority of Reviewer Comments and Recommendations:

Eighteen projects were reviewed, receiving primarily above-average to high scores (2.9–3.6), with an average score of 3.3. The scores are indicative of the technical progress that has been made over the past year.

Biological Hydrogen Production: Three projects in biological hydrogen production were reviewed, with an average score of 3.2. Projects in this area included efforts to improve the performance of algal and bacterial microorganisms that produce hydrogen through splitting water or fermentation of biomass. Reviewers noted that the projects have used logical, rational approaches and made progress in addressing barriers to hydrogen production from biological photolysis and microbial conversion of biomass. In particular, reviewers noted that the algal and cyanobacterial projects are complementary. Reviewers also noted that each project only addresses a portion of the challenges needed for the systems to become commercially viable, and they suggested that the project goals and results be more clearly framed in terms of the "bigger picture." Reviewers also expressed concern about the challenges to scaling up the systems to commercially viable sizes, especially given the complexity of some of the pathways involved.

Electrolysis: Three Small Business Innovation Research (SBIR) Phase II projects in the area of hydrogen production from water electrolysis were reviewed, receiving an average score of 3.2. Projects included efforts to decrease the platinum group metal (PGM) loading of the electrolysis cell electrodes while maintaining performance equivalent to higher-PGM electrodes. Two of the more promising approaches leveraged catalyst technologies originally developed for polymer electrolyte membrane (PEM) fuel cells. Also, one of the projects is focused on the development of catalysts and membranes for alkaline membrane electrolysis, which has the potential to reduce costs for low-temperature electrolyzers. Reviewers praised the progress made toward developing low-PGM, high-performing electrodes. However, reviewers noted that even with the significant reduction in PGM loadings achieved, the impact on the cost of hydrogen, via the resulting capital cost reduction, would be limited. To this end, reviewers suggested that performing Hydrogen Analysis (H2A) model cost analysis would be important for evaluating the ability of the proposed projects to reduce hydrogen production cost. Also, with the success in developing low-PGM electrodes with high performance, reviewers recommended that more emphasis be placed on durability testing.

Hydrogen Delivery: Six projects were reviewed in the area of hydrogen delivery, receiving an average score of 3.4. Projects were praised by reviewers for their technical approaches and relevance to DOE objectives. Recommendations were made for several projects to expand their economic analyses to ensure that all relevant aspects of mature markets are considered (e.g., the implications of high-volume manufacturing on electrochemical compression costs and the costs of man-ways in storage vessels). Other project-specific suggestions included materials testing (e.g., fluid dynamics testing of joints in fiber-reinforced pipelines) and the development of partnerships (e.g., collaboration with existing refueling station operators to ensure that dispensing hose designs account for real-world fueling conditions).

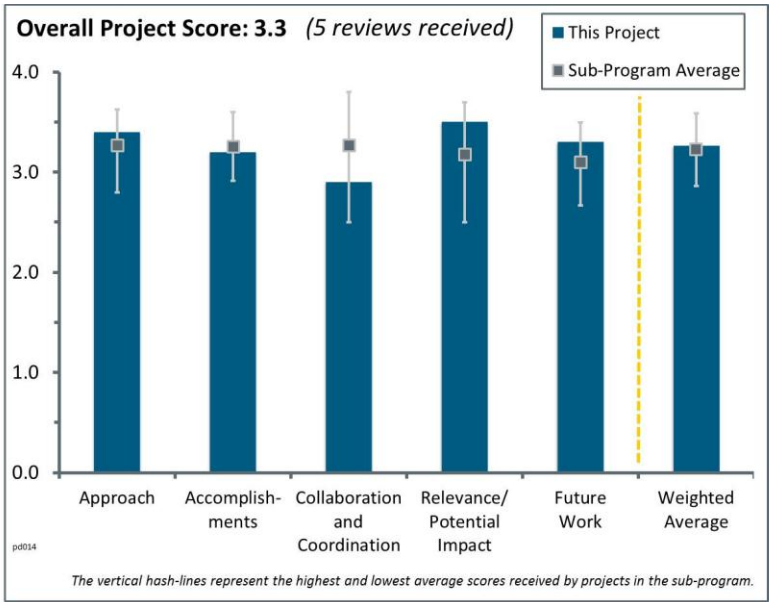
PEC Hydrogen Production: Two PEC projects were reviewed, receiving an average score of 3.5. Reviewers felt that projects in this area were well aligned with DOE objectives, with a focus on developing the most-promising PEC material systems and prototypes, such as those based on highly efficient III–V semiconductor materials. Projects were rated highly for advancing the state of the art in theoretical understanding and experimental development of PEC materials and interfaces. In particular, the coordination of theoretical model development with experimental validation work based on spectroscopic results was highly commended. Reviewers also highlighted the excellent collaborative successes of the projects involving the DOE PEC Working Group. Recommendations for future work included re-scoping the work to better match budgetary limits and further expanding collaborative efforts within DOE offices and across R&D agencies to better leverage synergistic resources. Ongoing R&D efforts related to this PEC materials and interface development work will continue through projects competitively selected in 2014.

Solar-Driven, High-Temperature (HT) Thermochemical Production: Presentations were given for three solar-driven, HT thermochemical hydrogen production projects—two addressing two-step, metal-oxide-based, HT reaction cycles, and one addressing a hybrid (multistep, including an electrolysis step) sulfur (HyS) reaction cycle. The projects received an average score of 3.0. Reviewers praised the innovative approaches and achievements in all three projects: the design of perovskite and hercynite reaction materials and the new reactor concepts for the HT cycles, and the membrane and electrocatalyst screening and test apparatus design and construction for the HyS cycle. Reviewers expressed concern about the complexity of the integrated reactions and reactors for all three systems, and they recommended that project emphasis be placed on materials RD&D to obtain the kinetics, durability, and other properties needed to achieve the hydrogen cost goal. Reviewers also recommended continued updating of technoeconomic analysis for the technologies, including realistic assessments of system original equipment manufacturer and capital costs. R&D efforts in these three HT reaction cycles will continue through projects competitively selected in 2014.

Hydrogen Production Pathway Analysis: One oral presentation was given in the area of hydrogen production pathway analysis. The project received a score of 3.1. Reviewers commended the project team's approach to developing analytical cases studies for PEM electrolysis, which involved gathering information on the state of the art from four electrolyzer companies. The results of the studies were seen as extremely useful, especially in terms of the capital cost breakdown and sensitivity analysis. The reviewers commented that the correlation between the project results and relevant DOE targets should be made clearer. Recommendations included a stronger focus on establishing and documenting specific quantifiable limits achievable through capital and operating cost improvements in hydrogen production pathways.

Project # PD-014: Hydrogen Delivery Infrastructure Analysis

Amgad Elgowainy; Argonne National Laboratory



Brief Summary of Project:

The main objective of this project is to provide a platform for comparing the impacts of alternative hydrogen delivery and refueling options on the cost of dispensed hydrogen. Cost drivers are identified for current hydrogen delivery and refueling technologies, and potential novel delivery concepts are evaluated. Cost modeling for hydrogen refueling stations evaluates high-pressure tube trailers and incorporates the implications of SAE J2601 refueling protocol.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- This project's approach is outstanding. While the team did not give clear priorities during last year's presentation, this year the priorities were clear and well defined. Through collaboration with industry, the project is focused on the critical barriers and challenges (i.e., lack of infrastructure, tube storage trailer delivery cost, and reliability) that need to be addressed to reach technical objectives along with cost, while managing the component designs. The path/approach looks feasible while supporting existing models and collaborating with industry for input and review.
- This project has a practical approach to evaluating fuel cell dispensing in light of economics. Delivery of analysis data was very effective.
- The development of models based on thermodynamics combined with real-world compression and fueling data has resulted in a rigorous analytical tool, which can be used to accurately predict the behavior of fueling systems.
- The approach, as described, is well thought out and properly addresses key barriers. The project is well organized and feasible. However, the efforts appear to be somewhat narrowly focused (e.g., high-pressure compressed gas as the only pathway for hydrogen delivery). Other pathways and tradeoffs should be at least mentioned and characterized. The tube trailer consolidation and cascade filling approach makes sense—it is known and practiced to some extent in industry—but space limitations at public fueling stations should be addressed in this context.
- It was unclear if shifting the cost upstream to tube trailers meant centralized production or if it referred to the cost of transport.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The project appears to be fully accomplishing its goal. Progress of the analysis should be faster, but this is probably being paced by funding.
- Development of modeling capabilities to optimize compressor size, storage, etc. is a major advancement for the DOE Hydrogen and Fuel Cells Program (the Program). For the first time, tools exist within the Program to optimize infrastructure components to match station demand profiles. Shifting high-capital-expenditure components to terminals to take advantage of economies of scale is a good strategy and an enabler for

hydrogen delivery. The tube trailer consolidation approach is a great way to minimize high-capital-expenditure compression capacity. High-pressure feed enables lower-cost (i.e., fewer stages) compressors.

- The progress toward defining refueling as being capital-intensive helps DOE define its goals and objectives. Information indicates that the compressor makes up 56% of refueling costs. This is a logical result of trying to move capital cost upstream to better share cost with other end users. The Program needs to continue looking at ways to optimize delivery pathways. Collaboration with other industries such as the U.S. Department of Defense (DOD) or the National Aeronautics and Space Administration (NASA) and their hydrogen programs, which were not discussed, is suggested. The project has very interesting plans of moving pressure toward delivery trailers. The project needs to work closer with the U.S. Department of Transportation (DOT) Pipeline and Hazardous Materials Safety Administration (PHMSA) Hazardous Materials group to communicate why the high-pressure trailer is needed. This effort may avoid future road blocks. The approach to track the mass, pressure, and temperature of each tube in a tube trailer is an interesting approach to match the delivery needs. A data-intensive balancing effort will be required.
- The stated task of simulation and optimization of the consolidation strategy is completed and well documented. However, a description of milestones and deliverables is not provided. Hence, the progress against targets cannot be judged. The consolidation strategy does provide a solution to achieve DOE goals for hydrogen delivery costs. The results and conclusions are anticipated; there are no surprises. A generic calculation program for public use for designing a cascade system for a given set of input and output conditions would be a useful outcome of the exercise. It would have been helpful to make reference to cost implications of the proposed solution in light of the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP) goals.
- Compressor investment is still needed, so it is unclear how this approach reduces investment cost. There is a tradeoff between additional investment for high-pressure tube trailers and the lower compressor capacity. Further evaluation is needed.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The principal investigator has successfully engaged vendors and other stakeholders to obtain data for developing and vetting models. Compressor manufacturers, tube trailer makers, etc. have all contributed to this project.
- The project presented data and experience from industry players. Although the number of collaborators was good, the project may gain additional benefit by seeking more input from equipment suppliers and installation owners.
- The project has good collaboration with industry, but better collaboration with other federal agencies still appears to be lacking. It is unclear what DOD is considering or modeling. NASA also has a hydrogen program, and this project appears to provide no insight as to what NASA is doing. The approach shown using the smaller tubes to store higher pressure was good.
- Collaboration with the Gas Technology Institute is well utilized; their contribution is visible. Collaboration with Pacific Northwest National Laboratory is not readily apparent. Input from industrial gas companies (IGCs) would be useful, as they routinely manage compressed hydrogen transport and delivery and are familiar with cascade filling strategies. No IGC is listed as a collaborator.
- Collaboration was not pointed out very much.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- This project is the only effort that quantifies the economics and tradeoffs associated with the creation of a hydrogen fuel cell electric vehicle program, especially a program that relies upon commercial investment prior to the public’s acceptance of the transportation mode.

- The project has excellent potential to address the cost of dispensing at the station while better utilizing trailing storage applications for early markets. Future cost work is needed for effects of trailers with more storage tubes (e.g., fittings, valves, and plumbing and control sensor control systems) and that impact on the overall cost of tube trailers. The project's very interesting approach aligns well with delivery objectives of the Production and Delivery program.
- High-quality modeling is an essential tool for the DOE Hydrogen and Fuel Cells Program (the Program). Without it, the Program is unable to identify critical processes and components in the production/delivery pipeline. Accurate models also enable cost-effective prioritization of Program activities.
- The work done and the output of this project do provide a useful framework and delivery infrastructure solutions. However, the results are limited to a specific case in this year's efforts. To fully address the goals and objectives delineated in the MYRDDP, the ongoing efforts should focus on providing cost tradeoffs and a broader comparison.

Question 5: Proposed future work

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

- The work planned is appropriate and logical, and it addresses all the necessary elements to provide a more comprehensive analysis and useful output.
- Critical barriers of future work to refine economics of fuel delivery and tradeoff analysis were discussed. The project appears to have excellent collaboration with industry partners.
- The project needs to evaluate the economics associated with tube trailers and tube size. It needs to clearly address redundancy of equipment to determine impact on project economics. The impact of supplying a compressor with products at various pressure levels (i.e., from tubes) is not clear.
- The project is technically focused, but it is unclear what the bigger picture is. It is unclear how this applies for all stations.

Project strengths:

- The analytical work is highly valuable and probably deserves additional funding to accelerate the determination of ideal solution(s).
- The project has excellent potential to address the cost of dispensing at the station while better utilizing trailer storage in the early market.
- The project demonstrates good understanding of issues, analysis capabilities, and access to necessary tools, data, and background work.
- Inclusion of added terminal costs is the logical next step and will help to assess the high-pressure tube trailer concept at a systems level.

Project weaknesses:

- The project has good collaboration with industry, but better collaboration with other federal agencies still appears to be lacking. It is unclear what DOD is considering or modeling. NASA also has a hydrogen program, and the project appears to provide no insight as to what NASA is doing. Future cost work is needed for effects of trailers with more storage tubes (e.g., fittings, valves, plumbing, and control sensor control systems) and that impact on the overall cost of tube trailers. The project needs to work closer with the DOT PHMSA Hazardous Materials group to communicate why the high-pressure trailer is needed. This effort may avoid future road blocks.
- The project has limited results (i.e., lacking cost data). It needs to incorporate practical aspects and narrow the focus.
- The project is progressing too slowly.

Recommendations for additions/deletions to project scope:

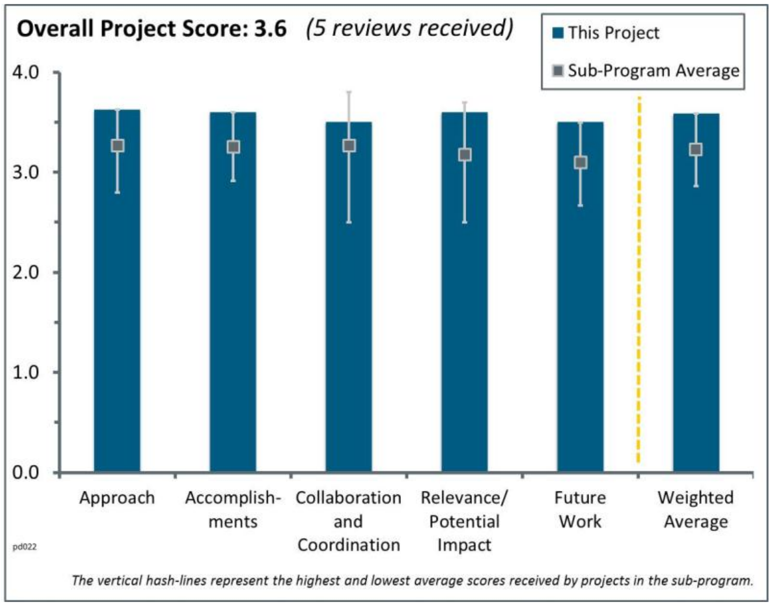
- Funding for this project should be increased to accelerate the work, and a communications strategy/plan should be created to disseminate the information to investors, operators, and the general public.

- The project needs to work more closely with the DOT PHMSA Hazardous Materials group to communicate why the high-pressure trailer is needed. This effort may avoid future road blocks. Better collaboration with other federal agencies still appears to be lacking. It is unclear what DOD is considering or modeling. NASA also has a hydrogen program, and the project appears to provide no insight as to what NASA is doing. The project has excellent potential to address the cost of dispensing at the station while better utilizing trailer storage.
- At some point, the costs of extra valving required for trailer consolidation need to be addressed.
- The project should include multiple pathway analysis and provide suggestions/challenges for potential new or modified pathways to enable reaching DOE cost targets.

Project # PD-022: Fiber-Reinforced Composite Pipelines

George Rawls; Savannah River National Laboratory

Brief Summary of Project:



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Composite pipeline technology has the potential to reduce installation costs and improve reliability for hydrogen pipelines. This project critically evaluates the current application of available fiber-reinforced pipeline (FRP) product standards and defines changes to the current FRP product standards to meet the ASME Code Methodology to provide the technical basis for using FRP in hydrogen service. The goal is to build a body of data to support codification in the ASME B31.12 Code Hydrogen Piping Code in 2015.

Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- Given the level of support and the complexity of the testing involved, the project's approach is optimum. One of the key objectives of the project is to develop information that can be used for the B31.12 codification of FRP pipelines by ASME. In this regard, the project's approach is based on fatigue testing of the FRPs in order to ascertain the life of the pipeline for a projected 50-year operation. Hence, the approach taken to develop the pressure versus cycles-to-failure data shown on slide 9 is most appropriate. In addition, the approach toward pipeline joint development seems to be sound, as there is a proposal for non-mechanical joints. It should be noted that ASME pipeline operators expressed reservations about mechanical joints.
- The project has done an excellent job of developing the information needed for standards incorporation.
- FRP technology provides reduction in number of joints compared to the current steel pipe technology. Mechanical, pH, burst strength, and fatigue life effects on FRP degradation so far show promising results.
- The team is addressing the project goals for providing a basis for the use of FRP as an alternative to steel pipeline and integrating FRP into pipeline code by 2015.
- The future needs for the pipelines are unclear. Variable sizes and lengths may be needed.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- The project accomplishments from last year are summarized on slide 5. These are accomplishments that address fundamental understanding of FRP degradation by hydrogen in the presence of a flaw. Certainly these accomplishments provided the basis to embark on work to study the conditions for extending the pipeline life from 20 to 50 years, which was this year's objective. An additional project accomplishment was the identification of the requirement for a 5% decrease in the fiber stress for the 20- to 50-year extension, which in turn set the pressure levels for testing toward 50-year design life. Given the refueling station demand for 36,500 cycles, the project results indicate safe operation up to 50 years at load ratios of 0.5. This is a key result of the project that is truly significant. Another important result of the project is the finding that the life of the FRP depends on the load ratio. The investigation of this dependence can be taken on in the future. Lastly, the project concluded that increased hardness remediates the extrusion failures of the O-ring in the FRP connectors, and, in response to ASME's pipeline operators' concerns over the use of mechanical joints, the project investigators came up with three new concepts for fiber-reinforced composite pipelines.

- All of the work is aimed directly at providing a basis for the ASME code. The work shows flexibility in successfully dealing with the desire by the gas supplier to increase design life (and value) from 20 to 50 years. The identification and parallel strategies for resolving the challenges with the FRP connectors are right on task. The codification status (i.e., review stage of the technical codification report) will potentially result in an early achievement of the 2015 milestone around code development.
- A good process is involved. It could be significantly improved by performing the tests with pressurized hydrogen, which is the ultimate use of these results. Over the long run, hydrogen diffusion through the pipe layers is certainly possible and could change the material response. This is a critical element that is missing from the research.
- FRP technology has addressed DOE technical barriers; however, no data on detail cost saving were presented.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- Collaboration with the ASME is very important. ASME's B31.12 panel is an authoritative body that checks the soundness of the project results toward codification. In addition, the University of Hawaii is involved with the engineering aspects of the pipeline installation. The project is also collaborating with Oak Ridge National Laboratory on economic analysis, but no results were presented.
- There is good technical collaboration, but it was not explicitly indicated which standards committees are bring used and especially what the timeline is for introducing this to the committees and achieving approval.
- Comments from the previous year noted that the team is working with only one company's product, and this seems to continue to be the trend, both with manufacturers and with other institutions. Collaboration is not always necessary, but there seems to be resistance, even upon suggestion to ensure the team is open to a variety of viewpoints. Increased collaboration with academics and pipeline manufacturers would only benefit the project.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- The use of FRP pipelines is a game changer for cost reduction and for installation in situations where steel pipeline is not applicable.
- Codification of the FRPs for hydrogen transport is a key step for hydrogen delivery to refueling stations. Codified FRPs can be used for hydrogen distribution at a reduced cost.
- FRP technology provides reduction in the number of joints compared to the current steel pipe technology.
- This is very relevant but would be greatly enhanced by testing under more realistic conditions using pressurized hydrogen. A lot of the focus was on pressure changes (i.e., R-ratio), but the speaker indicated that a constant pressurization, or at least less cycling, may be more realistic. As a result, diffusion of even small amounts of hydrogen under static, elevated pressure could change the material response.

Question 5: Proposed future work

This project was rated **3.5** for its proposed future work.

- The proposed work is the work necessary to complete the project.
- Proposed evaluation of the FRP non-mechanical joints is necessary and important. Further work on the variability of fatigue data and the load ratio dependence of fatigue life is also very important. It is stated on

slide 17 that the effect of cycle frequency and variability in the fatigue data will be done in collaboration with Fiberspar. It is strongly recommended that this investigation be carried out independently from Fiberspar. For codification, it is also very important that the Savannah River National Laboratory investigation be carried out independently of Fiberspar. Evaluation of the rupture-stress versus time relationship shown on slide 7 in the presence of hydrogen is very important regarding the estimation of the required decrease of the fiber fracture stress for the 50-year life extension. Currently, the plot of slide 7 does not account for the hydrogen effect on the fiber toughness.

- Future work is on track to meet DOE goals, except there are no efforts on cost analyses.
- Including pressurized hydrogen is recommended.
- Cost analyzes, big picture, etc. are missing. The work should be put into context.

Project strengths:

- This is very important work for the long-term development for hydrogen technology.
- The project has an excellent team and process.
- This project addresses the performance of FRPs toward codification for 50-year operation. This is done in an engineering way through cycle life assessment. The three new non-mechanical joint concepts may turn out to be valuable alternatives to the mechanical joints that rely on the use of O-rings.
- There is a clear path to success, and the project appears to be positioned to complete the work in a timely fashion.
- FRP technology provides a reduction in the number of joints compared to the current steel pipe technology. Mechanical, pH, burst strength, and fatigue life effects on FRP degradation so far are show promising results.

Project weaknesses:

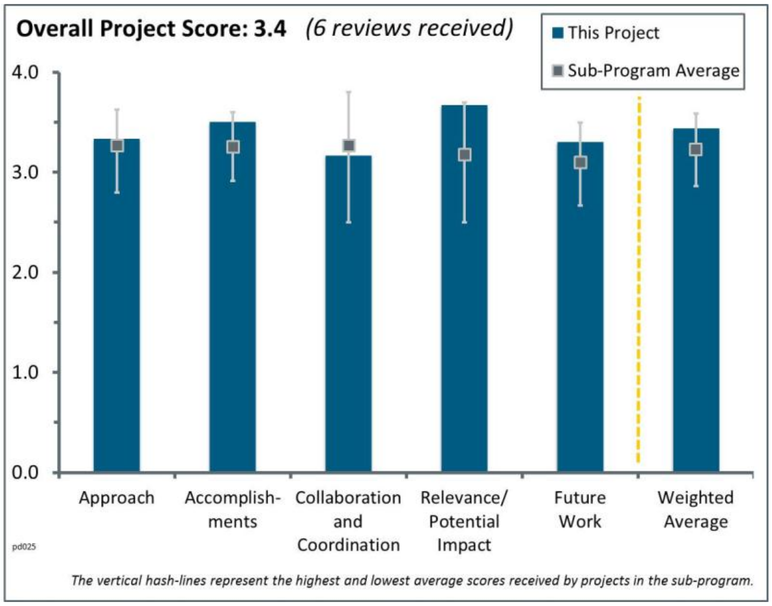
- Some of the proposed joint concepts reduce the internal diameter of the pipe. Choked flow is one of the major effects. (Choked flow is a fluid dynamic condition associated with the Venturi effect.) Additional analyses are needed for each proposed joint concept.
- The data charted in the ASTM D2992 data set for FRP for the data point at >10,000 hr (slide 7) look to be significantly different from the rest of the data, and there is no statistical analysis. There was also quite a bit of discussion during the presentation about the chart in slide 9. It is not clear if there are enough data to support the conclusion on the effect of the R-ratio.
- The project should use pressurized hydrogen. Even small amounts of diffusion over time will change the polymers response. The project has tested the glass fibers under a hydrogen environment but neglected the changes that may occur in the polymer and interfacial properties.
- The project is “too engineering” in nature. Fundamental understanding of the fatigue failure of the FRPs is missing. The load ratio dependence indicates a delta-sigma effect, which is similar to the effect prevalent in structural metallic alloys. If such understanding is pursued, perhaps the fatigue life extension to 50 years will be done in a safe way predicated on true mechanistic understanding instead of performance-based mechanical engineering, which is the current project’s approach.

Recommendations for additions/deletions to project scope:

- Additional funding is required for the project to address fundamental fatigue issues. FRPs are such a promising technology for hydrogen transport that they deserve attention and full certification from a fundamental fracture mechanic’s standpoint. The director of the project, George Rawls, understands the issues well and is capable of expanding in this direction, if provided with additional support. The project should dissociate from the FRP manufacturer so that a fully independent assessment of the fatigue life of the FRP pipeline is obtained.
- Additional fluid dynamic analyses are needed for each proposed joint concept.
- Hydrogen testing should be added.

Project # PD-025: Hydrogen Embrittlement of Structural Steels

Brian Somerday; Sandia National Laboratories



Brief Summary of Project:

The purpose of this project is to demonstrate reliability and integrity of steel hydrogen pipelines for cyclic pressure applications. Steel pipeline has been proven for hydrogen delivery under constant pressure, but this project addresses the potential for fatigue crack growth due to hydrogen embrittlement and susceptibility of welds to cracking under cyclic pressure. The project will establish microstructure-performance relationships that will allow steel pipelines to be viable for hydrogen delivery.

Question 1: Approach to performing the work

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

- This is a good study on the weaknesses of steel pipe embrittlement with hydrogen. This has a good movement to study of welds as failure sources. Analysis depends upon fatigue crack growth laws. Perhaps the project should also look at weld failures and techniques for detecting them that are better than the methods used today.
- The presenter addressed barriers focused on safety related to steel pipelines. The project team looked at the welds more susceptible to failures than base metals (based on ASME B31.12 code). They applied basic research to test X65 samples of pipe sections to determine whether there are differences between base metals and the heat-affected zones. Barriers are clear and defined. Targets and testing protocols are well designed. The scope of work is feasible, and several objectives are integrated within tasks.
- Safety, reliability/integrity, and weld susceptibility to hydrogen-accelerated fatigue crack growth in hydrogen steel pipe were recognized and addressed.
- The project is a continuation of recognized work.
- To get the pipe in the ground, data and models on base material and welds are needed to ensure safety and to develop codes and standards. To reduce cost, a better understanding of performance will enable the appropriate assignment of safety factors to reduce waste due to overly conservative design and to implement new pipeline materials. The project appears to be reasonably well integrated with other efforts but should continue to move more in that direction. The approach must include modeling to be successful.
- The stress rate was constant during both increasing and decreasing stress as shown in the presentation (i.e., a linear saw tooth profile). It is not clear whether the stress rate used for the testing is reflective of the anticipated stress rate. It is unclear whether the crack growth rate is affected by the stress rate. The test conditions to answer the question of crack growth rate should reflect the anticipated use conditions. The future work does not include efforts to address either the ferrite and pearlite effect on crack growth or the orientation of the microstructure relative to the load orientation on crack growth, both of which, according to the presentation, show substantially greater importance than any weld effects. The factors that are most impactful to the crack growth are the most important to address in the codes and standards surrounding the use of steel pipe for hydrogen transmission. The presentation did not address the cost of installation. Because the project has ended, this barrier was not addressed.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- The project focused on crack growth from embrittlement and was targeting fatigue growth laws to apply to general conditions. There was a focus on girth weld on carbon steel pipe. This research is vital to existing pipeline companies that deliver hydrogen and future infrastructure for fuel cell applications.
- The focus of the project in the past year was to measure fatigue crack growth laws for hydrogen gas for pipeline steel girth welds. The task was challenging because of the complex geometry, stress state, and gradient of material and microstructure associated with pipeline welds. By applying a creative yet sound scientific approach, the team was able to overcome challenges that confounded the classic measurement method and was able to successfully and reliably measure the susceptibility of the base metal, heat-affected zone, and fusion zone to enhanced fatigue crack growth in the presence of hydrogen. The team also identified that the brute force approach was not enough to solve this complex problem and that a microstructural-based modeling approach is needed to resolve the enormous challenge of reducing the cost of pipeline delivery of hydrogen. Of particular interest was the work to understand the orientation dependence of the performance of X65 base metal, which provides an additional argument for a microstructure-based modeling approach to removing technical barriers.
- Good methodical progress has been made through the issues associated with hydrogen embrittlement. The project used a novel approach to look at fusion zone crack growth rates by reorienting the specimen.
- Active partners are identified and included with sufficient budget to ensure work is completed on time and within funding limits. Results indicate that orientation of base metal shows macrostructure effects. More information is expected from the remaining work.
- Steel pipe technology has addressed most of the DOE technical barriers; however, no data on detailed cost saving was presented.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The project shows dedicated effort to revitalize U.S. government relationships, international connections (i.e., Japan), and industry. The project is working with the National Institute of Standards and Technology (NIST) and numerous others. More effort should go into collaborating with current pipeline companies.
- The team expressed the development of a coordination plan to take up the work of the now defunct Hydrogen Pipeline Working Group. The new group, which has apparently been co-authoring a white paper to address the technical barriers associated with “Safety, Codes and Standards, Permitting” and the “High As-Installed Cost of Pipelines” (slide 2), consists of representation from federal laboratories, academia, industry, and standards development organizations. Coordinated and collaborative research in this area is absolutely necessary to achieve the DOE cost-point goals in the desired timeframe because of the limited number of hydrogen test facilities available to produce data. While it is too early to judge the quality and execution of the plan detailed in the white paper, it is reassuring to see a real desire expressed by the principal investigator to collaboratively address the barriers. It was also good to see individual examples of collaboration as demonstrated by the inclusion of such work in the presentation. While it is understandable that a team cannot work with every stakeholder, it was surprising to see only one partner each from industry and academia listed in the presentation. The project could benefit from a wider variety of stakeholder partnerships.
- Industry partners and other institutions are identified collaborators with the work scope and expected outcomes.
- The project has adequate collaboration with others. Perhaps they should be looking at other pipe samples besides those from ExxonMobil.
- There was not any report of the work performed by the International Institute of Carbon-Neutral Energy Research, and apparently no friction stir welded pipe was supplied by Oak Ridge National Laboratory (ORNL).

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- Safely light-weighting steel pipeline is a direct cost savings measure. Understanding the behavior of welds for various material types is critical for the safe implementation of new materials. This work directly supports the development of the codes and standards that will enable the safe implementation of new pipeline materials at significant cost savings.
- Results will be used to quantify the ability to protect pipe materials against fatigue crack growth relationships, which is a safety/reliability issue that needs to be understood to meet the DOE Hydrogen and Fuel Cells Program (the Program) goals and objectives.
- The project is very relevant because hydrogen embrittlement is a serious problem with steel pipelines for hydrogen transport.
- Issues regarding the use of steel transmission pipe are highly relevant and of enormous potential impact to the cost and ability to transmit hydrogen through pipelines.
- The work appears to complement work performed by NIST to increase loading on hydrogen pipelines.
- Steel pipe technology has been around for a long time, and some of the technical challenges are well understood.

Question 5: Proposed future work

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

- The proposed future work basically consists of two components: (1) providing a fundamental understanding of fatigue crack growth mechanisms as they relate to the material (the mechanisms would then be leveraged to develop predictive models to reduce testing and data needs); and (2) the use of the learning from the complete body of work in this area to predict pipe wall thicknesses, ultimately translating the science into application. These are clearly the next steps and are the ones needed to deal with all new base metal and welds in new pipelines.
- Future work is on track to meet DOE goals, except there are no efforts on cost analyses.
- The work clearly showed that welds are not the most impactful issues regarding steel piping. The future work should address the pearlite and ferrite effects as well as the orientation of the microstructure. The future work should also address lowering the installed cost, which was not addressed in the project.
- The project goal is to identify macrostructure performance and the material relationships to ensure safe and reliable transport of hydrogen and the best cost-effective means. Future work needs to consider alternative welding approaches under consideration by the pipeline industry (e.g., friction stir welding). The project needs to have models developed that will calculate wall thickness based on realistic operation and inspection parameters.
- The project should show how this information will be used to calculate steel pipe thickness for given conditions for hydrogen transport.
- There is not much detail in the next steps.

Project strengths:

- The team has a clear path to success based on a fundamental understanding of the problem and a deep and thorough understanding of the scientific challenges associated with the problem. Also, the strong collaborative nature of the team is refreshing in an area where other groups seem less focused on actually solving the problem.
- This work is probably vital to NIST and Sandia National Laboratories (SNL). It is filling a void that may not be performed by commercial organizations.
- SNL has a strength in addressing the material-related issues related to hydrogen embrittlement.
- This is a good fundamental approach to the cracking of pipe.

- The remaining challenge is the long-term effects of compression cycling across the large range of current pipeline system materials with temperature changes and modeling required to simulate results.
- Steel pipe technology has address most of the DOE technical barriers; however, no data on detail cost saving was presented.

Project weaknesses:

- No weaknesses were identified.
- So far all the efforts were on addressing fatigue crack growth measurements for pipeline steel friction stir weld in hydrogen gas. In the previous year it was indicated that "there is a framework for calculating the steel pipeline wall thickness required to satisfy the inspection interval based on the measured fatigue crack growth rates in hydrogen gas. Solidifying the wall thickness calculation will allow more definitive assessments of steel pipeline costs." However, a detailed cost framework can be developed in parallel.
- The project did not address the cost of installation issues. A more complete sampling of steel pipe from multiple vendors would have improved this project.
- The project should show how this information will be used to design better pipe for hydrogen transport and should test other hydrogen-exposed pipe.
- While it is clear that the team is headed in the direction needed to address the barriers, it is unclear whether there is sufficient testing infrastructure (i.e., hydrogen test facilities) to perform the number of evaluations necessary to develop and validate the models to safely implement the desired cost savings. Even when taking into account the other facilities that are collaborating with the team, it is unclear that the goals will be achieved without additional test facilities. Outreach, partnering, and education of stakeholders are critical for the adoption of the scientifically based codes and standards that will ultimately realize the maximum savings achievable through this work. Unless pipeline owners and operators feel comfortable with the recommendations (e.g., thinner pipe walls), pipe will not go in the ground, and the savings will not be realized. It is unclear whether pipeline owners and operators are involved in the work.
- There is no collaboration with corporate research and development organizations. It is unclear whether the project team is doing enough to determine the usefulness of research for industries. There is a noted need for relationships with industrial gas organizations.

Recommendations for additions/deletions to project scope:

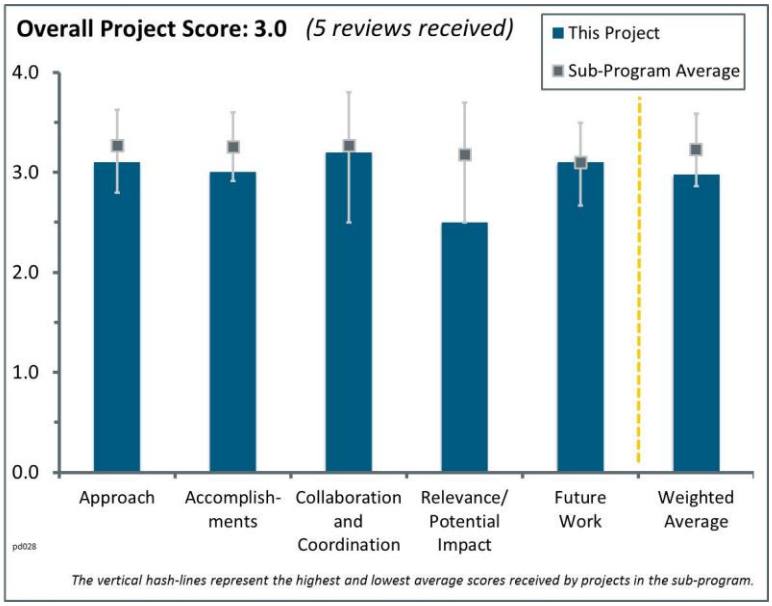
- More testing on fatigue crack growth measurements is required. In parallel, detailed cost analyses are in order.
- If the project is to include addressing the cost of installation, then there should be stronger involvement from a collaborating partner that actually installs pipeline.
- There is a need to have models developed that will calculate wall thickness based on realistic operation and inspection parameters.
- The objective of the project must be determined. It is not clear whether the project is a high-level analytical study to write papers or whether there is a practical objective to make a real impact on existing and future gaseous hydrogen pipelines.
- The project should show how this information will be used for design of better pipes for hydrogen transport; test the fatigue crack growth rates in steel pipe exposed to hydrogen from other sources; look at base metal, heat-affected zone, and weld zone; and look at girth weld with ORNL friction stir welded pipeline steel as planned.

Project # PD-028: Solarthermal Redox-Based Water Splitting Cycles

Al Weimer; University of Colorado

Brief Summary of Project:

The overall objectives of this project are to develop efficient, robust material and operation methods for a two-step thermochemical reduction/oxidation (redox) cycle and to develop a scalable solar-thermal reactor design that will achieve the U.S. Department of Energy (DOE) cost targets for solar hydrogen. Specifically during this year, the goal has been to develop an understanding of hercynite cycle chemistry, multi-tube reactor performance, and redox behavior. Development has also been under way for continuous particle flow reactor and materials concepts with independently controllable redox conditions.



Question 1: Approach to performing the work

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

- The approach is sound, and it is obvious that the researchers are completely engaged and enthusiastic about the work. Work is focused on critical areas and meets the reviewers' comments from 2013, with which the reviewer disagrees in some respects. The idea that spherical particles could improve durability is reasonable, though more development is needed.
- The development of the pseudo-isothermal hercynite cycle is an excellent example of an innovative design to overcome some thermochemical barriers. The founding analysis of efficiency of the pseudo-isothermal cycle is outstanding. The approach to this work is excellent. Barriers to anticipated performance are identified, but the current and future work needs more focus on the most important barriers. Performance is critically dependent on heat recuperation and is so identified in the presentation, but a focused design effort to determine gas–gas recuperation efficiency is not apparent in the presentation. Long-term operational stability depends on maintenance of active particle characteristics with minimal attrition and accretion. These are frequent problematic issues accompanying gas-entrained particle transport systems, and the proposed work does not clearly demonstrate an assessment and mitigation approach to assuring robust long-term operation. The design concept for a particle flow solar thermal reactor is innovative but lacks detail and modeling necessary for assuring transport performance, requisite residence periods, and heat recuperation performance.
- Moving solid materials is extremely difficult, especially at high temperatures and low pressures. Alternatives need to be considered. Examining 1350ºC active materials means the reactor will need to be hotter. This will make the construction harder and will require exotic materials, increasing costs. Materials that operate at lower temperatures are needed, and more focus should be given to their development. How the materials will be moved will increase material degradation. The presenter talked about including binders, etc. This may decrease the degradation, but now less active material will be available for reaction, thus increasing the amount of material that needs to be moved.
- This project focuses on development of innovative solar thermochemical water splitting processes based on metal-oxide redox cycles in general, and a cobalt ferrite/hercynite cycle in particular. The work encompasses fundamental understanding of redox materials and different types of redox cycling (e.g., isothermal and temperature-swing). Isothermal operation is not possible for a pure thermal water-splitting process. The project's isothermal operation is accomplished by cyclically varying the steam composition

for the oxidation and reduction reactions by sweeping the steam from the reactor (using He) prior to the reduction reaction, then reintroducing it for the oxidation reaction. Clearly this is not a pure thermal water-splitting process, and the governing thermodynamics are different and still being worked out. The specifics of the cyclic operation were not clear from the presentation. It is clarified in the technical reference listed in the presentation.

- Generally and normally, progressing fundamental understanding, materials, and process in parallel, as the investigators are doing in this project, is viewed favorably because these aspects are integrated, influence one another, and are needed to assess economics. But in this case it is fairly clear that the hydrogen productivity and kinetics of the materials are the current key limiting factor towards the ultimate objective of efficient and cost-effective hydrogen production. Materials with much higher performance (i.e., likely >10x, possibly 100x) are needed for that, and discovering them should be prioritized if economically feasible hydrogen production is the ultimate objective. However, compared to 2013, there seemed to be much more emphasis on process than on materials. The reason is not clear, but that does not seem to be the more promising path to achieve a practical technology.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- Accomplishments and progress are excellent. The identification of the pseudo-isothermal cycle and its quantitative demonstration assure potential success for thermochemical performance of this concept. Detailed design of integration with a solar interface and analytic reactor performance modeling remain to be addressed so that the capability for this cycle to meet long-term DOE goals can be demonstrated.
- Good progress has been made in analyzing the process and identifying impacts on efficiency, such as heat recuperation, the effect of O_2 removal, and the differences in reduction temperature versus oxidation temperature. Also, the project team came up with an alternative reactor design to overcome limitations of prior designs. However, the technical information on the new reactor presented at the review and provided in the reviewers' files was sufficient to understand what drove the choices and how the new design is potentially better, but not enough to assess the merits of the new design. Maybe the new concept is very recent, and work is in progress. The new design seems related to (or possibly inspired by) fluid catalytic cracker (FCC) reactors—there may be learnings from FCC reactors that could be used in this project, although the materials and conditions are different. Many process hurdles remain, of course, and the new design would have to be confirmed by experiments—likely at first by separate testing of certain critical process portions before an integrated system. But it seems that the materials are currently the critical limitations.
- The researchers have demonstrated isothermal operation, with oxidation and reduction occurring at the same temperature, with different partial pressures of steam during the reduction (i.e., low P_{H2O}) and oxidation (i.e., high P_{H2O}) steps. However, to avoid simultaneous hydrogen and O_2 production and to improve cycle performance, high reduction temperatures (i.e., 1500ºC) and lower oxidation temperatures (i.e., 1350ºC) can be used. This "pseudo-isothermal" version of the process has also been demonstrated at small scale. The researchers' analysis has shown that, as with many of these processes, in order to achieve the highest overall efficiency, heat recuperation is critical. In this case, the steam/hydrogen gas mixture leaving the reactor must be used to effectively preheat the steam flowing into the reactor. In addition, heat recuperation between the oxidized solids and the reduced solids is needed. A ceramic heat exchanger will be needed for temperatures above 1000ºC. Results of a thermal analysis on a multi-tube reactor design were presented. Also presented was a solar thermal particle reactor concept in which the oxidation and reduction processes occur in different vessels, with redox particle circulation between the two. In this system, solar heat is directed at the reduction vessel where O_2 is produced. The reduced particles are then introduced to the oxidation vessel where hydrogen is produced. The redox support particles can be formed by a spray drying process. An economic analysis was also presented.
- Interesting density functional theory analysis was done. The presenter stated tests were done that suggest the reaction mechanism they proposed was correct. However, it was unclear what the evidence was and what tests were done. Moving from a particle flow reactor from the old reactor design may help efficiencies, but it will increase the system complexity. There is good analysis on the cycle life. Now the needed durability is known. It is unclear where the tests for this are. The active material needs to have long-

term testing to verify that it will work. In the new reactor design, the materials will be at high temperatures, reacting and bumping into each other. This will be similar to sandpaper that will rub the material, causing degradation. Doing the cycle testing in a stationary system will not be sufficient; it will now need to be done in a moving system that simulates the reactor conditions. The Hydrogen Analysis (H2A) economic analysis' capital investment costs seem very low. The other assumptions are not listed, so it is hard to tell the reasonableness of the analysis. For example, it is unclear how much spray processing of the materials will cost. The reaction vessels in the new designs may have relatively high operation and maintenance (O&M) costs, especially considering the fluid bed reactor design that was proposed. This needs to be considered.

- New reactor design offers improvements. The improved yields under pseudo-isothermal conditions are encouraging. The amount of active material is a concern. According to the presentation, 1 g of active material yields 0.0002 mol or 0.0004 g of hydrogen. This yield is very low for a commercial process that operates under sunlight. Another concern in this project is the durability of the particles. The use of results from chemical looping combustors (CLCs) may not be appropriate for this project. The reactions are quite different. In the CLC, a metal oxide is used to oxidize C or CO, and the metal oxide is reduced to the metal. The second bed then reoxidizes the metal. The reaction in a CLC has a displacement mechanism, not an O-vacancy mechanism. The potential for attrition of the moving particles appears high, especially at 1500ºC–1350ºC. X-ray diffraction (XRD) is not the "best" technique for assessing particle durability. XRD is not considered sensitive to materials present at 5%–10%. The use of intensity as a measure of sample integrity is very difficult because exactly the same amount of material has to be present in all samples that are being compared. Fines may or may not have the same crystalline structure, and it is expected that the number of fines will change with time. The summary statement that material costs cannot meet the target cost indicates that more work should be focused on establishing the material itself and its durability and cost. Defining symbols and providing yields in understandable units was appreciated.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- Collaboration and coordination are good. The National Renewable Energy Laboratory (NREL) provides excellent sources of experimental facilities and solar design capabilities. Sandia National Laboratories (SNL) has outstanding system design and analysis capabilities, but this institution is focused on its own competing reactor concept and is unlikely to provide significant support to the design and modeling effort essential to this project. ETH Zurich can and likely does provide considerable active particle assessment. The project would benefit enormously from adding additional collaborative effort in reactor system design and modeling, as well as solar system and interface design work. The presentation mentioned collaboration with "ANU," but this institution was not identified.
- Students are afforded the opportunity of working at NREL, SNL, and ETH Zurich. The potential for cross-fertilization of ideas is high.
- This project was led by the University of Colorado Boulder (CU-Boulder), in collaboration with NREL, SNL, Lawrence Livermore National Laboratory (LLNL), and ETH Zurich. The project supported a large number of graduate students.
- This was lightly touched on during the review, but the partners seem to be working well together. It is not clear who is responsible for new materials innovation and synthesis. Perhaps it is CU-Boulder. Expanding effort and collaboration in this area to accelerate materials innovation should be considered.
- It seems that most of the collaboration involved using others' facilities or asking questions. Increased collaboration would be good for progress. There does not seem to be anyone on the team with practical experience in building commercial systems. There is a university and two national laboratories. A partner with industrial experience would be a good addition.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.5** for its relevance/potential impact.

- This project provides outstanding support to progress toward DOE Hydrogen and Fuel Cells Program (the Program) goals via the solar thermochemical hydrogen production pathway. Solar-to-hydrogen efficiency potential is high, providing some promise of reduced solar capital cost if heat recuperation can be demonstrated. Concern regarding durability of the active material needs to be addressed as well. The project should be continued with some change in project priorities and with additional collaborative support.
- This work is generally relevant to the overall objectives of the Program. However, this project is an example of high-risk, long-term research that has a low probability of ever being practical. There are so many technical challenges, many of which are acknowledged in the presentation: durability of the redox particles (both in terms of mechanical durability associated with particle transport and in terms of redox effectiveness over a large number of cycles), solar receiver design (i.e., materials and scale-up), heat recuperation at very high temperature, etc. In addition to the numerous engineering and materials issues associated with this concept, the ultimate potential for large-scale deployment of this technology is minimal. Even if the discussion is strictly limited to consideration of purely solar technologies, photovoltaic-water electrolysis is currently available off the shelf with solar-to-hydrogen efficiency of at least 18%. High-temperature steam electrolysis, which is at a high level of development, powered by photovoltaics, and thermally integrated to concentrated solar thermal energy, can achieve a solar-to-hydrogen efficiency of at least 30%.
- The project is extremely ambitious, as there are development challenges for nearly all aspects of the project. The costs associated with the solar field and the reactors are very high. In theory, this project meets the DOE goals, but the challenges are overwhelmingly high. The relevance of the project cannot be properly assessed until particles with the desired redox properties and necessary durability and cost are identified. Preliminary H2A analysis is promising but not complete—the summary statement specifies that tower and materials cost targets have not been met. Materials of construction are still under development and therefore not included in the cost. If atomic layer deposition is required to prepare containment material of varying compositions, costs could be very high. But it is expected that the costs of the solar field and the reactors will dwarf all other costs.
- This is a long-term development area for DOE. The reactor design is extremely complex. Having multiple reactors on the power tower will mean that should one reactor fail, then all of the reactors will need to be shut down.
- As it stands today, the project has poor prospects of meeting the goal of efficient and cost-effective hydrogen production. Here are some broad numbers to illustrate the reason. Given the productivity and kinetics of the current materials, the reviewer estimates to achieve the target of 50 ton/day hydrogen production as mentioned in the review material, the reactor will need to circulate a few hundred tons per minute of solids and have a solid inventory of over a 1,000 tons (solar intermittency is included in these calculations). These numbers are several times those of the largest FCC reactor the reviewer knows of and are likely to require close to $1 billion for just the reactor. (The reviewer used the FCC because it is a solid circulating reactor, with established technology and economics. Also, energy storage solutions will save reactor cost but increase capital requirements for other units.) A number of smaller reactors could be built, but that increases cost by negating economies of scale. These considerations, not even including the cost of the solar tower(s) and the rest of the plant or the challenge of building such a massive reactor(s), led to the conclusion that unless much more productive materials are found, the prospects for economically viable hydrogen production using this technology are poor. For comparison, a 50-ton-hydrogen-per-day steam methane reformer (SMR) plant (quite small compared to a world-scale plant) would likely cost around $100 million. Also, CO_2 capture has been demonstrated at scale with an SMR (e.g., Air Products and Chemicals, Inc., in Port Arthur, Texas). Also, it is not clear what the total capital investment figure of $70 million reported on chart 15 include, but it seems to be much too low an estimate.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- Proposed future work includes evaluation of redox material and particle stability, continued screening of potential redox materials, improved reactor design, and more detailed techno-economic modeling. These tasks address the major development issues.
- It is good to see increased collaborations are planned. It is good to see that durability tests are included. The design of these experiments must take into consideration the reactor operating conditions (i.e., temperature, pressure, reactions occurring, and high material flow rate). It is strongly recommended that someone with industrial experience in fluidized bed reactors be consulted or included in the experimental design to ensure all considerations are taken into account. The H2A needs to be updated, and it needs to be transparent on the capital costs, operations and maintenance costs, etc. Materials that operate at lower temperatures, have faster kinetics, etc. should be developed.
- The plan has the correct elements but should focus most resources on assessing whether much better materials can be found. More effort in fundamental understanding, synthesis, and testing of materials will be needed and is recommended. The economic basis should be reevaluated and strengthened.
- Proposed future work addresses the critical deficiencies with some lack of clarity in work priorities. For example, the viability of the proposed cycle depends critically on heat exchange in flowing solid particle media and gas–gas recuperation, features that can only be extracted from a detailed concept design with modeling and analysis effort. Some mention of computational fluid dynamic modeling is referenced in the presentation, but a good deal more system design, along with thermodynamic and fluid dynamic modeling, must be done before substantial assessment of heat recuperation will be possible.
- The project is focused on identifying the “best” materials for two-step solar water splitting, which is critical. The redox material represents a potential showstopper.

Project strengths:

- The project demonstrates really interesting and good science and engineering. The execution seems solid. Collaborations seem complementary and effective.
- This is an innovative and potentially promising concept that has demonstrated significant potential to meet long-term Program goals. Available facilities are good to excellent, and project personnel capabilities are good.
- This is innovative, high-quality work, incorporating aspects of fundamental materials science, thermodynamics, and challenging engineering design. The effort is highly collaborative and supportive of many graduate students. Pseudo-isothermal hydrogen and oxygen evolution were demonstrated experimentally at high temperatures.
- It is a very interesting academic study.

Project weaknesses:

- More emphasis should be placed on experimental performance validation, moving toward demonstration of a fully integrated system.
- This project uses a challenging route to the goal of cost-effective hydrogen production. Current materials have low hydrogen productivity and no new leads reported. Solar intermittency increases cost substantially for any given production rate compared to continuous processes, such as SMR. It is not clear how energy storage would help much without new materials, but it would be interesting to see the economic analysis.
- The project is conducted in a university environment with project tasks tailored to meet student and academic requirements in lieu of project programmatic requirements. This deficiency can be mitigated through establishment of significant additional collaborative/teaming strategy to address critical project programmatic efforts that are not aligned with student priorities and/or capabilities.
- The current design scheme requires a large amount of materials to be moved at high temperatures and low pressures. The design will cause a high number of material interactions, which may cause degradation. The materials chosen operate at extremely high temperatures. There is a need for more materials development to discover materials that operate at lower temperatures and have faster kinetics. It was hard to tell whether

the H2A analysis was done correctly because very few details were shared. The high-temperature operation will require expensive exotic materials, and it was hard to determine whether those costs are captured in the H2A. There is no one with industrial experience on the team. There are no companies doing this, but there are processes that are similar, though in less aggressive temperatures. Someone with experience in a similar industrial process would add value to ensure the proposed process is practical.

Recommendations for additions/deletions to project scope:

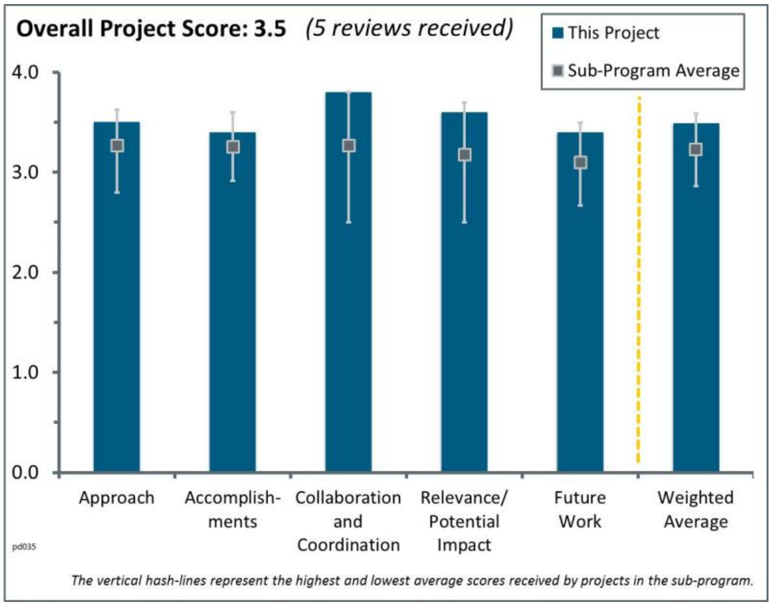
- The energy industry has much experience with reactors that are similar in design to what has been proposed. The researchers may want to engage some energy industry experts to better understand the challenges of the proposed approach.
- The project should establish a collaborative effort with an institution to address reactor design and modeling work necessary to assess gas–gas heat recuperation performance and solids heat transfer efficiency. SNL could fill this role, although the staff members engaged in SNL's competitive reactor design and modeling should be excluded from such additional collaboration. Alternatively, other non-academic institutions could be engaged, such as Argonne National Laboratory or LLNL. In any case, an institution with adequate capabilities should be engaged to accelerate detailed reactor system design, modeling, and analysis.
- Much more focus on better materials and realistic economic assessments is recommended. The project should deemphasize, but not totally eliminate, the reactor design effort. Bigger efforts can be resumed when better materials are found.

Project # PD-035: Semiconductor Materials for Photoelectrolysis

Todd Deutsch; National Renewable Energy Laboratory

Brief Summary of Project:

A long-term objective for this project is to develop a highly efficient, durable material that can operate under 10x–15x solar concentration and generate renewable hydrogen for <$2/kg via photoelectrochemical (PEC) water splitting. Objectives in the current year are to push the boundaries on achievable semiconductor PEC solar-to-hydrogen (STH) efficiency values and to continue development of stabilizing surface modifications viable at high current densities, focusing on III-V crystalline semiconductor systems and stabilization of $GaInP_2$ surfaces.



Question 1: Approach to performing the work

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

- A clearly defined research objective has been implemented and continues to show great promise. The III-V materials class is shown to provide a viable pathway to cost-effective hydrogen production. The focus of passivating the surface against photo-corrosion is showing promise, and the supporting collaborations provide the pathway to the development of a viable material.
- The focus of this team on improving efficiency and durability of existing materials in addition to investigating novel materials is an excellent approach to address some of the main barriers for the PEC Working Group.
- This is a premier group working on high-performance materials for PEC electrodes. The group's extensive experience, thoughtful approach, and innovations set a high standard for the field. The only reason the reviewer did not rate the project as "outstanding" is because the materials work could be strengthened to include a strong failure analysis component to truly understand the physics and chemistry of device degradation, and to utilize processes that are manufacturable for large-scale installations, rather than laboratory tooling. This knowledge will be invaluable to making progress toward the DOE Hydrogen and Fuel Cells Program (the Program) goals. A partnership exists to do this work; however, it is not clear whether it is active enough to serve the needs of the Program.
- The approach to developing high-STH, efficient, durable PEC materials is excellent. The work has resulted in significantly increased operational durability, greater understanding of the mechanisms of degradation, and demonstration of significantly increased STH efficiency. While these metrics still fall short of the requirements for cost-effective PEC hydrogen production, the progress is encouraging. An integrated production concept is presented, although detailed design awaits final selection of active interface materials with effective catalyst formulations, as well as electrolyte formulations that avoid or mitigate electrode fouling. Accelerated progress might be possible upon implementation of in situ observations of operational changes in interface characteristics, such as chemical composition and/or energy states of interfacial active materials. Gross or integrated performance measurements are useful but seldom carry all the information necessary to identify underlying causes of performance changes.
- It is refreshing to see optimization/extension of durability for known materials versus continuing to look at new scattered directions. It is unclear what impact lifetime has on cost projections. A tornado plot would help. There is a good and balanced mix of detailed characterization, modeling, and modification. It is not clear what the practical/achievable scale is. There is good grounding in Pt content, but it is unclear if there is a long-term plan to get around.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- Accomplishments over the past year show excellent progress toward the project goals, which themselves are targeted on the DOE goals. Especially encouraging is the continued advance of interfacial material understanding developed under this project. Faradaic yield measurements are encouraging, although the on-sun experiments showed damaged electrode surfaces, indicating continued work needed for interface protection. The DOE PEC Working Group and Joint Center for Artificial Photosynthesis (JCAP) address very similar goals and could benefit significantly by effective and mutually agreeable collaboration. Such collaboration might benefit from encouragement to both the PEC Working Group and JCAP by DOE Hydrogen and Fuel Cells Program (Program) management. Insufficient data are presented for a firm conclusion, but sonication of fouled electrodes followed by fresh electrolyte appears to accelerate failure of treated samples. The one case presented of electrode performance without sonication and refreshed electrolyte appeared to show reasonably stable operation until termination of the measurements. If this observation is true, then it seems essential that the project identify the causes for rapid failure of cleaned, treated electrodes.
- Significant progress made by this team on this project include the publication of the book on PEC standards, the improvements in durability on the $GaInP_2$ material, and the photoreactor testing.
- The recent progress is encouraging. The dilute Pt/Ru passivation discovery provides a clear research direction to enable a process effective semiconductor for photoelectrolysis. This group continues to be a central leader in photoelectrochemistry, with efforts in both fabrication of high quality III-V semiconductor material and methods for improving the semiconductor durability.
- This project is making notable progress toward its goals; however, the true feasibility of this approach to reduce the cost of hydrogen at scale was not clearly explained. It is not only about the cost of the materials; it is also about the cost of production of the reactors (i.e., large-scale devices), and this has not yet been considered. It is a great accomplishment to keep these materials from dissolving fast, but a clear path to month- or year-long durability has not yet been laid out. It would be very useful if these two points were considered in developing plans for next year.
- The difference in decay on slide 12 between treating and not treating is not clear. There is good identification of side issues, such as Pt fouling, which could have misled results. The setup of the surface passivation test was well thought out. It was very good to see some type of reactor and measurement of H_2/O_2, even if rudimentary. Comments from the previous year were taken into consideration and addressed.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.8** for its collaboration and coordination.

- Collaboration and coordination with other work and institutions in the PEC Working Group are outstanding and should be seen as an excellent example for other projects to emulate. At the same time, there is much effort outside the DOE/Energy Efficiency and Renewable Energy PEC arena that could benefit from and contribute to the field of PEC research and development if an effective collaborative framework could be established.
- The extensive collaboration of this team with other researchers is an outstanding element of this project.
- The National Renewable Energy Laboratory (NREL) group goes to great lengths to establish and support collegial relationships for the development of this technology. A very close relationship has been established to support the surface analysis effort, which has been proven to have high efficacy with the joint discovery of a passivation method for the semiconductor surface.
- This project is very well connected to key collaborators. The relationships are well leveraged to reach the project's goals. One item the reviewer would have liked to have heard more about is how all the collaborators besides those at University of Nevada, Las Vegas, (UNLV) and Lawrence Livermore National Laboratory (LLNL) are contributing to the work. Also, the UNLV team seems to have broad resources that could be useful but are not really being used for the project.

- UNLV and LLNL collaborations are clear and add important contributions. Roles of others are not as clear and could be further elaborated.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- Given the current state of the art, this project pursues the most promising materials system for practical PEC.
- Relevance and potential impact of this project are excellent. Successful implementation of improvements identified in the waterfall chart would lead to meeting DOE's long-term goals for PEC hydrogen production. The roadmap implicit in the waterfall chart provides excellent guidance to project tasks and priorities. Quantitative metrics for identifying go/no-go decisions are identified in the tasks in the waterfall chart. The project would be improved by estimation of the schedule and cost for meeting these quantitative go/no-go decision points.
- This project is definitely well aligned with DOE's long-term objectives on renewable hydrogen production pathways via PEC water splitting. It is addressing some of the critical parameters to meet DOE's cost target for this technology.
- As shown in the presented waterfall chart, the III-V materials class has a viable pathway to achieving the DOE benchmark for cost of hydrogen production. This material currently presents the minimal risk for achieving these goals. The work being conducted here is highly relevant to achieving the DOE goals.
- The project is an important part of the portfolio, but the general agreement is that commercial PEC is a long way off. There is a need to balance near- and long-term components of the Hydrogen Production and Delivery sub-program portfolio (this is a general comment for the Program).

Question 5: Proposed future work

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

- Proposed future work is to be focused on durability, efficiency, and materials cost. A photoreactor prototype should provide a good basis toward achieving the main goals of this project.
- The continued development of this materials class for durability should remain the primary focus. The NREL group proposes raising the efficiency of the III-V tandem to 25%—a requisite to achieving the cost metric. Alternative, more cost-effective fabrication methods are proposed. Although it is interesting to identify cost-effective alternatives, at this point it is most important to develop the passivated materials.
- Segmented cell characterization will provide value; work to extend durability seems reasonable and is a critical need.
- Proposed future work identifies tasks, but little detail on how these tasks are to be implemented is provided. Task priorities and mitigation or "workarounds" for unsurpassed barriers are a weakness in the project. Optional approaches should be identified for those critical issues that could lead to unacceptable concept performance.
- The proposed future work (slide 22) seems to be too large of a step from where the project is today. This is the correct long-term direction; however, moving to solar concentration and solving all the resulting materials problems without robust options for corrosion protection and system durability and stability seems very challenging. The reviewer would have liked to have seen more of the roadmap laid out.

Project strengths:

- This project has a significant history with, and understanding of, the III-V semiconductor material for PEC hydrogen production. This material offers the best pathway to achieving the DOE goals for cost-effective solar hydrogen production. The expertise with the NREL group and the extended collaborative PEC Working Group results in a formidable team with an unprecedented capability for both theoretical and experimental investigations.

- The concept, approach, team, and collaborators make this a very strong and viable project. Integration of basic materials science into the fabric of assessment and analysis adds great strength to the approach.
- This project is focused on the most promising materials set for viable PEC generation of hydrogen. The understanding of the device requirements is deep.

Project weaknesses:

- The singular weakness with this project is the disconnect with the fabrication team for III-V material. It appears that the material is produced with a foundry-type relationship. It would be better served if the material fabrication was a more integral part of this project, with a shared ownership in the success. The project needs a larger quantity of material at this point of the research effort.
- Planning of future work is deficient in seeking optional paths forward should one or more of the current tasks fail to succeed.
- The chief weakness of this project is in its lack of materials characterization. Good connections exist with the UNLV team, but extensive routine chemical and physical analysis was not reported at the level the reviewer expected. Inductively coupled plasma (ICP) mass spectrometry, which is hard to do quantitatively for alloys, and optical inspection are the main tools—moving beyond them will really benefit the project by revealing the true nature of degradation and the consistency of the structures being built. What is being done now is too qualitative to inform the work at the level needed.

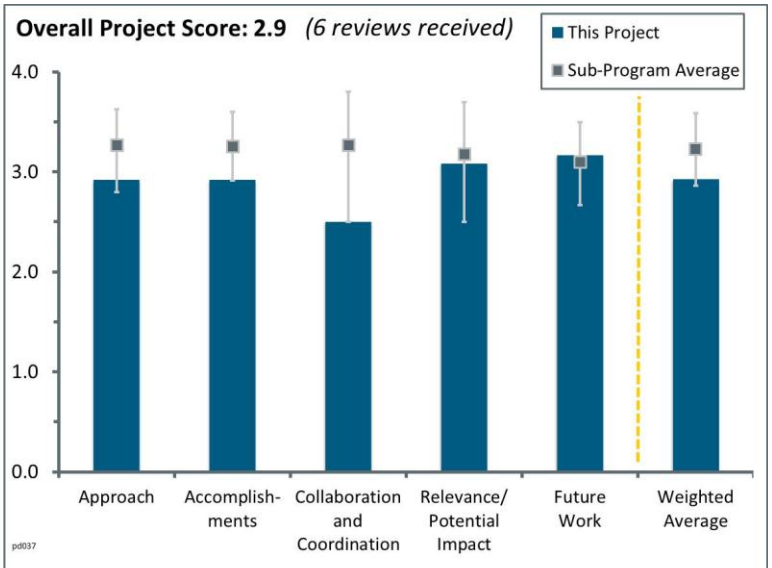
Recommendations for additions/deletions to project scope:

- Additional effort should be added in attempting to make in situ observations of material interface properties under operational conditions. This would be a first of its kind in materials science and could lead to process understanding of extraordinary merit. This is a rich area for instituting collaboration with other programs doing similar work, such as JCAP.
- It is recommend that a plan be made to expand the scope to incorporate new partnerships and, perhaps, new instrumentation for detailed materials characterization. Active partnering to bring in materials innovations as they are published and leveraging work in related programs as appropriate will be beneficial. This is truly essential for the project to meet its goals.

Project # PD-037: Biological Systems for Hydrogen Photoproduction

Maria Ghirardi; National Renewable Energy Laboratory

Brief Summary of Project:



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

The primary goal of this project is to develop photobiological systems for large-scale, low-cost, and efficient hydrogen production from water. Two specific tasks are being addressed. Task 1 explores the oxygen sensitivity of hydrogenase that prevents continuous hydrogen photoproduction under aerobic, high solar-to-hydrogen (STH) conversion efficiency conditions. Task 2 genetically adds various desirable traits to an algal straining expressing and oxygen (O_2)-tolerance hydrogenase to achieve higher STH and longer durations of hydrogen photoproduction.

Question 1: Approach to performing the work

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

- This project is well focused on the hydrogen production rate and O_2 accumulation barriers, and it is pursuing these in a rational fashion through a combination of genetic modification and improved and more consistent techniques. The project's work with algae complements work by Pin-Ching Maness with a cyanobacterium.
- The approach to boosting hydrogen production from the O_2-tolerant clostridial hydrogenase using new transformants with introns was a logical next step, even though it did not increase hydrogen photoproduction.
- The outlined tasks contribute to overcoming limitations in hydrogen production, including using a bacterial hydrogenase with less O_2 sensitivity. While the project aims are on task, some of the challenges due to experimental results and personnel changes have led to delays or changes in milestones.
- At a broad level, this project and PD-095 share similar traits, although the details are different. The comments here are almost the same used for PD-095. The approach is clear and seems suitable to the objective as stated, but the objective itself is narrow, and it is not put into the broader context of producing hydrogen in a cost-effective manner. The problem tackled here is only one of the pieces needed to make hydrogen, but there is no information about the relative importance of this piece versus the others (e.g., photosynthetic efficiency). It is perfectly fine to work on a piece of the whole in parallel, but context would be useful to assess whether the whole is worthwhile to begin with. The reviewer appreciates the project's recognition that more steps need to be taken care of (Subtask 2).
- The proposed approach appears logical, but this project seems to have had many changes in scope and milestones that have been postponed. Hence, there is concern that the principal investigator (PI) does not have a clear understanding of what is needed to achieve the proposed goals.
- There appear to be issues with control of gas headspace in the hydrogen production test cell, preventing assessment of the true kinetics of hydrogen production (e.g., Strain 55 cumulative hydrogen production should show steady increase over 30 min).

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- Steady progress continues; the researchers were successful in generating and testing the impact of intron transformants, albeit it is not clear whether this area was adequately pursued because inadequate funding was cited as a reason for working only with constructs of the RBCS2 intron. Perhaps if more funds were available, the same decision would not have been made. Additionally, the researchers made a significant observation that hydrogen pressure buildup in the bioreactor headspace was a contributing factor to hydrogen production, and hence, a simple modification of bioreactor size may significantly improve hydrogen yield.
- There is good progress with genetic engineering of *Chlamydomonas* to demonstrate improved O_2 tolerance to meet 2015 targets. The reasons for greatly decreased hydrogen production in the engineered strain were not clear; it seems that the 50-fold reduction relative to the wild-type strain cannot be due to a reduced Michaelis constant (Km) alone. More scientific discussion is warranted. No publications were reported for the current review year.
- This research has encountered some delays due to equipment (i.e., microscopes) and personnel (loss), as well as encountering a no-go decision. However, it appears to have rebounded from the no-go decision with new ideas and has continued to progress its research.
- The experimental challenges, equipment delivery problems, and loss of personnel have led to a delay or change in scope of milestones. The PI has taken steps to mitigate problems and has implemented new approaches to meeting milestones.
- Qualitatively, it looks like some progress has been made. The reviewer appreciates that this project reports data on hydrogen production, which is a relevant metric. As the authors clearly say in the presentation, the metric is still far from the target.
- There are some new results, but there do not appear to be any major accomplishments during the last year. Much of the truly challenging work appears to have had the completion date postponed.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.5** for its collaboration and coordination.

- While this project claims no formal partners, it is interacting with the laboratories of Professor Tasios Melis, Dr. Gilles Peltier, and Professor Matthew Posewitz in accessing genetic materials and techniques, as well as obtaining assistance from Professor Patrice Hamel for work related to Task 2 (i.e., acquiring desirable traits). These appear to be unfunded.
- The National Renewable Energy Laboratory team looks strong, but it does appear that the budget supports outside collaborators, particularly with expertise in bench-top photobioreactor design for biohydrogen experiments.
- There is little outside collaboration, and much of it appears to be out of necessity rather than experimental and task design. Work on understanding the reactor conditions for the *Chlamydomonas* is an area that may provide fruitful outside collaborations (e.g., photobioreactor design, etc.).
- There was one unpaid collaboration for the project. According to researchers, there would have been more collaboration if funding had been sufficient.
- The PI commented that the lack of collaborations was due to lack of funds. In response to a previous reviewer’s questions, the investigator indicated that unfunded collaborations were continuing. Any existing collaborations were not addressed in the main body of the presentation.
- Collaboration was not really discussed much. There were none for Subtask 1 and “unfunded help” for Subtask 2. It is not clear that the resources are appropriate for success.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- There were no issues; the research is highly relevant to the DOE Hydrogen and Fuel Cells Program (the Program).
- Photobiological water splitting coupled to hydrogenase-mediated hydrogen production is an important approach to achieving DOE long-term hydrogen production goals.
- The project is in line with the Program goals. Based upon the data presented, the project has the potential to meet the Program goals; however, the current low rates of hydrogen production and the multiple steps needed to increase to fiscal year 2015 (FY 2015) targets and beyond may take longer than outlined.
- If successful, an STH-efficient algae possessing an O_2-tolerant hydrogenase could make a significant contribution to DOE's photobiological hydrogen production goals. Equipment (i.e., photobioreactor) modification to provide more headspace, technique improvements, and crossing with three other strains to acquire desired traits could be a significant move to satisfying most of the 2012 Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan's 2015 photolytic biological hydrogen production targets.
- While this work is interesting, it is not clear how this PI's work has the potential to be high-impact. This could be more a function of the presentation and less a function of the science.
- This is only a piece in producing hydrogen from water splitting by microorganisms. In isolation, it is hard to assess whether even complete success would help the final goal.

Question 5: Proposed future work

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

- Switching to a different plasmid to progress Task 1 is commendable; crossing an O_2-tolerant recombinant with three other strains to confer additional traits in pursuit of increased STH efficiency is very laudable.
- The proposed goals for FY 2015 are excellent, but there are concerns about the PI's ability to meet those goals, given past performance. However, because of the amount of effort already put into this work, it should be seen through to a logical completion.
- The proposed future approach includes many experiments to increase the rate of hydrogen production and is well thought out. There may be risks and challenges that have an additive effect and result in delay of project targets, though.
- The proposed future work seems appropriate for the narrow objective. Resources are questionable.
- More detail on Subtask 1 would be appreciated, specifically with regards to the hypothesis-based approach to (a) identifying current rate-limiting steps and (b) finding and decreasing data variability.

Project strengths:

- The PI clearly understands the Program goals and has designed milestones and tasks to address current barriers for efficient hydrogen production. There are multiple approaches to increasing hydrogen production, and they span many different aspects of the production process.
- Future plans to introduce several new traits in pursuit of improved STH are very laudable.
- The pioneering work that was completed over the past 12 years and the body of knowledge generated will continue to advance similar efforts.
- The team's biology expertise is a project strength.
- Good progress on genetic engineering of *Chlamydomonas* for improving O_2 tolerance under hydrogen production conditions at full sunlight has been made.
- The project has interesting approaches to decreasing O_2 sensitivity and increasing hydrogen production.

Project weaknesses:

- The team could benefit from the knowledge and experience of other experts in the field.
- There may be other steps in the pathway that can limit hydrogen production even if the problem at hand is successfully solved.
- There are concerns about the project's ability to complete stated goals.
- While the multiple approaches to increasing productivity are appreciated, it presents a challenge to the team to optimize production with many variables in play. It could be useful to assess the relative impacts of variables and focus efforts more in that direction. A more thorough description of the biological methods to improve hydrogen production, particularly with respect to the aggressive targets, would have been appreciated.
- The hydrogen production test cell should be redesigned to provide data on the true kinetics of hydrogen production. The project needs a hypothesis-based approach for addressing low hydrogen production to better interpret current data and to identify more specific strategies for proposed future work. Plans are needed for presentation and publication of the work to get feedback from the biohydrogen research community.

Recommendations for additions/deletions to project scope:

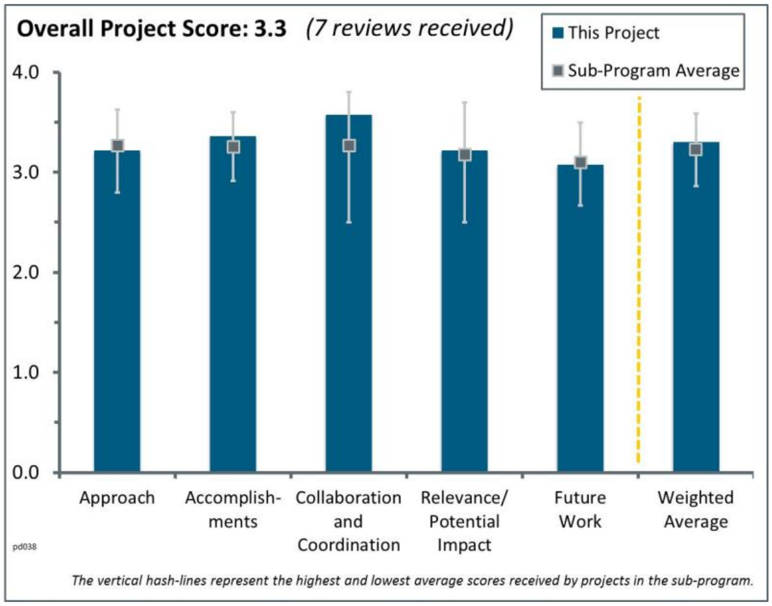
- The PI should consider collaborations to increase the likelihood of success for the project, specifically ideal photobioreactor use/design and organismal engineering.
- The project should consider appropriateness of resources and reassess the potential of the entire pathway.
- The basis of milestone targets is not clear. This research project had a Q2-1 go/no-go milestone of: (1) an initial rate of 11 µmol H_2/mgChl/h, (2) a final rate of 0.06 µmol H_2/mgChl/h for (3) at least 30 minutes, and (4) equal to or >1x (slide 8) or 2x (slide 15) than the final wild type (WT) net yield. It would be useful if such values could be related to DOE targets in some way or simply related to being confidently better than a baseline (e.g., WT) performance.

Project # PD-038: Fermentation and Electrohydrogenic Approaches to Hydrogen Production

Pin-Ching Maness; National Renewable Energy Laboratory

Brief Summary of Project:

The overall objective of this project is to develop direct fermentation technologies to convert renewable lignocellulosic biomass resources to hydrogen. Feedstock costs are being addressed via bioreactor development using lignocellulose. The bioreactor is optimized by testing parameters, such as lignocellulose loadings, hydraulic retention time (HRT), and liquid volume replacement and frequency, using the cellulose-degrading bacterium *Clostridium thermocellum*. Hydrogen molar yield is boosted through genetic engineering and integration with microbial electrolysis cells (MECs).



Question 1: Approach to performing the work

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

- The programmatic approach is logical and has a clear target.
- The project objectives are focused to address the barriers related to hydrogen molar yields and system engineering. The feedstock cost barrier is partially addressed by projects funded by the Bioenergy Technologies Office (BETO) in other groups at the National Renewable Energy Laboratory (NREL). Some of the cost-prohibitive elements of utilizing lignocellulosic feedstocks (e.g., pretreatment of biomass and removal of acetate in future work) are being studied by BETO projects.
- Trying direct conversion of cellulose instead of sugars to try to lower feedstock costs is an understandable and good approach. An alternative is direct biomass gasification to hydrogen. This approach should be compared to the project’s approach to see which pathway has more promise. The tasks of optimizing the bioreactor and of redirecting metabolism to improve hydrogen productivity make sense, but an assessment of what the targets need to be to reach cost-effective hydrogen production would be helpful. It is not clear whether there has been consideration of what to do with the C5 sugars, which are a substantial fraction of the feedstock. Presumably, they are supposed to be used in the subsequent microbial electrolysis step, but clarification would help. It is clear why MEC is envisioned as a way to use a lot of the feedstock unutilized in the fermentation reactor, but it would require more investment.
- The genetic toolkits developed for pathway engineering and generation of *C. thermocellum* mutants were effective. However, the approach would greatly benefit from metabolic flux analysis experiments, which may identify more appropriate targets for metabolic engineering leading to increased hydrogen production and lowered organic acid/alcohol byproduct formation.
- The project is well-focused on the hydrogen molar yield) and feedstock cost barriers by employing a novel electrolytic approach to increasing hydrogen yield and processing lignocellulosic biomass. Systems integration awaits further development and characterization of both the fermentation and electrolytic subsystems.
- The approach continues to be adequate. There is still a long way to go, but the approach may need reconsideration of components to make real progress.
- The three-step approach to increasing hydrogen yield (i.e., optimizing the bioreactor process, knocking out pathways to lactate and ethanol production, and using MEC to convert byproducts to hydrogen) is sound

from the perspective of increasing hydrogen generation, but it is hard to imagine that this rather complex approach will meet the DOE Hydrogen and Fuel Cell Program's (the Program's) economic targets.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The overall project made a significant amount of progress to achieve stoichiometric hydrogen production from cellulose. There was one publication and three presentations resulting from the work during the reporting period.
- Other than a delay in generating a delta triple mutant, progress appears to be on track.
- Good progress and intermediate accomplishments have been presented, although the project has been going for eight years.
- Many milestones are completed, particularly for utilization of cellulose from lignocellulosic biomass, while many others are on track after delays.
- The results that indicated no lignin inhibition were very strong. The knockout work to generate a strain that contains only the acetate pathway is interesting, and the initial results with the formate pathway knocked out are promising. The Pfl knockout results did not appear to have a significant difference in final production. On slide 10, figures A and B do not seem very different. Slide 10, section C does show an increase in lactate from 10 to 14 hr, but by 22 hr the Hpt knockout is similar. The MEC results in Task 3 were interesting, but apparently there are concerns about scale-up that were raised by another reviewer.
- Progress has been slow but steady. The improvements in lignocellulose conversion due to bioreactor optimization, the unique capability to transform *C. thermocellum*, and the design of a plasmid to knock out lactate production are all significant contributions toward efficient conversion of lignocellulose to hydrogen.
- The project has made very good progress in increasing hydrogen productivity in the bioreactor, but much more is needed. There is good progress in redirecting metabolic pathways to increase hydrogen production. However, the expected improvement from this approach, if successful, is not clear. Clarifying what success looks like and assessing whether it is worth the effort in terms of increased hydrogen are suggested. The 10% improvement suggested in the table on slide 8 may not be large enough. Similarly, while it is clear why the researchers are using MEC and how it increases the overall use of the feedstock towards making hydrogen, it is not clear what the MEC performance target needs to be to make the overall process viable. Whether success in MEC is nice to have or critical, and/or worth the investment, needs to be assessed. Also, clarifying the source of waste heat for MEC in the overall process or, alternatively, defining the needed power to drive the process is suggested. Overall, this project would benefit from a better definition of the integration between the three key pieces: bioreactor, metabolic pathways, and MEC. As it stands, it is not easy to see how improvements in each benefit the whole and how much improvement in each is needed.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- This project has strong external collaboration.
- The project has strong ties with a biomass feedstock source, Canadian researchers developing genetic methods (with leveraged Canadian funding), and Dr. Bruce Logan (microbial electrolyzer).
- The two principal investigators (PIs) are well coordinated and are working towards the same goal. For the past and proposed future work, further collaboration with NREL scientists working on BETO-funded projects is encouraged to fully utilize their biomass and organismal design capabilities.
- The collaboration with Dr. Bruce Logan is strong and well integrated.
- The collaboration with Dr. Bruce Logan is apparent, as are the in-house NREL collaborations and the collaboration with Genome Canada.
- The collaboration appears adequate.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- There are no issues; the research is highly relevant to the Program.
- This work will significantly add to the body of knowledge that hopefully will ultimately lead to a one-step process where neither pretreatment nor MEC will be needed to produce the 8 mol of hydrogen or greater yield at the targeted costs.
- The inexpensive feedstock and potential of near-complete conversion to hydrogen make this technology pathway very attractive, assuming rates are adequate to keep capital costs competitive, other efficiencies are reasonable (including identifying the source of the yield reduction when compared to pure cellulose feedstock), and the ultimate system can demonstrate high utilization rates.
- The PIs are focused on addressing barriers to biological hydrogen production that would address the programmatic needs of the Fuel Cell Technologies Office (FCTO). The rates of microbial production, source of sufficient biomass resources, and rate of MEC hydrogen production may be insufficient when scaled up to significantly impact the market.
- Lack of information on the bioreactor/MEC integration and on MEC production rates and power use makes it difficult to assess the overall scheme in terms of cost-effective hydrogen. The data on the bioreactor can provide the basis for some estimates. In addition, the 2020 targets shown on slide 3 need to be addressed. Assuming success in reducing feedstock cost to 8 ¢/lb sugar and in increasing the yield of hydrogen in the bioreactor, then just the biomass feedstock (no capital costs) adds up to nearly $3/kg of hydrogen. Of course the extra production from MEC improves the hydrogen cost because the feedstock is free, but there is not enough information to assess the contribution of MEC to the overall cost. Based on the hydrogen production rates in the presentation and from subsequent discussions with the PI, to achieve 50 tons/day hydrogen production currently, the total bioreactor volume would need to be several 100 million liters. The largest corn ethanol fermenter may be close to 2 million liters today, and three of those are usually used. Either the production rate of the bioreactor is increased almost 100 times, or there is a substantial contribution to hydrogen production from MEC, which is difficult to estimate with the data provided.
- The results of this effort seem to have demonstrated a step forward in the field. There are concerns about the real-life applications and scale-up of the current technology.
- This is still far from being a viable effort, except for niche applications.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- Future work is the logical extension of past successful work by existing collaborators.
- The proposed future work is well thought out for the three tasks presented. More information regarding Task 4 would have been appreciated.
- It will be exciting to see whether blocking both lactate and ethanol production pathways significantly increases hydrogen yield.
- The proposed work should continue to advance this interesting project. There are concerns about the ability to knockout all but the acetate production pathway. The cell seems to favor other methods, as demonstrated by the increased ethanol production. Leaving only one of four pathways as a formate pathway could overwhelm the cell and result in other unintended mutations.
- Metabolic flux analysis is needed. It is unclear how the process flow diagram can be simplified. The project may benefit from collaboration with a chemical engineer.
- It is not clear that the proposed work on the bioreactor and the metabolism has the potential to achieve the DOE goal of cost-effective hydrogen production. This point should be clarified. The role of MEC in the overall process and its integration with the bioreactor should be clarified, as should its potential to lower hydrogen cost. Also, the power and size requirements of MEC should be quantified in a manner consistent with the bioreactor (e.g., using a common basis for the amount of feedstock processed and the consequent

bioreactor effluent fed to MEC). The fate of the C5 sugars should be clarified (e.g., whether they are utilized in MEC) in order to understand the overall efficiency and cost of the scheme.

Project strengths:

- Consolidated bioprocessing organism work is good and has demonstrated that lignocellulosic materials can be used for hydrogen production. The successful knockout of the formate-producing enzyme suggests that knocking out the ethanol and lactate pathways will increase the yield of hydrogen.
- The project has clear goals, has made very good progress, and is likely to continue to yield interesting results.
- There is good progress on bioreactor and MEC. There is impressive genetic work.
- The pioneering work to develop a plasmid that can transform *C. thermocellum* to knockout the lactase dehydrogenase competing pathway is a key strength of the effort.
- Cheap feedstock and high hydrogen yield are two project strengths.
- The project is making good progress towards achieving stoichiometric hydrogen production from cellulose.

Project weaknesses:

- It appeared that the presentation did not provide feedstock and electrode cost data to compare with the 2012 FCTO Multi-Year Research, Development, and Demonstration Plan 2015 targets.
- Some of the goals for knockout strains may be overly ambitious.
- From a process engineering perspective, the flow diagram is cumbersome and complex, requiring two bioprocess-based subsystems.
- There was a lack of discussion of other metabolic engineering that could be performed to increase hydrogen and acetate production. Mitigation strategies for potential deleterious consequences of knocking out the lactate and ethanol pathways were also not discussed. Techno-economic analysis for this project needs to be considered, particularly around feedstock cost. The cited 2011 cost from previously funded BETO work may be out of date considering new cost projections of $80/dry ton of biomass and the new pretreatment method proposed for the feedstock. Additionally, comparison of current hydrogen production methods with fermentation and MEC would be critical. It was unclear from the presentation what, if any, separations methods are being tested or will be tested for cleaning up the fermentation effluent before introduction into the MEC. Separations could be critical to successful future commercial implementation of this technology.
- Integration of bioreactor and MEC is unclear. The impact of genetic work is unclear. Overall, it is not clear what the targets for success are and if they are achievable to meet the DOE goal of cost-effective hydrogen production. Little economical assessment was done.
- After 10 years of effort, there is still no technoeconomic analysis that validates that this is a viable approach.

Recommendations for additions/deletions to project scope:

- It may be possible to think more imaginatively about this project. Right now, two subsystems are needed for the process to work. It may be possible to integrate these two systems in a way so that the carbohydrate is converted directly to hydrogen in a single bioreactor system.
- The project should look into the newly reported one-step lignocellulosic-to-hydrogen process using *Thermoanaerobacterium thermosaccharolyticum* M18 to see if the claims are credible and if the findings offer any important insights.
- This project could be more cost-competitive with a better understanding of the yield and rate of hydrogen production during fermentation, as well as characterization of the remaining solids in the fermenter. Many biorefineries use the remaining lignin after pretreatment and hydrolysis for combined heat and power, thus reducing their energy costs and greenhouse gas emissions. Understanding the quality and impact of utilizing the remaining lignin may be a new positive for implementation of this project. Separations and clean-up technologies should be addressed as part of this proposal. Perhaps instead of tuning the anodes to tolerate protein, etc., removing contaminants or fouling agents before adding the fermentation effluent to the MEC should be considered. The positive and negative impacts of separations methods should be considered within the context of a multi-step process.

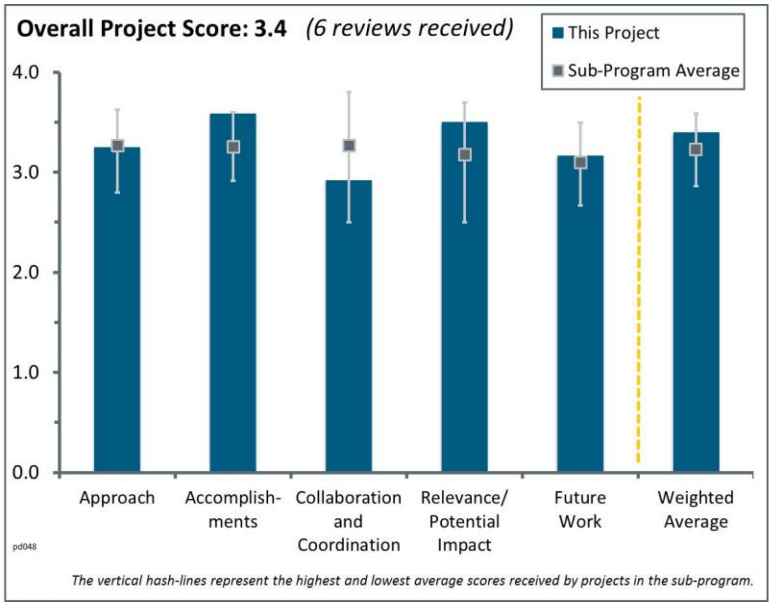
- There could be improved optimization of directed evolution of cells to improve efficiency.
- The project should clarify targets needed for success and whether they are achievable with the current organism and process; do some overall process modeling/engineering work to clarify integration of the bioreactor with MEC to assess synergies and potential; and enhance the economic analysis.

Project # PD-048: Electrochemical Hydrogen Compressor

Ludwig Lipp; FuelCell Energy, Inc.

Brief Summary of Project:

The objective of this project is to provide highly efficient, reliable, and cost-effective hydrogen compression between 6,000 and 12,000 psi through development of a solid state electrochemical hydrogen compressor (EHC). Development of an efficient EHC will increase reliability and availability of hydrogen over current mechanical compressors and eliminate the possibility of lubricant contamination, as there are no moving parts. The project strives to reach compression efficiency at 95%, which is expected to significantly reduce hydrogen delivery costs in the long term.



Question 1: Approach to performing the work

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

- FuelCell Energy has a great approach to the issue of developing a hydrogen compressor that does not use mechanical compression features. This technology also reduces the susceptibility to contaminants.
- Aggressive work to cut costs and increase pressure for electrochemical compression has been well thought out and implemented.
- The approach to this effort has been outstanding.
- The general approach has led to steady gains in hydrogen flux, cell efficiency, hydrogen compression, and cost reduction. Any future research should include process optimization studies based on key parameters, such as current density, membrane thickness, and operating temperature (i.e., optimizing conductivity vs. hydrogen back diffusion).
- Reliability, cost, and efficiency were identified as barriers, and the current project is addressing all of these.
- The project is a little weak on details of how improvements were achieved. It is understood that there is competition-sensitive information in the details, but the general thought process, analysis techniques, design principles, etc. should be discloseable.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- The move to a higher-surface-area cell is being pursued aggressively. Durability tests represent a significant milestone. Achieving 30-3,000 psi compression with a 185 cm^2 EHC stack is also a great milestone.
- Lifetime data, tests to >12,000 psi, and scale-up demonstrate the technical viability of this EHC. By increasing the current density and cell active area and reducing the cell part counts, the team has demonstrated a 60% capital cost reduction since project inception. However, to determine commercial viability, the team should also provide an economic evaluation that compares the cost of EHC to mechanical hydrogen compressors.

- The project has a clear strategy to overcome each of the barriers (i.e., higher cell area, higher current density, and lower part count to reduce capital cost). Cell improvements will increase efficiency and operating life.
- Good progress has been made over the term of the project. There are good results on membrane electrode assembly activity. The team needs to describe the benefits of having electromechanical compression over mechanical in more detail. There was some mention, but the team should stress the benefits for project recognition. There was good description of cost reduction, but the assumptions for volume manufacturing were not stated.
- Impressive gains have been made in efficiency and performance, but pressure capability is a little misleading. It is unclear to what pressure this design has been proof tested and how that relates to the International Organization for Standardization (ISO) standards for operating pressure.
- The scale-up of the prototype has been very successful.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- This technology is being developed by two strong contributors that are working well together. The EHC offers significant benefits specifically for those that want to implement tri-generation systems.
- The partnership of FuelCell Energy with Sustainable Innovations appears to be an effective collaboration with contributions from both partners.
- Clearly there is a close collaboration with Sustainable Innovations, but no other partners are on the project.
- FuelCell Energy has one collaborating partner to assist in EHC stack development efforts. It may be beneficial to partner with research institutions that can assist in optimizing the membrane technology for high-pressure applications.
- There are only two collaborators.
- The project is collaborating with only one partner. Working with more may allow for faster and better progress.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The power cost demonstrated for EHC appears to be less than that of mechanical compression alternatives, and EHC offers operational benefits over mechanical compression: no moving parts, no required maintenance, and no potential for contamination of the hydrogen stream, and it promises significant operating cost savings.
- The compressor cell has utility for many applications; it could use additional focus in the DOE Hydrogen and Fuel Cells Program (the Program) portfolio either as a standalone or with electrolysis.
- Hydrogen compression is high-cost and unreliable. This project has high relevance.
- The degree of compression needed as well as the life, cost, and efficiency are all highly relevant and impactful to the goals of the Program.
- The project is related to the development of an electrochemical hydrogen compressor, which has the potential benefit of reducing the cost of hydrogen compression for various hydrogen-producing technologies.

Question 5: Proposed future work

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

- The description of the future work is exactly what will be needed to overcome the barriers still facing this technology.
- The proposed taller stack is the next step, and this work should be funded.

- Continued cost-reduction and efficiency improvements may make electrochemical compression viable.
- The proposed future work appears to be focused on continued life testing and scale-up to larger multi-cell stacks that can achieve higher throughputs. Optimizing the membrane and the process operating parameters should be considered prior to scale-up.
- Endurance tests should be continued for longer hours. The project should develop a multi-stack of EHCs to demonstrate larger production of hydrogen as needed for site refueling.
- The goal of scale-up is important, but there are no details on how this is to be accomplished and what the critical issues are expected to be. It is unclear why this project needs DOE funding. The project needs to describe the fundamental challenges.

Project strengths:

- The project strengths include the zero-maintenance compressor system, no concerns about compressor fluids contaminating the hydrogen stream, zero-noise compression, and the ability to work well with low suction pressure.
- Both partners have strengths to address the barriers to use of this technology. The presentation clearly showed the progress made during the project, which is a strong indicator of the effectiveness of the project team.
- Strong results and progress were demonstrated.
- The project has the potential to reduce capital cost and downtime as compared to mechanical hydrogen compressors.
- A big strength is that few parts are needed for high-pressure compression of hydrogen.

Project weaknesses:

- Commercial feasibility may require high current density operation to reduce capital costs.
- An essential part of the cost reduction is to address manufacturing methods to produce the EHC parts in greater quantity and at lower cost. The manufacturing methods may affect the cell/stack design.
- Cell variability is a weakness. There was no demonstration of larger-scale production from multiple stacks.
- There was a lack of detail in approach and analysis. There was no response on reviewer comments from last year. It is unclear if this project was reviewed last year.

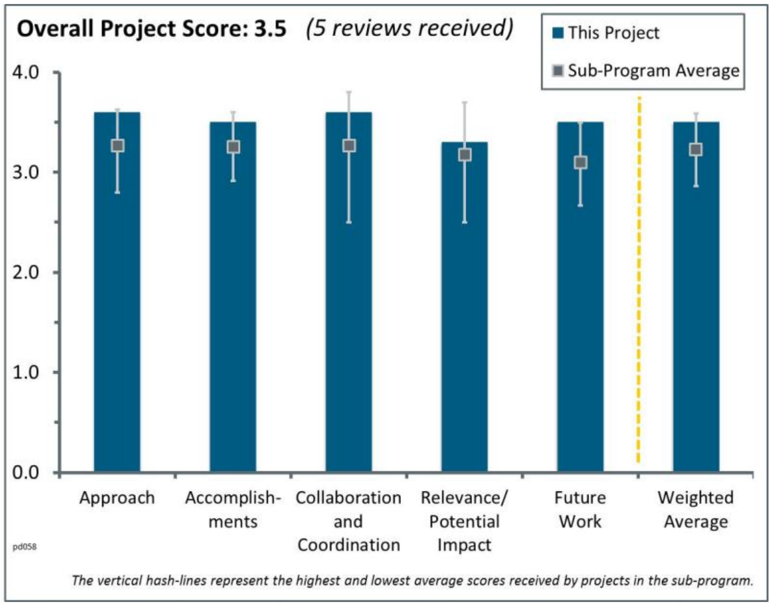
Recommendations for additions/deletions to project scope:

- The project team is on a good course. Further cell and stack improvements to both improve efficiency and reduce cost would have been a good addition to the presentation. Also, a comparison to the three-stage mechanical compression not only in efficiency but also in cost would be helpful to focus the project on areas of technology improvement relative to the mechanical compression alternative.
- The project should include project compression energy that will be required to go from 200 to 12,000 psi.
- The electrochemical hydrogen compressor development effort should be optimized before scale-up. An economic feasibility study comparing EHC technology to other compression technologies is advisable to understand commercial feasibility.
- Cell variability should be attached to ensure uniform current use. A multi-stack production system should be developed to make larger quantities of hydrogen.

Project # PD-058: Characterization and Optimization of Photoelectrode Surfaces for Solar-to-Chemical Fuel Conversion

Tadashi Ogitsu; Lawrence Livermore National Laboratory/National Renewable Energy Laboratory

Brief Summary of Project:



The objectives of this project are to: (1) develop a theoretical tool chest for modeling photoelectrochemical (PEC) systems, (2) compile a publications database of research on relevant photoelectrode materials, (3) uncover key mechanisms of surface corrosion of semiconductor photoelectrodes, (4) understand the dynamics of water dissociation and hydrogen evolution at the water-photoelectrode interface, (5) evaluate the electronic properties of the surface and water-electrode interface, (6) elucidate the relationship between corrosion and catalysis, (7) provide simulated X-ray spectra to the University of Nevada, Las Vegas (UNLV) for interpretation of experimental results, and (8) share research insights with PEC Working Group members.

Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- The approach to this seriously underfunded effort is excellent. Important aspects of electrode corrosion and transport processes at the electrode–electrolyte interface have been identified. The collected database of materials properties and PEC processes has provided important insights into observed behaviors of various PEC components. In spite of limited resources, the project is aligned with other efforts that will ultimately provide improved tools for even more detailed investigation of the chemistry and dynamics of hydrogen evolution in PEC systems.
- This group is performing high-quality theoretical work on topics relevant to the Hydrogen production sub-program. It is well connected to experimental programs and to the scientific and technological communities, so its projects are appropriately targeted. The reviewer gave it an excellent rating, rather than outstanding, because it would benefit from refocusing on fewer projects and addressing them more deeply than is currently possible.
- The approach taken by the researchers on identifying the electrode properties that effect electrode stability integrated with the results and work done at both UNLV and the National Renewable Energy Laboratory is an excellent approach towards the main efforts of the PEC Working Group.
- The Lawrence Livermore National Laboratory group is responsible for theoretical work relating to the surface at the semiconductor–electrolyte interface. Using density functional theory simulations, models are being developed that help explain current issues with the surface corrosion for the III-V semiconductor material. The research provides value towards allowing the materials to achieve the U.S. Department of Energy (DOE) benchmarks for durability.
- Within the provided budget, the scope is probably all that can be managed, but it would be good to get past the schematic of “stick in a beaker” for PEC. It is not clear how this will ever get past a bench cell level. No one has done system modeling—it is unclear if this is planned. There is a good connection/linkage between characterization and performance.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- Keeping the limited budget in perspective, DOE received outstanding value from this research. The science was expanded, a product of three archival papers was generated, and the understanding and capability for these models may offer predictive efficacy for the development of future materials.
- Accomplishments have been outstanding. Progress toward improved capability is excellent, although resource limitation inhibits the kind and rate of progress needed to keep pace with overall PEC objectives.
- Several publications in fiscal year (FY) 2014 have resulted from the ongoing efforts of this project in addition to enabling the development of a novel PEC hydrogen evolution reaction (HER) model.
- This team is performing excellent work with very few resources. However, it is not clear if all the work will help overcome barriers. At this point, the main levers are understanding and eliminating failures due to corrosion, which means understanding mechanisms, and this is where theory can really help. The papers presented as accomplishments focus on HER mechanisms and III-V/water interfaces, which are already known to be inherently unstable. This focus seems to be off the main path to success given the resources available. The proposed work from this year and 2013 include critical path work, but it does not seem that the team has the resources to make the progress it wants to (the team pointed this out on the slides).
- Accomplishments include steps toward thorough fundamental understanding and broad application/data compilation.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- Collaboration and coordination with other institutions are outstanding and an essential ingredient of this project.
- There is a good existing network, as well as recognition of a skill gap and work to bring in new partners.
- This team is very well connected with active and appropriate collaborations.
- The team has a significant collaboration with the PEC Working Group.
- This work was conducted as part of a larger collaborative effort to understand the issues with surface degradation of the semiconductor material. Although the project is most likely budget-constrained, it would be interesting to include the copper-indium-gallium-diselenide (CIGS) system for modeling.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The reviewer gave the project high marks for working in areas of relevance to the broad technical needs for the PEC program, but it is not focused or large enough to have the impact it should. A step back to consider a restructuring of goals for this project could help.
- The project clearly aligns very well with DOE's Hydrogen and Fuel Cells Program for the PEC hydrogen production pathway by focusing on some of the critical barriers, such as materials efficiency and durability.
- Relevance and potential impact on PEC progress are excellent, especially in light of the general strategy of the PEC Working Group. Resources are inadequate for concerted code development and validation effort that could lead to breakthroughs in understanding the microscopic dynamic processes of corrosion and photolysis, so it is not possible to assert the level of impact that might accompany this effort.
- The project is working toward thorough fundamentals and linkage to device; toolset development should be applicable for a range of projects.
- These models will provide great value if they can be predictive and help direct the nature of the materials fabrication—e.g., hydrogen diffusion is better for the InP compared to the GaP. It is not clear how this knowledge can affect the development of the semiconductor surface.

Question 5: Proposed future work

This project was rated **3.5** for its proposed future work.

- Proposed future work is excellent and is focused on identifying efforts that could further illuminate the processes of corrosion, hydrogen evolution, and other processes affecting electrode durability and the dynamics and chemistry of interfaces under operational conditions.
- The proposed work presented by the principal investigator is clearly defined and should provide significant progress towards achieving the main objectives of this project.
- The proposed development of a quantitative PEC HER model would be very interesting, and the ability to apply this model to a variety of materials might prove its value. It will be important to model the system as close to reality as is possible, which would require the semiconductor operating in the presence of light.
- Key descriptors for electrode durability are a good focus.
- This team is well focused on the right actions but is too small to do them—the team pointed this out clearly.

Project strengths:

- The tools and expertise brought to the PEC hydrogen production effort by this project are outstanding and add to the strength of the entire PEC effort. Continued dedication by the project members in spite of little prospect of receiving needed funding increments is admirable.
- The team has excellent technical strength and deep understanding of what theory can bring to this very important and challenging technical area.
- The project brings a great deal of science to the greater effort of development of semiconductor materials for solar photoelectrolysis. The insight these models provides should help shape the direction of research.

Project weaknesses:

- The models to date serve as a more ancillary research effort, helping explain past issues with materials durability. If sufficiently developed, these models may offer predictive power, helping shape the future experimentation—of course, this would require a greater monetary investment.
- It is too small and spread over too many topics to work as intended. This is a result of a genuine desire to contribute, not poor planning, but the end result is that effectiveness is compromised.
- This project is severely underfunded. This is a DOE Program Office problem, not a project problem.

Recommendations for additions/deletions to project scope:

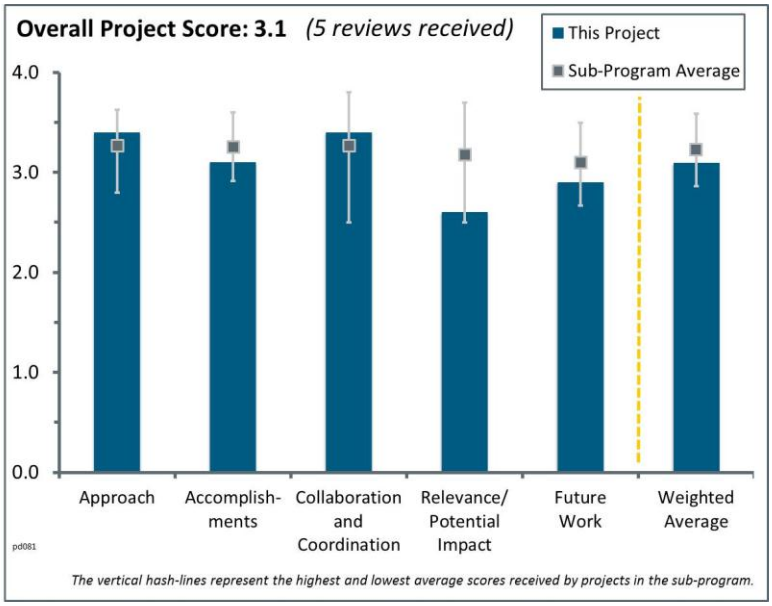
- It is recommended that the work be replanned to have at least one activity be critical mass. The most important thing to understand is corrosion mechanisms of passivated surfaces (N implant with metal impurities).
- The Surface Validation Team should invest adequate resources to enable development and implementation of studies of interfacial transport, chemistry, and energy states of PEC materials under operational conditions. A necessary element of this would entail continued development of in situ capabilities for atomic- and molecular-level experimental characterization of interface materials under operational conditions. This particular project should make every effort to establish broader ties with the semiconductor and catalyst communities in hopes of stirring interest and acquiring resources essential to the Surface Validation Team's ultimate success.

Project # PD-081: Solar Hydrogen Production with a Metal-Oxide-Based Thermochemical Cycle

Tony McDaniel; Sandia National Laboratories

Brief Summary of Project:

This project's goal is to develop a high-temperature solar-thermochemical reactor and redox materials for efficient hydrogen production based on a two-step, nonvolatile metal oxide cycle. Objectives in 2013/2014 include discovering and characterizing suitable perovskite materials for two-step, non-volatile metal oxide thermochemical cycles; developing particle receiver-reactor concepts and assessing feasibility; and constructing and testing a reactor prototype.



Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- The general approach of material selection in tandem with reactor design and performance modeling is an excellent example of process research and development (R&D). Thermodynamic modeling and framing of high-level system performance in terms of active material properties is an outstanding approach to candidate material identification and selection. The focus on technical barriers to solar thermochemical hydrogen (STCH) performance is outstanding as well.
- The approach is excellent but extremely ambitious, as it includes system analysis, materials discovery and characterization, and reactor design and development. If successfully completed, a two-step solar-driven water-splitting cycle will have been discovered, which offers significant advantages over ZnO and CeO_2.
- The speaker was very enthusiastic, and it appears that the people working on this project are involved and excited.
- This project is focused on the development of a two-step solar thermochemical process for hydrogen production based on the metal oxide redox cycle. The cycle is based on concentrated solar energy for high-temperature heat addition at ~1500°C and heat rejection ~1300°C. Improved cycle performance is achieved with low pressure during the reduction step (vacuum reduction). A cascading pressure design has been proposed with multiple reduction chambers for this purpose. The predicted levelized cost of hydrogen production is dominated by capital cost, so high efficiency is critical. Achievement of U.S. Department of Energy (DOE) cost targets requires a large decrease in capital cost and a significant improvement in solar-to-hydrogen (STH) efficiency compared to the 2015 case. Cycle performance is also dependent on achieving good gas-to-gas and solid-to-solid heat recuperation at high temperature (HT) (~1400°C), although this requirement can be reduced or eliminated with high-performance perovskites.
- As with the other STCH project (PD-028), this one combines materials improvements and process development. In general this is a good approach because materials and process are integrated, influence one another, and are both needed to assess economics. But, as for PD-028, it is fairly clear that the hydrogen productivity and kinetics of even the current materials Sr- and Mn-doped LaAlO (SLMAs) are the key limiting factors for the ultimate objective of efficient and cost-effective hydrogen production. Materials with much higher performance (likely over 10x, possibly 100x) are needed for that, and discovering them should be prioritized if economically feasible hydrogen production is the ultimate Hydrogen and Fuel Cells Program objective. It is good that the project in 2014 continued to search for better materials, but much more will be needed to achieve a practical technology.

- The high-throughput material screening is well done. Solid material movement for long periods of time is extremely difficult. Being able to move the material at HT and under low-pressure conditions is a significant risk.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- Accomplishments reported for this project are outstanding, while progress is somewhat tempered by failure to find a perovskite with better performance than SLMA. Nevertheless, the search parameters have been changed as new understanding evolved, and the researchers believe success is much more likely in the near future. The project demonstrated enhanced reduction under low oxygen partial pressure and determined that operation under vacuum conditions would be more cost-effective than use of an inert sweep gas. The new cascade design for the reactor enables much lower-pressure operation for reduction enhancement while reducing vacuum operating costs. Reactor design innovation for these improvements is outstanding. High-flux mirror testing was completed but may not be important for implementation in the redesigned reactor concept. Scheduling tasks in coordination with needed results would be a better way to proceed.
- Progress on materials discovery was satisfactory to fair. On other aspects, progress was excellent, especially for reactor design and secondary screening test development. The Hydrogen Analysis (H2A) analysis required another slide to justify huge reductions in capital costs with time. Over 85 materials have been synthesized, but only one SLMA was identified as promising. Very clever work that screens materials for their redox properties based on their enthalpy and entropy properties was described. One sentence on page 12 says that the researchers are confident that a perovskite can be found that will achieve the DOE 2020 STH efficiency target, indicating that the SLMA found and the others that appear promising are not sufficient. A better approach is needed to identify potential materials before synthesis and testing. The amount of material needed to produce one mole of hydrogen or two grams should be defined in clear terms. The molar ratio of hydrogen to steam in the outlet gases needs to be specified to get a feel of the amount and number of moles of active material needed to produce a mole of hydrogen. Then the durability has to be determined. Definitions of symbols should be included. For example, efficiency of gas–gas recuperation was not defined. The case for reducing capital costs by 50% in five years was not made. The ultimate cost target of about $2/kg is achieved with a 75% decrease in capital and operating and maintenance costs relative to the 2015 case. Again, the reviewer was not convinced that such a large cost reduction was possible.
- The project's attempt to build on the SLMA success to find better perovskites was appreciated. However, although no direct measurements were presented, it does not appear that hydrogen production and kinetics were improved vs. SLMAs. Finally, on the subject of materials, although it would be good to reach the goal of about 20% STH efficiency, this should not be the only target, as the economics, while affected by efficiency, depend on many other considerations, including hydrogen productivity and kinetics. Good progress has been made in analyzing the process and identifying impacts on efficiency, such as heat recuperation, effect of O_2 removal, and differences in Ttr vs. Tws. Also, the project should come up with an alternative reactor design to overcome limitations of the prior design. It is nice to see the process model and how it was used to identify how to achieve the desired efficiency. Again, as for materials, efficiency is only one figure of merit; other considerations should be added to understand whether the performance can achieve the goal of cost-effective hydrogen production. And of course, the process must be demonstrated, but it seems that materials development is much more important at this stage.
- The project team has identified a perovskite material that exhibits improved water-splitting cycle performance compared to CeO_2 and ferrites. Additional perovskite candidate materials have been screened. Even higher-performance material is needed in order to achieve the desired solar-to-thermal efficiency. A system analysis has been completed for a 100,000 kg/day central-receiver-based STCH production plant based on the redox concept. An engineering test stand has been designed to evaluate the HT vacuum reduction process.
- It is not clear why a delta of 0.3 yields uncommonly large hydrogen yields. It seems that any material with a delta in this situation would make the amount of hydrogen described; therefore, it cannot be uncommon. Achieving the 0.3 delta may be difficult, but that is different from uncommon. Having identified the

material properties is good, but this should have been done at the project inception, not the middle. Since the rate was determined, a cycle life should have been included in the target material properties. Operating the reactor at low pressure is a significant risk and will be more difficult than the presenter believes. It is not clear what will happen if the cascade reactor does not work. The H2A analysis needs to be included, and the assumptions need to be transparent.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- Collaboration is outstanding. Students conducting research at Sandia National Laboratories (SNL) have a great opportunity to use "real" equipment. Ideas generated at the two institutions give better integration of ideas. Continuing to work with Professor Nathan Siegel is a big plus as well, as he was involved early on in the development of the particle reactor.
- Collaboration and coordination with other institutions is good in this project. The collaboration with the University of Colorado has enabled continued progress toward that institution's goal, but the collaboration has not added significantly to advances in this project. The two institutions are pursuing competitive concepts, and the lack of value-added to this project is no fault of this project institution. At the same time, collaboration with the Colorado School of Mines has added value to the synthesis and screening of perovskite candidates, while solar field and interface design is facilitated by collaborations with Bucknell University.
- Collaborations are working on the right things and seem to be well integrated.
- This project is led by SNL with collaborators from Bucknell University (solar interface), the Colorado School of Mines (perovskite screening), and the University of Colorado.
- There are a lot of partners, but it is not clear what their contributions were. Partner roles should be better defined.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.6** for its relevance/potential impact.

- The promising results of preliminary investigations of real materials within the context of developed thermodynamic models and theoretical studies indicate for the first time that STH efficiencies targeted by the hydrogen production sub-program goals are achievable by STCH concepts. This is an outstanding achievement that deserves recognition.
- This supports the long-term goals for the Hydrogen and Fuel Cells Program.
- Restricting consideration to purely solar-based hydrogen production, STH efficiencies of at least 18% can be achieved with commercially available technology using state-of-the-art photovoltaics providing power directly to conventional water electrolysis units. With high-temperature steam electrolysis (HTSE), which is at an advanced stage of development (technology readiness level 5), an STH efficiency of at least 30% should be achievable using concentrated solar heat for the required HT heat addition (at 800°C instead of 1500°C) and state-of-the-art photovoltaics, even without system integration/optimization. This efficiency is three times higher than the 2015 case associated with the metal oxide thermochemical cycle and higher than the ultimate efficiency predicted for the concept. Furthermore, the materials challenges associated with this process are not to be underestimated. These comparisons beg the question of whether this technology is the best STH technology to pursue.
- Much like PD-028, as it stands today the project has poor prospects of meeting the goal of efficient and cost-effective hydrogen production. Although there were impressive advances with the SLMA materials compared to Ce oxides, the current productivity and kinetics of the materials would need significant improvement. The reviewer estimates that to achieve the target of 100 tons/day hydrogen production mentioned in the review material, the reactor will need to circulate something like 500 tons/minute of solids and have a solid inventory of over a 1,000 tons (using data on SLMA from 2013 and optimizing contact time for smallest solid circulation; also, solar intermittency is included in these calculations). These

numbers are several times those of the largest fluid catalytic cracker (FCC) reactor that the reviewer knows of and likely require on the order of $1 billion for just the reactor. (The reviewer used the FCC because it is a solid circulating reactor with established technology and economics). A number of smaller reactors could, of course, be built, but that increases cost by negating economies of scale. These considerations, not even including the cost of the solar tower(s) and the rest of the plant or the challenge of building such a massive reactor(s), led to the conclusion that unless much more productive materials are found, the prospects for economically viable hydrogen production using this technology are poor. For comparison, a 100-ton-hydrogen-per-day steam methane reforming (SMR) plant (sizeable but not a world-scale plant) would likely cost around $200 million. Also, CO_2 capture has been demonstrated at scale on SMR (Air Products, Port Arthur, Texas).

- The potential impact is uncertain. Reducing H2A costs to meet DOE's target appears unrealistic at best. The H2A results show a nearly fourfold decrease in total costs from 2015 to 2020 and a corresponding 50% decrease in capital costs. Reductions of this size typically require miracles. Cost increases for a project of this magnitude over a long time horizon are more likely. Therefore, the relevance of the project is in question because meeting DOE's ultimate cost target is unlikely.

Question 5: Proposed future work

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

- Proposed future work is outstanding and comprehensively addresses R&D tasks for new material discovery, reactor design and demonstration, reactor integration with solar thermal energy, and characterization of active material stability under operational conditions. These are all essential tasks for success by this project.
- Proposed future work includes continued materials screening, integration of multiple thermal reduction chambers into the engineering test stand, and design of centralized tower and field configurations. The proposed work represents a logical progression.
- It is good to finally see durability tests. The team has enough data at this point to know how many cycles the materials need to achieve. They should have presented that information. Moving the active material will increase the degradation. The durability tests need to take into consideration the reactor conditions, including material movement, in order to be relevant. The active materials still operate at very high temperatures. Materials that operate at lower temperatures while maintaining fast kinetics need to be discovered.
- The future plan has the right elements, but the team should focus the project on new materials. Also, the economic analysis should be strengthened.
- Materials represent a potential show stopper; more effort should be focused on materials. The future work described does not contain an innovative method to screen the huge composition space prior to synthesis. The screening method based on thermogravimetric analyzer results should reduce the work load. It is necessary but not sufficient. Future work includes implementing a durability testing protocol for redox active materials. This is critically needed but will be difficult. X-ray diffraction (XRD) was proposed as a method but will not be sufficient. XRD cannot be used to identify crystalline materials present at 5%–10%, as it is not sufficiently sensitive. XRD does not detect non-crystalline materials. A combination of XRD and Rietveld analysis, as well as surface area measurements, may be useful.

Project strengths:

- The professional credentials of the team are excellent, and each of the team organizations has significant experience in the work they will undertake. The facilities of all organizations are superbly equipped to undertake the proposed work. The proposed work, along with identified risks and remediation efforts, the facilities, and their alignment with the work, all combines to make it highly probable that the project will meet or exceed its promised objectives.
- The concept is very innovative. Significant contributions to the perovksite materials database have been made. Techno-economic analysis has been utilized to identify critical threshold operating points and efficiencies needed to meet ultimate DOE levelized cost requirements. A concept for integration of a cascading pressure design for low-pressure reduction has been developed. Many graduate students have been supported by this work.

- The project has good science and process innovation. The project has the right elements in place: materials discovery, process development, and economic analysis.
- The project has a good approach on materials discovery and good progress on goals.

Project weaknesses:

- This is a challenging route to the goal of cost-effective hydrogen production. Current materials have low hydrogen productivity. Solar intermittency increases cost substantially for any given production rate compared to continuous processes such as SMR. It is not clear that energy storage would help much without new materials, but it would be interesting to see the economic analysis
- They have a very complicated reactor system. They still have not presented durability data. Moving solids around at the rates, temperature, and pressure is extremely difficult, and the presenter seemed to downgrade the challenges. The active materials reaction temperatures are still very high.
- Aside from the redox material, many significant materials issues must be sorted out to make this concept feasible. For example, it is not clear what the reactor material is for operation at 1500°C.

Recommendations for additions/deletions to project scope:

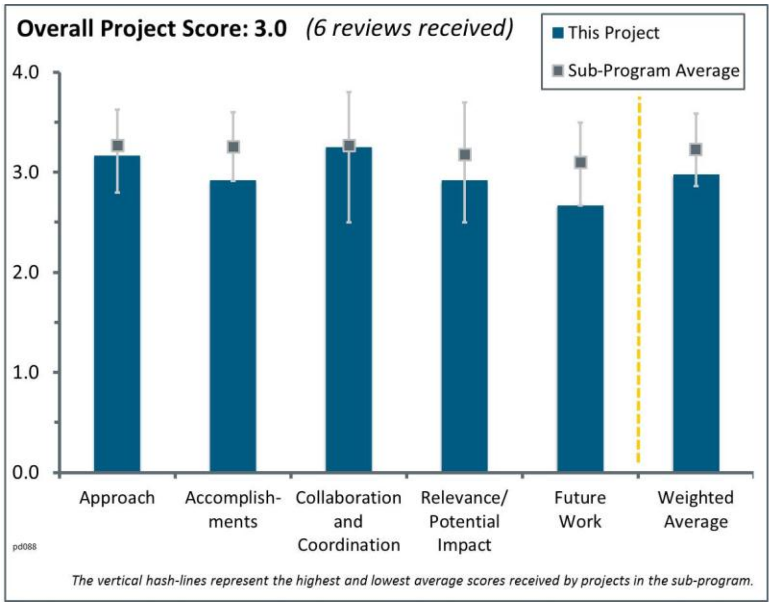
- Effort to characterize interface materials before and after thermochemical cycling and before and after reactor system cycling might prove useful in characterizing and/or resolving material durability issues.
- More emphasis should be placed on experimental demonstration, eventually leading to a fully integrated hydrogen production system.
- Increasing the focus on better materials and realistic economic assessments is recommended, and more details should be provided, as currently there is not enough information to assess the robustness of the reported cost of hydrogen. The project should deemphasize, but not totally eliminate, the reactor design effort.

Project # PD-088: Vessel Design and Fabrication Technology for Stationary High-Pressure Hydrogen Storage

Zhili Feng; Oak Ridge National Laboratory

Brief Summary of Project:

The objective of this project is to address the significant safety and cost challenges of the current industry standard steel pressure vessel technology by developing and demonstrating steel/concrete composite vessel (SCCV) design and fabrication technology for a stationary storage system of high-pressure hydrogen that meets the U.S. Department of Energy (DOE) technical and cost targets. SCCV technology integrates modular hydrogen storage system design, composite steel/concrete storage vessel for cost reduction, novel inner steel vessel design to eliminate hydrogen embrittlement, and advanced fabrication and sensor technologies for cost reduction and improved operation safety.



Question 1: Approach to performing the work

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

- Oak Ridge National Laboratory (ORNL) has proposed a novel approach for hydrogen storage using steel vessels and concrete. The approach uses bi-metal layers of stainless steel to prevent/reduce hydrogen embrittlement and pre-stressed concrete to provide strength for pressure containment, which is truly novel. The project now needs to better understand the issues associated with using and installing such vessels. The project needs to include these issues as part of the approach.
- The approach to diffusion experiment is simple and elegant. The team is addressing cost barriers toward the target and making good progress.
- The methodical approach was sound, and the methods on proving the viability of the design and manufacturing process required were also good. How and where this technology from a practical hydrogen forecourt station perspective could be used was not presented, and this prevented a 4.0 rating.
- The approach looks very practical.
- The principal investigator (PI) has taken a reasonable approach to the problem.
- The authors state that a secondary advantage of reinforced, pre-stressed concrete is protection against third-party damage. However, this is more compelling than their claimed primary advantage. The tension elements must stress the concrete and then supply additional stress to augment the strength of the internal bottle of steel. This means that not all of the strength can be used for containment.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- There is good progress to date. The project now needs to address the issues associated with installation and how the orientation and installation will affect the competitive cost of the storage system.
- Preliminary work aimed at fabrication has progressed well. It is not clear how fabrication costs are determined. Hydrogen permeation rate data show good results for layered design.

- As many of the prospective refueling stations will have space considerations to account for, it appears that with the reinforced concrete these vessels will take up more space than conventional storage vessels. Cost analysis is impressive and well documented and would present an option for significant cost savings for forecourt storage in stations with space. The hydrogen permeation mitigation technology with the weep hole is a novel approach that would prove to be a significant safety improvement along with cost, as shown in the PI's tables. The fabrication technique and proven results of the stir welding process are impressive.
- Concrete is the key to the approach, yet no physical work has been done with this material, despite an ample budget. Instead, the authors have patented an instrument for fatigue testing of metals, which was not a goal. Patents are generally more useful for consumer or industrial products produced in large quantities (in this case, the SCCV tanks) than for a specialized low-cost scientific instrument.
- It is not clear if there has been much progress since 2013.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The project has made excellent use of collaborative partners. ORNL has organized a group of subject matter experts who have made the project a success to date.
- There is broad collaboration between multiple parties, and this is proven within the presentation and is exemplary. This is clearly demonstrated by the PI.
- The project team has performed good work in lining up collaborators.
- There is strong collaboration evidence.
- The partnership with MegaStir is substantial and productive. The other partners/interactions seem superficial. In particular, it would have been good to see a publication with University of Michigan utilizing their listed competency, high-performance concretes. The California Fuel Cell Partnership would be an excellent partner; however, there is no evidence provided of their level of interest.
- It is not obvious that ORNL has collaborated with entities other than those immediately required to design and fabricate the test vessel.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.9** for its relevance/potential impact.

- The project addresses storage needs and cost barriers with available materials and fabrication techniques.
- Hydrogen storage for stationary use is a major cost factor requiring a breakthrough to make hydrogen delivery and storage more cost-efficient. This project provides a technical option for helping reduce the cost of storage.
- Stations are now funded and being built in California. Less expensive storage, or at least the prospect for it, would significantly accelerate the rollout.
- The relevance of this type of technology is unclear because of the physical space required (more than existing technology); a layered inner vessel will require welding, which will add to manufacturing cost—not decrease it. The potential impact is also unclear and has not been effectively communicated by the PI. The project should be compared to existing high-pressure storage for which there are significant data available. From a multi-year research, development, and demonstration plan technical and cost target perspective, the work done by the PI works to meet these targets as written—but in practice, the space consideration of real stations, especially in most early adaptation scenarios for station deployment, will be in high-traffic urban areas where space is an issue. The addition of a significant concrete reinforcement will—while perhaps lowering cost—compound the space issues.
- The reviewer fails to see the relevance of the concept in comparison to established fiber-wound storage technologies. There were no references to National Fire Protection Agency standards regarding setback distances. It is unclear whether this project will offer advantages over a standard tank.
- The projected costs are no better than existing costs for Type II steel tanks.

Question 5: Proposed future work

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

- The fact that the project is evaluating new welding technology for multi-layer strength is appreciated.
- The next phase work seems ready to reduce to practice.
- The proposed work seems good. The project should add significant work to prove the concept for greater than 700 bar pressure storage.
- Economics need to be rigorously investigated and compared to existing technologies to ensure that concrete reinforced tanks have real potential to be competitive. Fabrication and installation costs need to be fully understood for these more complex tanks.
- For future work to include a manway, a cost analysis should be done to see if the extra cost of the manway will make this storage option uneconomic with competitive storage options.
- It is not evident that a manway is useful. Progress in imaging in the visible and infrared, together with developments in robotics, should counter the need for direct human observation and exposure. It would be preferable to see tests on multiple small mockups.

Project strengths:

- The project team had a strong technical presentation. Well-presented and technical assumptions and approach are sound. The cost analysis approach was well presented. The project has the potential to provide a significant low-cost option for stations without space considerations. The PI has a clear understanding of the technology and its technical merits (and challenges).
- The welding of the multi-layers is a project strength.
- Less expensive, large-scale storage is critically needed. This is an important topic that needs to be solved.
- The project's application of existing technology to address the need for low-cost storage is a strength.
- A strength the project offers is a unique proposal using two media (steel and concrete) to provide low-cost hydrogen storage.

Project weaknesses:

- The project should continue to explore the cost effects of installation and orientation of installation, as well as the cost effects of a manway.
- Nondestructive examination for layered-up welds has not been presented (assuming 100% x-ray for head-to-shell joints), and some leak testing or magnetic particle exams for the layer welds would also be helpful for the project. What is/has been done, what would be done to the proposed technology, and how this affects delivery and cost was not presented by the PI. The cost of the manway is unclear. Engineering sense says this could add 10%–15% to the overall cost because of required construction techniques. It is unclear how the expected cost reduction is to happen—meaning is it unclear whether it is from improvements in manufacturing efficiency or economies of scale. This is the question across the spectrum of low- to high-pressure storage.
- There is a lack of relevance to the fuel cell electric vehicle market.
- The case for concrete is not well articulated. It should be possible to explain this more plainly. A complicated spreadsheet and calculation, while necessary, is not complete in itself—an interpretation is needed.

Recommendations for additions/deletions to project scope:

- An estimate of the cost for a small mockup and comparison of the actual costs of fabrication would be more convincing support for the concept than detailed modeling.
- Cost analysis of installation should be added.
- The project should examine other methods to fabricate heads—a significant cost. Perhaps it is possible to fabricate and inspect without a manway (a cost improvement). Transportation to the final installation site should be addressed—the steel model shown on page 19 may exceed common International Organization for Standardization (ISO) container dimensions for intermodal transport; it will be an oversize load. The

ISO container outside floor dimensions are 8 ft by 20 ft/40ft. Please address any installation site corrosion issues—it is not clear if paint is enough. It is not clear how to protect diffusion paths from closing up as a result of moisture and corrosion.

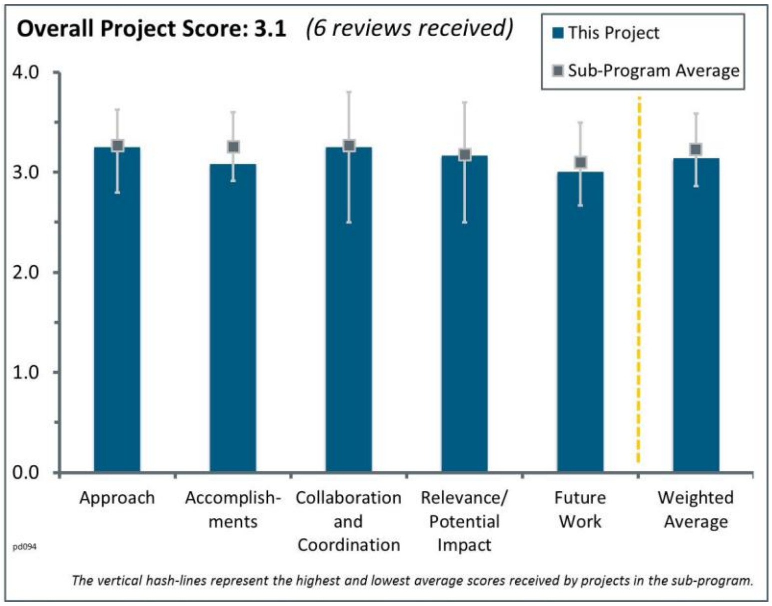
- Addressing delivery and supply chain issues would be great, as early/midterm adoption of the proposed technology will require better delivery and strong supply chain streamlining. Finding ways to simplify the layering technology and reduce welding present in hydrogen affected areas will be important. Further in the high-pressure case, the welding of the "ends" or covers for manways will be a key challenge to the technology. In many and most hydrogen applications, welding in these high-pressure applications is frowned upon and even forbidden. How the welded area responds and holds up to cyclic conditions must be analyzed. For the proposed work for fiscal year (FY) 2016 and beyond, the project should study how the large physical size will affect the utilization of this type of technology in forecourt stations deployment. Underground storage is a good idea—but perhaps not practical in most station scenarios in the near midterm. Of critical importance is demonstrating this technology's capabilities on greater than 700 bar storage. It is unclear if it scales up well or if the welds hold up. It is unclear how cyclic conditions will affect the technology (perhaps in FY 2016 and beyond).
- In this reviewer's opinion, project funding should be discontinued, since it does not address onboard storage.

Project # PD-094: Economical Production of Hydrogen through Development of Novel, High-Efficiency Electrocatalysts for Alkaline Membrane Electrolysis

Katherine Ayers; Proton OnSite

Brief Summary of Project:

The objective of this project is to demonstrate a technology pathway to reduce the cell stack capital cost and resulting hydrogen production cost of alkaline membrane electrolysis. The approach is focused on synthesizing a stable oxygen evolution reaction catalyst to enable low-cost flow fields for reducing the cost of anion exchange membrane operation.



Question 1: Approach to performing the work

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

- This highly innovative project is focused on the development of anion exchange membranes (AEMs) for water electrolysis. These membranes are an alternative to polymer electrolyte membrane (PEMs). They are conductors of OH-ions, rather than H+ ions. A typical AEM is composed of a polymer backbone with tethered cationic ion-exchange groups to facilitate the movement of free OH-ions. The potential advantages of this technology include the use of stainless steel bipolar plates instead of Ti, and lower-cost catalysts. The AEM material candidates include polysulfone, polyphenylene polymers, and others. The catalysts under consideration are in the pyrochlore family, including $Pb_2Ru_2O_7$ (lead ruthenate). These catalyst materials have been shown to support fast kinetics for the O_2 evolution reaction and are stable in alkaline solutions. They are also amenable to production as nanoparticles. Membrane and catalyst development are major thrusts of the work. A porous Ni gas diffusion electrode (GDE) has also been developed.
- The alkaline membrane electrolysis is a promising approach to developing lower-cost electrolyzers since non-precious metal catalysts can potentially be used. This project is taking an excellent approach in developing electrocatalysts and integrating them into alkaline membrane electrolysis cells. The main issue with AEM electrolyzers is membrane durability. Membrane durability (without carbonate recirculation) should be the main focus.
- The approach is well-reasoned, starting with catalyst and membrane development and ending with electrode development and testing. The catalyst approach is investigating ABO(6-7) pyrochlore materials. The A constituent is Bi or Pb, and the B is Ru and Ir. The reviewer wonders whether there is more of the catalyst space that could be explored with rapid throughput screening similar to the approach used at The Joint Center for Artificial Photosynthesis (JCAP). Proton Onsite should ask itself if Bi, Pb, Ru, and Ir are really the best materials for the job and what it would take to explore the space further. Determining the potential value in doing so is also important. The integration approach is laid out logically and contains a vision for a product at the end.
- It appeared from the discussion that progress has been made on each task in the past year. The future work plan was explained in a reasonable fashion.
- The approach is reasonable in light of the long-term nature of this project. However, this project addresses a very small fraction of the total hydrogen cost pie, and its impact is inherently limited.
- Alkaline membrane electrolysis was proposed as a means to reduce capital costs associated with polymer electrolyte membrane (PEM)-type electrolysis. For example, replacement of Ti with stainless steel for the flow fields will reduce capital costs. However, based on the information in the presentation, many aspects of the alkaline membrane electrolyzer need to be modified or replaced, such as membranes, catalysts, and

electrodes. Corrosion is an issue, and degradation mechanisms need to be investigated. So while capital costs for one aspect of the cell should decrease, other costs may increase. Electricity costs represent 65%–80% of the total costs of hydrogen in PEM electrolysis as discussed in the talk by Dr. Colella (PD-102). In addition, this talk also demonstrated that future stack costs are expected to be less than half of the current stack costs. Therefore, the focus of the work on reducing capital costs does not appear warranted.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- Significant progress is made in all fields of work. The stable anode GDEs in the AEM cell are especially impressive.
- The project has accomplished good progress with the catalyst and membrane. If ultimately successful, this project has the potential to overcome some of the limitations of PEM electrolysis. The durability test to date does not seem to include the entire electrochemical package, only the membrane with the default Nickel/Cobalt catalyst. It would be good to have a durability test with the proposed final, total electrochemical package. It is important to understand the system and cost implications of using carbonate in the electrolyte, unless the performance can be improved without it. It is far better than using KOH but may not be as simple as PEM.
- It was a good talk. Proton OnSite's results are impressive. The catalyst investigation was minimal, but this was dictated by priorities and reviewers' comments from last year. Several types of membranes, alternate polymers, and undefined materials from Sandia National Laboratories (SNL)/Los Alamos National Laboratory are being investigated. Characterization work is ongoing, but it appears that the membrane choice has not been finalized. In general, the temperature stability of hydrocarbon membranes is relatively low. A durability test using Proton Onsite's electrode, a commercial membrane, and an Illinois Institute of Technology (IIT) anode catalyst showed stability for 200 hours, but cell voltage was higher than the goal, while the current density was lower. Nevertheless, this represents progress. No details were provided for the efficiency calculations for the alkaline membrane electrolyzer vs. the PEM electrolyzer. More details and better definition of the take-home message on each slide are needed for non-experts to fully appreciate the work.
- An AEM bench test stand has been developed. Stable performance of an AEM and ionomer in the electrode layer has been demonstrated. Lead ruthenate oxygen evolution reaction (OER) catalyst activity has been shown to be superior to IrO_2, using the rotating disk electrode method. The performance of this catalyst has also been demonstrated in-cell. Cell efficiencies as high as 79% (at 200 mA/cm^2) have been achieved using an experimental alternate catalyst. Membrane development activities include anode and cathode binder synthesis. Stable gas diffusion electrode behavior was demonstrated in-cell using a porous nickel GDE. The performance effect of carbonate in the feedwater has been quantified. Carbon ingress from atmospheric air is a potential issue. Stack design and flow modeling has been performed. Degradation mechanisms are not well characterized.
- The project successfully synthesized a higher-activity catalyst. However, the project team appears to be behind schedule in accomplishing milestones, including initial ionomer composition screenings, a cost/strength material assessment of alkaline compatible stack materials, and electrode fabrication.
- The limitations to the current technology still seem formidable. Although a plan is in place to make further progress, the progress should be greater than it is.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The project is making good use of SNL, IIT, and fundamental Advanced Research Projects Agency-Energy (ARPA-E) work. Perhaps some of the activities discussing hydrogen embrittlement evaluation could be helped by working with the materials group at SNL's Livermore, California, location. That may make the evaluations unnecessary, or they may be able to limit the choice of candidate materials. Also, the team should investigate working with JCAP on catalyst screening.

- Collaborations between Proton OnSite and IIT appear to be working well.
- The project team included industry, national laboratory, and university participants. The project was led by Proton OnSite, with collaborators from SNL and IIT. Proton OnSite provides cell and stack design, system, and testing expertise. SNL has supplied AEM materials based on work performed under an ARPA-E program. IIT is investigating alternate polymer membrane materials.
- As this project deals with a technology concept that is longer term, broader collaboration with multiple skill sets may be beneficial at this stage of the development.
- The interaction with the IIT partner was included in the discussion but could have been further explored. Work with SNL was not clearly developed.
- There was insufficient information to provide a higher grade, but collaboration was shown to exist, and partners are well coordinated. It would have been helpful if the institution doing the work was identified.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- The relevance of the project is good. One of the important issues facing PEM electrolysis is the capital cost of the system. This project has the potential to lead to a step-change in system cost. It takes a variety of novel approaches at all of the most-costly pieces of an electrolysis system.
- Successful completion of this project does have significant potential value in demonstrating the exploitation of the technology.
- The project is targeting the main challenges of the hydrogen production targets for electrolysis: reduction of the high amounts of expensive anodic platinum group metal catalysts currently needed and reduction of electrolyzer capital cost by eliminating expensive materials such as Ti separator plates and Ti current collectors.
- Increasing efficiency and reducing capital costs will advance progress towards the hydrogen economy. These attributes are critical to meet the goals defined by DOE. However, the best case calculation for efficiency that was presented is not sufficient to meet DOE's target, primarily because of the electricity costs (see the PD-102 presentation).
- This technology is in the early stages of development, but it appears to have potential for capital cost reduction in the area of bipolar plate and catalyst materials. The cell efficiencies will not be as high as PEM technology, however. Furthermore, stack capital cost contributes only ~10% to the total hydrogen production cost associated with PEM electrolysis. So capital stack cost savings will not make a major impact on hydrogen production cost by water electrolysis.
- Assuming the stated potential savings of $0.11/kg hydrogen with alkaline membrane electrolysis, the potential impact of this project is relatively small—at best 2% of the current hydrogen production cost of $5.00/kg.

Question 5: Proposed future work

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

- All of the proposed future work at IIT and Proton Onsite is good and important, especially the characterizing of the ionomer decomposition mechanisms and the corrosion study for titanium, stainless steel, and nickel porous micro-layers.
- The future work is well planned. It is important to perform some analysis with the Hydrogen Analysis model (H2A) to provide some guidance on how low this technology can push the hydrogen production cost relative to PEM. The planned operational testing of the complete stack/system should focus on durability, degradation, and efficiency.
- Proposed future work includes basic research tasks to be performed at IIT: membrane development and characterization, bipolar plate corrosion studies, and characterization of degradation mechanisms. Proton Onsite tasks include stack and system development. The future work plan is well reasoned.
- The proposed future work is adequate but would benefit from acceleration.

- The future work looks reasonable.
- It is too early to focus on stack development. The challenges associated with the cell components, such as the membrane, catalysts, and electrode, should have a higher priority. However, in view of the data in PD-102 that indicate electricity is the primary cost driver, the focus should be changed to a study to determine whether the efficiency of alkaline membrane electrolysis can be significantly higher than that of PEM electrolysis, and that the potential to meet DOE's targets exists with this technology.

Project strengths:

- The project takes a number of novel approaches to redefine what an electrolyzer can be. The team seems creative and effective at breaking down barriers.
- Excellent research was done by the individual partners in their respective fields of expertise.
- The principal investigator appears to be very knowledgeable with regard to water electrolysis science and technology. This highly innovative project is focused on the development of AEM for water electrolysis. These membranes are an alternative to PEM. They are conductors of OH-ions, rather than H+ ions. The potential advantages of this technology include the use of stainless steel bipolar plates instead of Ti and lower-cost catalysts.
- Leveraging the strong PEM-based skill set from the Proton OnSite team is a project strength.

Project weaknesses:

- The project does not provide contextual impact with respect to overall projected hydrogen cost. The stated potential of 80% material cost reduction over baseline of OER catalyst may be true, but it is a very tiny slice of the overall cost.
- Expected cell efficiencies will be lower than PEM efficiencies (but higher than alkaline electrolyzers). The potential capital cost savings associated with the use of stainless steel bipolar plates and lower-cost catalysts will not contribute significantly to the levelized cost of hydrogen production, which is dominated by feedstock (electricity) cost.
- From the results, the advantages over the classical advanced alkaline electrolysis are not seen, except the unneeded KOH.

Recommendations for additions/deletions to project scope:

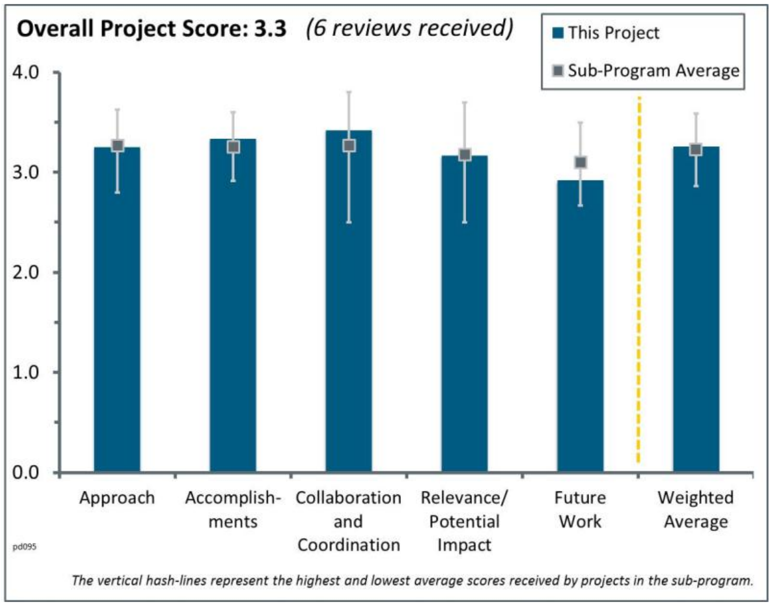
- The project should identify the potential niche applications for alkaline membrane electrolysis, and build relevance and economics accordingly.
- The project should focus more on fundamental research for membrane performance and durability issues before getting more involved in stack and system development.
- The project should do the following:
    1. Work with SNL Livermore on hydrogen embrittlement.
    2. Investigate working with JCAP on high-throughput catalyst screening.
    3. Perform an H2A analysis to investigate the hydrogen production cost from a commercial system.
    4. Perform durability testing on a completely integrated electrochemical package with the best catalyst and membrane that have been developed.
    5. Evaluate the cost/commercial implications of carbonated electrolyte or find a way to do without it.

Project # PD-095: Improving Cyanobacterial O_2-Tolerance Using CBS Hydrogenase for Hydrogen Production

Pin-Ching Maness; National Renewable Energy Laboratory

Brief Summary of Project:

The objective of this project is to develop a robust O_2-tolerant cyanobacterial system for light-driven hydrogen production from water while increasing system durability. This objective is divided into two tasks. Task 1 is to probe hydrogenase maturation machinery in the Casa Bonita Strain (CBS) of *Rubrivivax gelatinosus*, and Task 2 will express the more O_2-tolerant CBS hydrogenase in *Synechosystis*. The long-term goal is for cyanobacteria to be O_2-tolerant for eight hours (during daylight hours).



Question 1: Approach to performing the work

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

- The approach is well organized, using several researchers to systematically evaluate the CBS genome and the functions of several maturation genes in order to select the optimal genes to co-transform with O_2-tolerant hydrogenase into *Synechocystis* for increased hydrogen production activity in the presence of oxygen (O_2).
- This project is well focused on the oxygen accumulation barrier in a cyanobacterium and complements work by Dr. Maria Ghirardi with algae. The approach to probing the hydrogenase maturation machinery is commendable, as well as the promoter tuning strategy to improve hydrogenase activity in *Synechocystis*.
- There is a strong rationale and strategy for development of promoters for expression of CBS hydrogenase in *Synechocystis*.
- The approach is logical. The step taken to assess the lack of product for the tagged product was well thought out. The evaluation of the function of *hyp1* and *hyp2* followed. The knock-out evaluation has the potential to be challenging, but the investigator appears to have a clear approach to overcome the challenge.
- The project is focused on addressing the O_2 inhibition of the native *Synechocystis* hydrogenase. The project objectives are appropriate for understanding the ability of the CBS hydrogenase to overcome the limitations of the O_2-sensitive hydrogenase. Despite this, some of the experimental design glosses over critical experiments to add to the body of knowledge for the enzyme activity.
- The approach is clear and seems suitable to the objective as stated. But the objective itself is narrow, and it is not put into the broader context of producing hydrogen in a cost-effective manner. The problem tackled here is only one of the pieces needed to make hydrogen, but there is no information about the relative importance of this piece vs. the others (e.g., photosynthetic efficiency). It is perfectly fine to work on a piece of the whole in parallel, but context would be useful to assess whether the whole is worthwhile to begin with.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- There are multiple notable accomplishments, including the identification that *hyp1* genes cluster near the O_2-tolerant hydrogenase and have a similar induction profile to CBS hydrogenase whereas *hyp2* proteins cluster near the hydrogen uptake hydrogenase. Additionally, other maturation genes and their functions, in particular *slyD*, were identified in the CBS genome. Combinations of these genes and the O_2-tolerant CBS hydrogenase were successfully transferred into the host. But it is unclear why the tetramer was selected over the hexamer.
- Most milestones have been met. One delayed milestone is on track, as are the future milestones.
- With the exception of a one-quarter delay in Task 1's subtask to determine *hyp2*'s effect on hydrogenase activity, subtasks that have been accomplished appear to have been completed on schedule, and the remaining subtasks appear to be on track.
- The accomplishments on the various tasks appear to be satisfactory.
- Efforts were highly focused on strategies to insert and express CBS hydrogenase in *Synechocystis*. No actual hydrogenase activity or hydrogen production data to date were shown, although the task was scheduled to be completed by September 2014. There were two publications and three presentations resulting from the work during the reporting period.
- Qualitatively, it looks like good progress. But there are no data on hydrogen production, so it is not possible to judge whether the work led to progress. Based on duration of continuous hydrogen production (not the best or clearest metric for several reasons), the target is still far away.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- There are multiple collaborators with clearly identified roles, and their work is nicely integrated.
- There are strong academic collaborations.
- The project has partnerships with the J. Craig Venter Institute—Task 2, expression of CBS in *Synechocystis*—and the laboratories of Dr. Jin Chen (Michigan State University) and Dr. Jonas Korlach (Pacific Biosciences)—Task 1, probing CBS hydrogenase maturity machinery.
- The accomplishments were made through coordination with external partners.
- The collaboration on this project is sufficient.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- Development of a robust, O_2-tolerant CBS for solar-driven production of hydrogen from water represents a significant advance in photobacterial hydrogen production and progress towards Multi-Year Research, Development, and Demonstration Plan objectives. This project appears to be well focused upon identifying the genes involved with expression of an O_2-tolerant hydrogenase in *Synechocystis* and tuning expression levels to achieve adequate activity.
- There were no issues; the research is highly relevant to the DOE Hydrogen and Fuel Cells Program (the Program).
- If the right combination and levels of maturation genes and O_2-tolerant CBS hydrogenase are transferred into the *Synechocystis*, this could lead to hydrogen production that meets both cost and volume targets.
- The tasks outlined in the presentation have the potential to support Program objectives. However, there are concerns about the experimental design underpinning the tasks, which may make it difficult to meet milestones and prove process robustness.

- While the impact of this work is likely high, it was not possible to discern the high-level impact from the presentation. That is likely more a reflection of the presentation and less a reflection of the work.
- This is only a piece in producing hydrogen from water splitting by microorganisms. In isolation, it is hard to assess whether even complete success would help the final goal.

Question 5: Proposed future work

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

- The future work proposed includes the logical next steps. Much of the work has preliminary data that indicate success is likely.
- The project's proposed work is a logical continuation of current work.
- The future work seems appropriate for the narrow objective.
- While the proposed work has the potential to meet the project goals, there is concern that some of the experimental design is skipping crucial steps to better understand the nature of the CBS hydrogenase and its maturation pathway. For example, the CBS *slyD* homolog shares only 33% identity with the *E. coli* protein, and it is unclear from the presentation how much of the identity occurs within the catalytic domain/active site of the protein. A better understanding of the protein function of the CBS *slyD* is necessary before transforming it into *Synechocystis*. It is quite likely that the protein will not have the same activity and that this will be very difficult to troubleshoot without the fundamental knowledge of protein sequence, structure, and activity.
- More detail is requested on plans to "demonstrate in vitro and in vivo hydrogen production, the latter linking to the host photosynthetic pathway."

Project strengths:

- The principal investigator and team have a strong understanding of barriers to effective biological hydrogen production and have targeted a possible solution through engineering an O_2-tolerant hydrogenase into *Synechocystis*. The project milestones have been met or are on track to be met. Some project risks have been assessed and are being addressed. Previous weaknesses have been or are being addressed through current and future work.
- The organization of work is a key strength.
- The biology expertise is a strength.
- The project has a logical approach to address the stated objective. The investigator appears to have a clear vision for the project.
- A detailed description of progress was presented on the development and implementation of strategies to insert and express CBS hydrogenase into a non-hydrogen-producing strain of *Synechocystis*.

Project weaknesses:

- Some of the experimental design is questionable—more foundational knowledge about protein function should be determined before moving forward with some of the experiments proposed. The previously mentioned *slyD* experiment is one example. There are also concerns about the relative amount of focus on identifying and understanding the *hyp1* and *hpy2* deletion mutant—it is quite possible that this will not be as straightforward and should be emphasized more over transforming the proteins into *Synechocystis*. Some of the experimental rationale and results were not made clear. It was unclear how the evidence presented demonstrated that the affinity tag did not disrupt protein function. Relatedly, there are alternative methods for troubleshooting and problem solving that were not explored or explained.
- There may be other steps in the pathway that can limit hydrogen production even if the problem at hand is successfully solved.
- The big picture goals were not clearly stated. It would have been helpful for the reviewers to know what end applications are being targeted because this work does not appear to have just a basic science focus. The 2013 presentation had a large focus on the evaluation of promoters, but there seems to have been minimal progress in that area.

- At present, there are no results to demonstrate hydrogen production from the recombinant *Synechocystis* to determine whether the very detailed genetic engineering strategies will be successful.

Recommendations for additions/deletions to project scope:

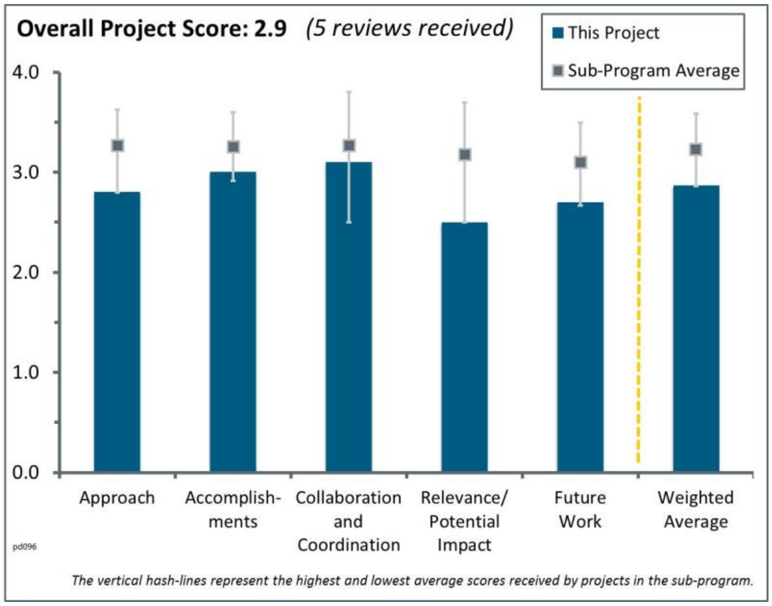
- It is recommended that some of the work be refocused to understanding protein function and activity in either the native organism (CBS) or *E. coli*, using deletion mutants where possible. Much of the work appears to be putting the cart before the horse by transforming the proteins into *Synechocystis* before having a good foundational understanding.
- A better definition of the entire pathway and overall potential for hydrogen should be added.

Project # PD-096: Electrolyzer Component Development for the Hybrid Sulfur Thermochemical Cycle

William Summers; Savannah River National Laboratory

Brief Summary of Project:

The overall objectives of this project are to develop improved technology for the hybrid sulfur (HyS) thermochemical process to permit low-cost, highly efficient hydrogen production from concentrated solar energy and to focus on the HyS SO_2-depolarized electrolyzer using polymer electrolyte membrane (PEM) technology. Fiscal year 2014 objectives are to identify and quantify performance of anode electrocatalysts and advanced PEMs; to address the challenges of faster reaction kinetics, high specific output, elimination of sulfur formation, and longer operating lifetime for the sulfur dioxide (SO_2) depolarized electrolyzer; and to demonstrate improved components through button-cell operation at increased temperature and pressure.



Question 1: Approach to performing the work

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

- The team had a good presentation. The approach is sound as it is critical that the electrolyzer in the HyS cycle operate at higher temperatures and at elevated pressures in order to meet the U.S. Department of Energy's (DOE's) cost targets. Demonstration of higher-temperature operation for the electrolyzer will show that it is possible to reduce cell voltage and decrease electric power consumption. However, other aspects of the work, e.g., high-temperature (HT) membrane development, must also be addressed.
- The approach to work in this project is good. Only one of the three barriers to performance was addressed specifically, although the particular task engaged was related to the other two barriers. The remaining barriers to cycle performance are nonetheless significant, and resolution of the barrier to efficient and cost-effective electrolyzer performance is not in and of itself sufficient to assure HyS performance. The funding level for the work was limited so that there were insufficient resources to undertake a broader set of tasks. Earlier assessment and laboratory efforts identified the electrolyzer as the critical barrier to improved performance, so the effort was appropriately focused on that topic, given resource constraints.
- The team is working on the right technical improvements for this process but is unclear on the basis for success. Particularly, it is unclear why a solar-based thermal process is better than other electrolytic pathways. Some advantages of this particular approach are highlighted (mostly lower potential compared to water electrolysis), but it is not clear how this translates into higher efficiency, lower cost, increased reliability, etc. Solar thermal heat is expensive to get and use. It is not clear what other advantages of this process are believed to compensate for that investment and how they do so quantitatively.
- Polybenimidazole (PBI) membranes are known to have durability issues. While they may have operated well in the short-term tests, it is unlikely they will in long-term tests (thousands of hours). A different material should have been used, and if not available, this could be an area of development. The theoretical potential was 0.16, and they are much higher than that (0.4 V). The voltage reduction goal seems moderate. It is good to see higher pressures being examined. An analysis using the Hydrogen Analysis model (H2A) should have been included in this work.

- The objective of this project is to develop and demonstrate improved component technology for the HyS thermochemical process. Along with the sulfur-iodine thermochemical process, the HyS process was originally developed by Westinghouse for coupling to nuclear energy for large-scale hydrogen production. As it is cast for this project, it could also potentially be coupled to concentrated solar energy for the HT heat input (the sulfur-iodine thermochemical process and HT steam electrolysis could also be coupled to concentrated solar for the required HT heat addition). The electrical requirement can be provided by the grid or photovoltaics. The HyS process includes an electrolysis step that operates at ~100°C in which sulfur dioxide and water react electrochemically to yield sulfuric acid and hydrogen in a SO_2-depolarized electrolyzer. The presence of SO_2 on the anode side reduces the open-cell voltage significantly to ~0.16 V. However, a significant activation overpotential must be overcome to drive the electrolysis cell with reasonable current densities such that practical cell voltage is ~0.6 V. A HT heat addition process in which SO_2 is regenerated by sulfuric acid decomposition is required to close the cycle. The electrolysis cells use PEMs, including PBI for HT (130°C) pressurized operation. Precious metal catalysts are required on the SO_2–sulfuric acid side of the cell. The work performed under this project addresses only issues associated with the electrolytic step of the overall process. The significant materials and reactor design challenges associated with the sulfuric acid decomposition step are not addressed.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- Accomplishments under the restricted funding were outstanding. Progress, however, was less impressive mostly because anticipated reduction in electrolysis power was only partially realized. Purported improvement through higher-temperature operation was demonstrated, although cell voltage required for 500 mA/cm^2 remained higher than 600 mV. The potential improvement from advanced catalysts is somewhat constrained because all improved catalysts are noble metals, so cost effectiveness might not be realized. The electrolyzer work focused entirely on sulfonated polybenzimidazole (s-PBI) membranes, and concerns regarding the long-term performance of this material in highly acidic electrolytes were expressed and not countered by the presentation.
- The project has made good progress in finding new catalysts with lower overpotential but did not quite reach the target of 100 mV reduction. It is unclear if this is enough for now until membrane electrode assembly (MEA) is fabricated and if further progress towards the target will come from other parts of the process or if further work on catalysts is critical. It is not clear if there are additional catalyst leads. The new PBI membrane looks very promising and seems critical to achieve targets. Questions were raised at the review about the stability of PBI and should be addressed. It is unclear what other leads there are should PBI not be suitable. The Pressurized Button Cell Test Facility (PBCTF) looks to be an important tool for this project, and good progress was made in building it. Researchers recognize that this process needs to be cost-effective, but no information or metrics were presented to judge.
- Improvements were achieved, meeting project goals. The higher current was good. It would have been nice to see how the improvements achieved would decrease hydrogen production costs. An updated H2A analysis would have been useful. The catalyst loading needs to be clearly stated. The major problem is that progress was made using a membrane that is likely not to be viable owing to likely limitations on its durability. PBI may work well for hundreds of hours, but not for the thousands of hours that will be required for this process to be viable. Phosphoric acid doped PBI has a history of problems during cycling on and off. It is likely that the version being used here will have the same issues.
- Good work has been made on screening potential anode catalysts. Good progress on completing fabrication of the pressurized button-cell has been made. The task of designing and fabricating a button-cell that operates at elevated temperatures and pressures is extremely difficult. Parts must be custom made and fabricated from corrosion-resistant materials for the very aggressive chemicals. Preparing a plan that meets all aspects for safe experimental work is time-consuming and is an essential task that is not fully appreciated by many. A promising membrane, s-PBI has been identified, but further development work is still required. The effort to complete this work by the end of 2014 appears gargantuan, and for this reason, the grade is a 3 because of the timeline.

- Technical accomplishments reported to date include development of improved electrocatalysts (Pt-Au alloys), screening of catalysts in acid solution, development of a pressurized button-cell test apparatus, and electrolyte membrane development at the University of South Carolina. The accomplishments were reasonable, considering the relatively low funding amount for this project ($300,000).

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- Collaboration with the University of South Carolina is excellent, as membrane development work for this project has been ongoing and supported by internal funds.
- The project was led by the Savannah River National Laboratory (SRNL) with collaboration from the University of South Carolina. SRNL was responsible for the catalyst screening activities and cell testing. The university was involved with electrolyte membrane fabrication and characterization, including the development of advanced membranes, such as s-PBI.
- There was good collaboration with the University of South Carolina. The roles were clearly defined.
- This project has had a strong history of effective collaboration and coordination with other institutions and researchers. The current funding environment has affected this history so that, presently, the project includes a single partner. It is acknowledged that the choice of partner is consistent with use of s-PBI membranes, but it is not clear if the choice of membrane material is driven by the collaboration or whether there might be better membrane material options.
- The project team should look at opportunities for other potential collaborations to beef up the pipeline with additional catalyst and membrane leads.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.5** for its relevance/potential impact.

- The HyS cycle is an all-fluid cycle relying on only two chemical reactions, thereby reducing cycle complexity. All-fluid cycles have material transport advantages over solid transport concepts. A two-step thermochemical cycle provides significant simplification advantages over multi-step cycles. The maximum temperature of this thermochemical process is lower than any other two-step thermochemical concept and provides materials of construction and capital cost advantages. These characteristics are relevant to the DOE Hydrogen and Fuel Cell Program (the Program) goals, and resolution of significant performance barriers could provide significant advances toward meeting the Program goals.
- The reviewer marked the relevance as satisfactory, but it is really not possible to judge potential to achieve the goal of cost-effective hydrogen production—needed information is not in the material presented.
- Much time and effort has been spent on the development of the HyS cycle. Considerable funds, most recently $5.2 million by the DOE Office of Nuclear Energy (NE) and an unknown inflation-adjusted amount by Westinghouse, have been expended, and yet cycle development is not complete. It is also expected that scale-up and completion of the current Task 3 (integration of the electrolyzer with the other steps) will be difficult and costly based on the experience at General Atomics for the S-I cycle. There is also concern that the bullet design for sulfuric acid decomposition is not practical from the perspective of industrial chemical engineering. Based on these issues, the chances for successful development of the cycle appear low. The project needs to determine how much funding is enough. Nevertheless, if the button-cell is successfully demonstrated at elevated temperature and pressure with the new membranes and catalysts and is durable for >100 hours, some degree of future work can be justified. H2A calculations indicate that DOE’s cost target will be met if the electrolyzer’s development is successful.
- The HyS cycle is interesting but was out selected by NE in favor of solid oxide electrolyzers. For this reason, it is not clear how much impact it can make. The process is interesting, but it is not clear that a low hydrogen price can be achieved because it combines solar and electrolysis. It would be interesting to compare the efficiency and economics of this process against a HT electrolyzer.

- This technology suffers from a number of serious challenges, including handling and circulation of highly corrosive sulfuric acid, large activation overpotentials, the requirement to use precious metal catalysts, membrane degradation, and special materials requirements for wetted components. While the potential exists for achievement of high overall hydrogen production efficiencies, the technical and cost (precious metal catalysts, special materials for handling hot sulfuric acid) challenges are formidable. The potential impact is therefore very long-term.

Question 5: Proposed future work

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

- The proposed future work follows logically from the tasks completed to date. Proposed tasks include button-cell testing for the baseline materials set, further membrane development, and testing of advanced MEAs.
- The plan is reasonable. PBCTF is a key tool for MEA testing. But it would be good to see more catalyst and membrane candidates in case current ones do not work out. Assume that PBCTF testing will address outstanding issues of S crossover with new membranes, PBI stability, and overall performance. The work presented has been focused on the electrolyzer, but the interface with the solar heat is also important and has not yet been considered in any detail. Given that, barring major issues with the new catalyst and membrane, the electrolyzer is approaching its target performance (see Chart 18). It may be time to start considering the solar interface.
- Degradation tests must be done. PBI is known to be unstable and difficult to handle. Most fuel cell companies that investigated using it have abandoned it.
- While the reviewer is sympathetic to the issues of materials and the need for custom parts encountered by the researchers, there is relatively little time to demonstrate the operation of the button-cell at elevated pressures and temperatures that will enable further funding.
- Proposed future work is essentially irrelevant in light of proposed future funding levels.

Project strengths:

- The HyS cycle is an all-fluid cycle relying on only two chemical reactions, thereby reducing cycle complexity. All fluid cycles have material transport advantages over solid transport concepts. A two-step thermochemical cycle provides significant simplification advantages over multi-step cycles. The maximum temperature of this thermochemical process is lower than any other two-step thermochemical concept and provides materials of construction and capital cost advantages.
- Clear targets for the electrolyzer are established. There is good progress towards targets with the new catalyst and membrane. Performance is not too far from the short-term target. Building PBCTF is a major step forward.
- Completion of the catalyst screening, test stand completion, and membrane development tasks are notable, considering the funding amount.

Project weaknesses:

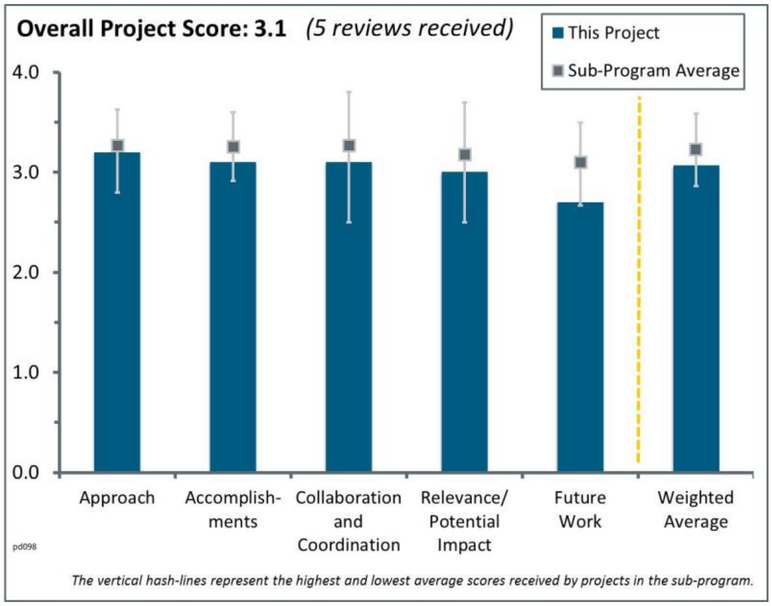
- This is a hybrid thermochemical process requiring an electrochemical step that complicates the simplicity of a two-step cycle.
- There is a limited pipeline of catalysts and membrane candidates. It is unclear how electrolyzer targets connect to economics. There is not enough information provided or available. The interface with solar heat was not really investigated.
- The membrane the project has selected is known to be difficult to manufacture in high volumes and has degradation issues. The H2A analysis needs to be updated and the assumptions made transparent.
- The technological challenges associated with the HyS concept are very significant. Precious metals are required for catalysts. It is not clear if any non-precious-metal catalysts have been tried. PBI is difficult to manufacture in large quantities and has shown large degradation. Corrosion is a significant issue for all process components that come into contact with the sulfuric acid solution. A polymer lining must be used on steel parts while wetted metal parts must be fabricated from tantalum or zirconium.

Recommendations for additions/deletions to project scope:

- In light of concerns expressed regarding viability of the choice of s-PBI membranes, the project should immediately resolve those concerns before undertaking any of the other tasks described under future work.
- A comparison between the best-case performance estimates and cost/kg for solar hydrogen production based on HyS vs. PEM water electrolysis powered by high-efficiency photovoltaics should be performed. For solar hydrogen production, the PEM/photovoltaic concept is a reasonable baseline because PEM electrolysis and photovoltaics are already commercially available.
- An H2A analysis needs to show how costs improve with their advancements. Development of other membranes should be considered.
- The project should: develop more catalyst and membrane leads, possibly through additional collaboration; begin studying/modeling an interface with solar heat; identify critical design issues and equipment needs; and develop an economic analysis to determine potential to achieve hydrogen cost targets.

Project # PD-098: Low-Noble-Metal-Content Catalysts/Electrodes for Hydrogen Production by Water Electrolysis

Katherine Ayers; Proton OnSite



Brief Summary of Project:

The overall goal is to reduce fuel cell stack capital costs for lower hydrogen production costs. This project leverages fuel cell advancements in electrolyzers to optimize anode catalyst utilization for >80% reduction in platinum-group metal (PGM) loading and to identify the optimum configuration for manufacturable, ultra-low loaded cathodes.

Question 1: Approach to performing the work

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

- This project is aimed at advancing polymer electrolyte membrane (PEM) electrolyzer technology with a focus on reducing PGM loadings while maintaining or improving electrode performance. Excellent cell performance (nearly equivalent to baseline) was demonstrated in the electrolysis mode in Phase I of this project with very low PGM loading on both electrodes. Phase II is focused on translating these advancements toward manufacturability and improving electrode durability. Low PGM loadings are achieved by synthesizing core-shell nanocatalysts, resulting in increased PGM-specific surface area and a significant reduction in required PGM loading. Ultrasonic spray (printer) coating of catalyst materials is being examined as a potential low-cost cell manufacturing technique.
- The concept of producing ultra-low Pt loading by synthesizing size-controlled core-shell nanocatalysts sounds interesting and has the theoretical potential of achieving the goal of 80% cost reduction in an oxygen-evolving reaction (OER) catalyst. This is a worthy approach if these materials are proven to be stable and can be easily be scaled up.
- The approach systematically works on cathode manufacturing, anode catalysts, and electrode and finally fuel cell development, finishing with cost analysis. The project takes advantage of technology developments at Brookhaven National Laboratory (BNL), which can lead to greatly reduced costs for catalysts.
- The approach was adequately explained. The milestone chart was useful at pinpointing the status of the progress on the project.
- The approach to reduce capital costs by reducing PGM loading and developing new manufacturing methods is aligned well with lowering the capital cost barrier. However, the catalyst represents only 6% of the total cost, as the principal investigator (PI) pointed out.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The project has made good progress on reducing cathode loadings to under 0.1 mg/cm^2 and anode to less than 0.5 mg/cm^2 with performance close to baseline catalysts. Although the catalyst is not a major cost element in electrolysis systems, it is becoming more of the cost fractionally as the other pieces become less costly. Reduction of PGM catalyst content will help insulate manufacturers from PGM price shocks. The Ru core of the catalyst is ~20 times less costly per troy ounce than platinum. Stable performance over 500 hrs has been shown.

- The progress is reported on ultra-low PGM cathode synthesis with good (~50 mV higher than baseline for long-term tests; current density was not specified) initial and long-term stable performance (500 hrs).
- Good performance of a spray-deposited cathode gas diffusion electrode (GDE) was demonstrated. Ru-Ir nanocatalysts were deposited on TiO_2 supports, showing similar performance to unsupported catalysts.
- The progress was good. Proton OnSite has a great reputation for producing results. A higher grade could not be assigned because only the results were presented. There was little discussion of the work involved in obtaining each result and the limitations within each result. For example, little or no detail was provided for the following technical accomplishments: (1) synthesis of cathode catalysts and (2) demonstration of applying the catalyst with an ultrasonic nebulizer. BNL appeared to be responsible for developing the cathode catalyst that reduced Pt loading by 98%. Results from Proton OnSite showed Pt reductions of >80%. The method for applying the catalyst at BNL was manual, while Proton OnSite was tasked with developing a manufacturing method. An ultrasonic nebulizer was used and found to give satisfactory results. However, no details were provided on Proton OnSite's contributions. For example, it is not clear if a new synthesis method was developed by Proton OnSite. It is not clear what the challenges were in applying the catalyst on a larger scale. Some discussion was needed on how the work would achieve a higher efficiency for the overall process. The organization of the presentation was not conducive to understanding. It would have been helpful if the approach and accomplishment for each task were discussed together.
- It looks like the project team has achieved the goal of down-selecting the cathode material with 10% Pt loading and the same performance and testing for 500 hrs. However, on the milestone slide, the durability milestone is given as only 50% achieved. Progress on the anode side was not as strong.
- Progress seemed less than adequate for many of the upcoming task milestones for 2014.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- This project is an excellent example of leveraging technologies from the national laboratories in order to improve commercial energy projects and systems.
- This project was led by Proton OnSite in collaboration with BNL. BNL was responsible for synthesis and characterization of catalyst materials and electrode formulations. Proton OnSite led the cell testing and manufacturing efforts.
- Collaboration was demonstrated. Proximity of Proton OnSite and BNL was seen as an asset.
- It appears that progress might be enhanced with more intensive collaboration between the partners.
- Because the development of ultra-low-PSG materials is still considered to be at an early stage, more broader and rigorous collaboration could benefit this effort.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- This catalyst improvement effort is valuable to the delivery of this technology.
- This project has potential to reduce costs in electrolysis and, just as 3M's nanostructured thin film (NSTF) has been tested in electrolysis despite being developed for fuel cells, this project could have fuel cell applications.
- Breakthrough findings are unlikely. However, in light of the investment size to date on PEM-based electrolyzers, incremental improvements to lower the noble metals and cost are relevant.
- As the PI indicated in her talk, PGM catalysts represent only about 6% of the capital cost of the baseline stack. Therefore, the potential for cost reduction associated with low PGM loading is very limited. Some additional savings can potentially be realized through lower-cost manufacturing techniques, such as spray deposition of GDEs, which is also under investigation as part of this effort.

- The catalyst cost represents only 6% of the total cost. Further anode development and longer durability testing are required to fully assess any potential cost reductions. However, if the cost projections given in PD-102 are correct, the key driver for hydrogen production costs is electricity costs. Stack costs, especially for the future forecourt case, are a relatively small fraction of the total cost (see PD-102). Because Proton OnSite is an engineering company, the cost of the electric power needed should have been well known. Rate data are available on the website for Edison Electric Institute.

Question 5: Proposed future work

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

- Proposed future work includes scale-up of manufacturing methods, additional MEA testing for durability, optimization of anode and cathode materials, and cost analysis. This proposed work appears to move the project forward in a logical fashion.
- Focusing on cathode manufacturing and anode performance/durability early on, followed by cell testing and cost analysis, sound like the right path forward.
- The cost analysis step using the Hydrogen Analysis model (H2A) for this project is important. In fact, it is possible that the cost analysis should be started earlier in the project in order to estimate the impact of the project and also to give the project guidance on technological milestones. For instance, it could guide efficiency and durability targets. The rest of the future work is reasonable and well-organized.
- Proton OnSite has identified limitations and barriers well. However, the cost of the PGM is a small fraction of the total cost. And even a significant reduction in the amount of PGM will have a relatively small effect on the total cost of hydrogen production. Based on the data in PD-102, realigning this work on possible methods to improve efficiency, such as higher-temperature operation, is suggested.
- There was no clear description of the future work necessary to bring progress on milestones up to target.

Project strengths:

- The project has the potential for a step change in catalyst cost for PEM electrolyzers.
- Excellent performance of ultra-low loading catalysts has been demonstrated using core-shell nanocatalysts. Ultrasonic spray deposition of catalyst material on oxide support material may result in low-cost manufacturing techniques.
- The project team looks like it has a good understanding and progress in cathode screening and manufacturing.

Project weaknesses:

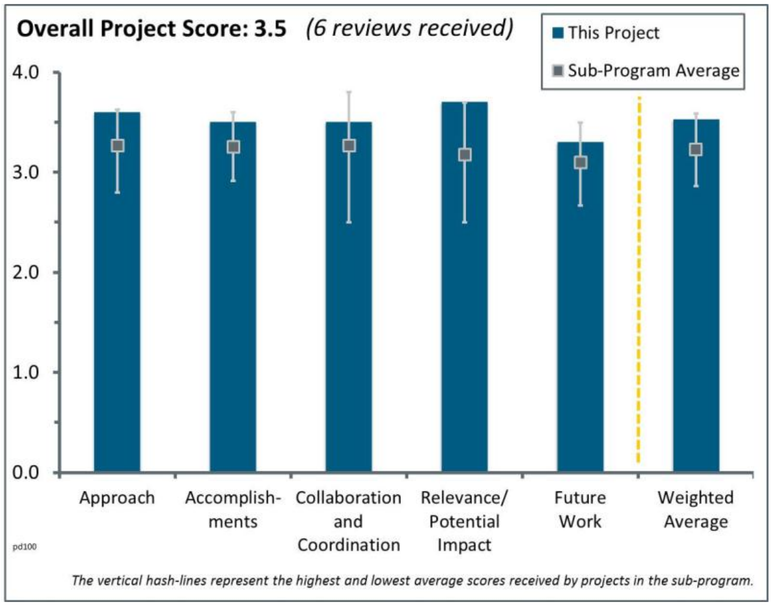
- This project includes several fairly independent lines of research. The integration of these tasks is not completely obvious beyond the general topic of catalyst development and manufacturing. The potential for cost reduction associated with low PGM loading is limited.
- The project team should consider providing the potential impact of this effort with respect to the overall cost and DOE targets in order to provide the right perspective.
- The future efforts are lacking. Efforts could be better coordinated between partners. The formulation of Ru-Ir on specific Ti materials is an expensive starting point.

Recommendations for additions/deletions to project scope:

- The project should perform an initial H2A assessment of the potential benefits to help set technical targets. This will be more helpful than waiting until the end of the project, as is shown now in the project plan.

Project # PD-100: 700 bar Hydrogen Dispenser Hose Reliability Improvement

Kevin Harrison; National Renewable Energy Laboratory



Brief Summary of Project:

The objective of this project is to characterize and improve upon 700-bar refueling hose reliability under mature market conditions. The National Renewable Energy Laboratory (NREL) designed a test system that subjects refueling hose assemblies to pressure, temperature, mechanical, and time stresses. The high-cycling test reveals the compounding impacts of high-volume 700-bar fuel cell electric vehicle refueling that has yet to be experienced in today's low-volume market.

Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- The project design is very well thought out. The test protocol is appropriate to conduct accelerated testing of the refueling system. The only thing that is not clear is how the team is planning to reduce the cost of the hydrogen refueling hose assemblies. The knowledge of how materials perform during refueling will allow more companies to build the hoses, which will increase competition and reduce cost. However, there is no discussion of cost assessment in the presentation. It is unfortunate that the limited number of hoses available will limit the tests that can be performed (e.g., burst test at different conditions).
- The approach to performing the work is logical and addresses practical issues anticipated in the field. The project is well designed to account for potential barriers. The robotic system should allow for reproducible results. Test protocol and measurement techniques are adequately described, and milestones/deliverables are well defined. It may be helpful to include some real-life exposure conditions, such as sunlight, environmental contaminants, etc.
- This is extremely valuable work to assure safe retail conditions. This project should be accelerated, given the planned roll out of vehicles in 2015–2016.
- The objective is to characterize 700-bar hose reliability under mature market conditions. The approach is well defined and clear. The project is working with industry and Colorado School of Mines (CSM) on computer control with temperature and pressure cycling with leak monitoring work following SAE J2601. The project work scope appears to be well designed and thought out with industry partners to quantify the reliability of the dispensing hose.
- It appears to be a good approach, but there will be lessons learned when actual testing with hydrogen has been started, which may change the direction of the approach based on lessons learned. The reviewer rated it as "good" because it is early in the project process.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- The test protocols have been selected, and the equipment has been acquired. Furthermore, all the tests have been evaluated. Some of the tests have already started. The performance indicators that are being measured will help accelerate the introduction of safe and reliable 700-bar dispensing hoses. The test robot is pretty impressive; conditions for the accelerated life testing seem to be in working order. The software works properly to direct the robot arm to connect and disconnect the hose.

- The work seems to have progressed well as per the plan. The installation and testing of the automation system has been completed. All of the test procedures have been established and characterized using hose material from one vendor. Additional hose materials need to be tested. There is no mention of any feedback and corresponding changes to the test plan as per the plan milestones and deliverables.
- An automated test fixture is in place, and the system is operational. Host failure was the baseline. Results indicate good bonding of exterior material with inner hose material. Expected results will show similar material degradation during host cycling. Scanning electron microscope (SEM) analysis will be used to show changes in material morphology.
- The progress is early development work. The project has not performed detailed tests to determine hose performance. The project succeeded in setting up the robot. The robot needs to include hysteresis to measure wear on the nozzle. Furthermore, the test setup should be benchmarked to existing installations at distribution centers across the United States.
- Hose pressure testing was done with air—it is not clear if this replicates the impact/effect of hydrogen at the same pressure. The project should consider repeating the test with hydrogen to find if differences in results occur. Nozzles will not last for 25,000-cycle testing; this should be considered as well, because it requires additional torqueing of hose and fittings (and potential damage). It will be interesting to see how fittings/crimps perform under climate extremes (hot and cold) and pressure cycling—the project should look into the fitting/crimps installation process and consider the impact of variations of crimp installation environments on operation in the field.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- At this point of the project, the right partners have been invited to participate. In the future, it would be good to invite additional host manufacturers to communicate the results of the testing and to boost confidence in the reliability of 700-bar dispensing systems to inspire domestic manufacturing.
- Host vendors are involved with testing at national laboratories. Future work/collaboration started with a small business and a university. NREL chemists are involved with determining the composition of the host material structure before and after testing.
- The project is working with CSM (SEM), SPIR STAR (hose manufacturing), Sandia National Laboratories, and NanoSonic (hose manufacturing). It would be good to see collaboration with station owners in California and distribution centers.
- Adequate collaboration partners are included. It may be helpful to include testing of a hose in-service in the field at an existing hydrogen fueling station.
- The project should explore collaboration and coordination with Yokohama Rubber in Japan—this company developed a 700-bar hose together with Iwatani Industrial Gases.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- Given that there is only one certified 700-bar hose manufacturer and that the manufacturer is involved and interested in the results of the project, this project has the potential to accelerate the introduction of refueling stations and boost additional hose manufacturing capability.
- The current industry's reliance on a single hose manufacturer (SPIR STAR) is not ideal, so testing the reliability of this product is a necessity.
- Accelerated testing of components in hydrogen service is important to the success of the overall Hydrogen and Fuel Cells Program. The hydrogen delivery hose is a critical component from a safety perspective, as it would be in contact with users. Demonstrating safe use of hydrogen is essential, and durability of components in hydrogen service is important for cost-effective hydrogen fuel.

- The scope of work will provide required critical information for the durability of dispenser hoses.
- There is a need to understand how leaks are analyzed. Sensors are calibrated to elevation. This work is vital to minimize the potential for an energy release in a retail location. Furthermore, extending the life of dispensing hoses will help reduce a major maintenance cost for the station owners.

Question 5: Proposed future work

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

- The future work plan is adequately summarized.
- The reviewer is looking forward to the initial results from testing using hydrogen.
- The team should also reach out to domestic hose manufacturers, as well as retail station owners, to get feedback on the testing protocol to discover what abnormal conditions need to be tested (e.g., breakaway events).
- The remaining scope of work includes full test-system integration and testing. An automated high-pressure, low-temperature cycling and post-material analysis after cycling should be done before going on to a testing program of other host manufacturers.
- The future work uses brand new materials from SPIR STAR (supplier). The project needs to improve compression equipment. It would be helpful to accelerate the effort.

Project strengths:

- The project is of high value to industry and is unique. Third-party testing is a project strength. Lessons learned from this project will improve knowledge available to manufacture reliable 70 MPa hydrogen fueling hoses.
- A project strength is that the host vendor is involved with testing at national laboratories. Future work/collaboration started with a small business and university. NREL chemists were involved with determining the composition of host material structure before and after testing.
- The technical capability, testing equipment, and laboratory space seem appropriate. The selected protocol is relevant to the performance indicators that need to be measured.
- Automation is a project strength, as is the extensive use of analytical techniques. Proper planning and appropriate collaboration are also strengths.
- Capturing an objective understanding of the equipment’s performance under dynamic test conditions is a strength.

Project weaknesses:

- No project weaknesses were identified.
- No critical weaknesses were found.
- The involvement of additional stakeholders is a weakness.
- Limited comparison material is available; there is limited variation of movement to imitate the reality of hydrogen nozzle/hose assembly usage.
- There is no clear objective, and the project seems to be proceeding too slowly. It is recommended that the project adopt the intention to confirm double or triple life expectancy.

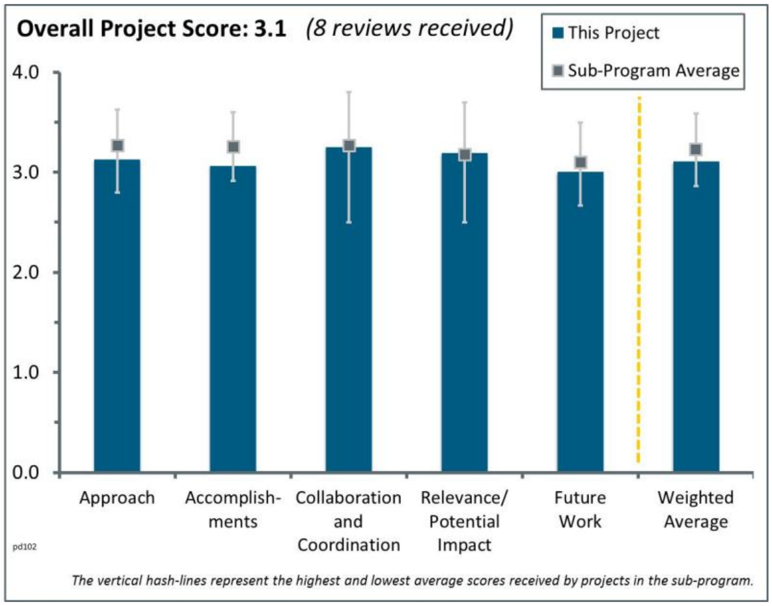
Recommendations for additions/deletions to project scope:

- The project team may want to mark the alignment of the hose with the nozzle to ensure consistent alignment and that there is no movement of the hose with the nozzle during the operation of twisting during the cycle test.
- The project should be expanded by adding additional hoses from different manufacturers.
- The testing and comparison of hoses in-service, which may be exposed to real-life conditions, is recommended.
- The project should bring in more data from field installations and operators.
- Domestic refueling hose manufacturers and/or station owners/designers should be brought in to provide input on additional mechanical stress conditions experienced by the hose during normal operations.

Project # PD-102: Hydrogen Pathways Analysis for Polymer Electrolyte Membrane Electrolysis

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:



The objectives of this project are to analyze hydrogen production and delivery pathways to determine the most economical, environmentally benign, and societally feasible paths for the production and delivery of hydrogen fuel for fuel cell electric vehicles; identify key bottlenecks to the success of these pathways; assess technical progress, benefits and limitations, levelized hydrogen costs, and the potential to meet the U.S. Department of Energy (DOE) production and delivery goals; and apply the Hydrogen Analysis (H2A) Production Model as the primary analysis tool for the projection of levelized hydrogen costs and cost sensitivities. In 2013–2014, these project objectives were applied to develop a validation case based on hydrogen generation with standalone, grid-powered polymer electrolyte membrane (PEM) electrolyzers.

Question 1: Approach to performing the work

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

- The approach used by the researchers in obtaining relevant technical and economic data from existing electolyzer suppliers in order to populate the H2A model is an excellent approach toward the main goals of this project.
- This project is directed work with objectives and deliverables defined by the DOE Hydrogen and Fuel Cells Program (the Program). In that context, the approach is outstanding. The use of expert opinions and the integration of the study with industrial resource data and the ultimate consensus by industrial participants with findings are significant achievements. The logically predictable loss of economies of scale by feedstock cost domination should have relieved the project of the requirement to address central plant analysis. General preliminary analysis should have demonstrated this feature in lieu of investment in scaled-up technoeconomic assessments. Technology improvements could reduce feedstock domination, but until that is realized, economies of scale cannot be anticipated.
- The project is feasible and integrates with other efforts, namely H2A, U.S. DRIVE, and the MacroSystem Model. The project is well designed because it gathers the necessary industry data to update our understanding of the cost and performance of electrolyzers currently and in a future case scenario, adding to H2A a PEM electrolyzer case in addition to the existing alkaline electrolyzer cases. The project updates only the existing data in the H2A model but does not directly address how electrolyzer companies are planning to reduce capital cost, increase efficiency, or improve manufacturing. The additional case is necessary to complete the dataset in H2A, but given that the efficiency of the PEM electrolysis system is already known, extensive analysis is not needed.
- Strategic Analysis (SA) is taking a well-planned approach to assess costs of hydrogen from PEM electrolysis. The collaboration with the National Renewable Energy Laboratory (NREL) ensures that this work is consistent with previous technoeconomic analyses in H2A.
- The analysis approach is excellent, but the message could have been improved if the conclusions were properly benchmarked against the established DOE cost target. The main take-home message should have

been that hydrogen from PEM electrolyzers will never meet current DOE targets so long as the electricity price stays high. This obvious omission in the project summary created the unnecessary impression of ignoring the "elephant in the room." Granted, this may be more a problem for the overall DOE Program and not the project team.

- Looking at the 1,500 and 50,000 kg/day is good. But it would be interesting to see smaller cases that are nearer term—50-100 kg/day. In the smaller productions, the capital cost should be a larger portion of the hydrogen cost. The lower production amounts would be useful to better understand near-term production costs that California and other states may find useful. They could also be used to validate the model because there should be real-life data available. It is not clear what analysis was done to verify the accuracy of the data provided by the electrolyzer companies. The companies may be overly optimistic in where they are and what they can achieve. It is not clear why the capital cost spread was 20%. This seems arbitrary. This examined only PEM electrolysis. There are a lot of alkaline electrolyzers that could be included.
- Fiscal year 2013–2014 objectives were to develop a "validation case" for hydrogen production based on grid-powered PEM electrolysis. The approach used to meet these objectives was to solicit PEM system technical and cost information from four electrolyzer companies and apply the H2A cost analysis tool to estimate the levelized cost of hydrogen production based on this technology. Current and future cases were developed for the forecourt scale (1,500 kg/day) and the central scale (50,000 kg/day). The bulk of the presentation focused on presenting the details of the H2A results for the current and future cases at each scale.
- This project seems to lack an aspect of creativity from the analysis. It seems as though the project team met only the goals for the project and did nothing more, using a sterile interpretation of the results and very few conclusions or considerations. Implementing the model without asking the relevancy of the results seems not helpful to the Program's overall needs. It is not really apparent if this project is scoped appropriately.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- Progress/accomplishments for the presented work are outstanding. The defined work in the project is on track, and the milestone for presented work was met.
- The completion of PEM cases and the incorporation into H2A are significant accomplishments and will enable quick comparisons with existing production technologies. A breakdown of capital costs will be useful in guiding research.
- The project's accomplishments are clear and meaningful within the context of the hydrogen cost breakdown.
- Significant accomplishments on this project as shown by the completion of a validation case were completed for hydrogen production by PEM electrolysis using the H2A model. A very valuable accomplishment of this work is the detailed capital cost breakdown for the PEM electolyzer system along with the sensitivity analyses provided.
- The project clearly addressed the goals using the required tools and accomplished the specific tasks, but the team failed to provide conclusions and inferences on how the tool may be insufficient—particularly with respect to the current and near-term markets. The "bottlenecks" are near-term, and the team should have made a better attempt to infer those issues to better equip the Program with the guidance.
- The results that indicated large capital cost reductions are predicted between existing and current systems and between current and future systems. It is not clear whether there is a formal methodology for estimating these cost reductions. Obviously, economy of scale plays a role for the predicted reduction between existing and current systems, but it is not clear how this is quantified. Also, the basis for the predicted cost reduction in going from current systems to future systems is not clear. Again, it is not clear if a formal methodology has been applied. There is a potential conflict of interest for the companies to predict lower costs, so measures should be taken to ensure that there is a sound technical basis for the predicted current and future costs. Surprisingly, there were very few predicted cost reductions in going from the current case to the future case, at both scales. Overall production cost estimates were higher than previous estimates; this difference is attributed to a more detailed and realistic estimate of capital costs provided by

the companies. Contrary to what is stated in the presentation, the final report on “Central and Forecourt Polymer Electrolyte Membrane (PEM) Electrolysis” was not yet available on the DOE web site.
- To say that the detailed capital cost breakdown is unique is not true; Giner has provided as detailed breakdowns. The reported costs are very similar to that of the independent analysis done around 2009. The electrolyzer efficiency from the current case should have included the data that NREL has on electrolyzer performance. NREL has actual data of operating systems. The actual data should have been used to validate the efficiency stated by the electrolyzer companies. On the tornado chart, it is recommended to not use red and green because those who are color blind cannot tell the difference. Blue and red would be a better choice. The team should have compared their findings against the DOE targets. With electricity being the major cost, it is not obvious why operating the electrolyzer at a lower voltage to increase the efficiency was not considered. There was no justification for the 1.75 V selection other than what the companies prescribed. If the electrolyzer was operated more efficiently, this would increase the capital cost but may result in an overall reduction in hydrogen cost because less electricity would be needed.
- Data have been gathered, and analyses are under way. However, it is not evident how this work will meet all of the objectives stated in slide 3: “determine the most economical, environmentally benign, and societally feasible paths for the production and distribution of hydrogen fuel for fuel cell vehicles.” The project does not address environmental or socio-economic issues. Regarding the second objective—identifying the key bottlenecks for the success of these pathways—these have not changed from the previous version of H2A. The bottlenecks are still the same; we just have better numbers now. That is the only difference. This project is not making a significant contribution to advancing progress toward DOE goals yet. Adding bio-fermentation and high-temperature steam electrolysis (HTSE) can provide interesting results, but both pathways are still too expensive. Also, it is a shame that the results of the “existing case” are not publicly available. These numbers can help with understanding the real status of the technology. The current case relies on an assumption of high volumes of production. This is not the principal investigator’s (PI’s) fault, so the score does not reflect this concern.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The existing collaboration on the analysis work with both NREL and Argonne National Laboratory (ANL) is of great value to this project. The collaboration with the electrolyzer companies was key to the achievements presented this year on this project.
- Collaboration and coordination is excellent with support from national laboratories and integrated contributions of data and opinions from industry. Coordination could perhaps be improved by making use of the extensive operational electrolyzer data collected by NREL over the course of its long-term electrolyzer testing project.
- Collaborators included NREL, ANL, and four electrolyzer companies. The laboratories were subcontractors on the project. The companies apparently participated voluntarily. The level of collaboration associated with this project was good. There was no university collaboration.
- The collaboration with the four unfunded PEM manufacturer companies provided a practical basis for performance and cost analysis, which probably is a strong point of this study.
- SA has done well in collecting data from PEM electrolyzer vendors and in collaborating with national laboratory workers to incorporate results into H2A.
- The review reports on compression, storage, and dispensing (CSD) costs without consulting CSD collaborators or without questioning the validity of the model to predict current state-of-the-art and near-term “bottlenecks.” The team did a good job of working on the key area of electrolysis and clearly formed good collaborative relationships to provide trustworthy results.
- Collaborations seem appropriate. The team did not describe who they will be approaching to obtain help from with the two additional cases.
- Based upon the presentation, it was hard to tell what the roles of the partners (other than the electrolyzer companies) were. Though during the question and answer time, it was clear that NREL helped a lot because they answered several questions.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- The evolution of a vetted and community-accepted approach and associated tool chest to assess cost performance and its sensitivity to variations in technology component cost and performance is a significant accomplishment that will ultimately be of great value to the Program's need to establish priorities in its investments. This benefit will flow down to the project level, assisting researchers in project planning and investment priority. The apparent inability of the subject technology addressed in the presentation to meet long-term (future) DOE cost goals should help identify niche applications such as forecourt or grid stabilization in lieu of central production.
- This project is relevant and the findings are trustworthy. The project team did a good job.
- This project is definitely very relevant in identifying the key cost drivers for the levelized hydrogen cost for the production of hydrogen by water electrolysis.
- Technoeconomic analysis of PEM electrolysis is a critical component in justifying support. PEM electrolysis needs to have promise of a significant advantage over traditional electrolysis for research to be justified.
- Meaningful and timely cost projections are critical in advancing the goals of the Program. The results from this project make it clear what future priorities in PEM electrolyzer research and development should be, namely electricity usage rate and the breakdown of capital costs. But the impact is not as big because the capital cost remains a very small fraction of the overall cost, which is dominated by external factors in the cost of electricity.
- These types of studies provide useful baseline cost estimates for comparison of a wide range of alternative hydrogen production methods. This particular study should be reasonably accurate because PEM electrolysis is an existing commercially available technology. However, the exclusion of the "existing" cost case is a bit disappointing.
- It would be interesting to have an analysis of what DOE's investments have done to aid in electrolyzer improvements and the resultant hydrogen cost reductions or, in other words, to determine what impact the DOE's investments have made on costs over the years. Part of the reason for this work was to measure progress against DOE goals; however, this comparison was never presented. As mentioned before, DOE has invested substantial funds at NREL in testing electrolyzers, but it was not clear that any of the testing done at NREL was used in the analysis. At a minimum, it could have been used to validate the assumptions that the companies provided.
- This is just an update to H2A numbers so far. The results are not very different from the previous version and are not likely to make an impact in the electrolyzer industry. Furthermore, the project does not identify the most environmental or societally feasible paths.

Question 5: Proposed future work

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

- The proposed future work will complete the update to the H2A cases. Also, it would be beneficial to see the variability of the results in the waterfall charts as opposed to just the "most likely" case. Also, a horizontal line should be drawn to reflect the target cost on the chart.
- The proposed future work looks like a good path forward to this project's objectives, especially if the same methodology as for the PEM electrolysis case study is employed.
- The concepts going forward are good. CSD costs may already be addressed by other studies and not necessary, although this group of researchers could do well in that task.
- Two additional studies will be performed: bio-fermentation and HTSE. The HTSE case is of particular interest because the electrical requirement (which is the dominant cost for water electrolysis) for HTSE is reduced by about one-third compared to PEM electrolysis.
- Work on bio-fermentation and steam electrolysis are good additions but will be more difficult because these technologies are not close to commercialization. The PI should take care to ensure that current cases

are based on proven yields/efficiencies and not overly optimistic estimates of near-term improved technologies.

- Proposed future work is defined and directed by the Fuel Cell Technologies Office.
- It is hoped that biological hydrogen cases will not assume unrealistic accomplishments in terms of solar-to-hydrogen, efficiencies, and other assumptions that some of the previous studies in those areas have done. The researchers should talk with the Solid State Energy Conversion Alliance for the high-temperature electrolysis.
- The proposed future work in bio-fermentation and high-temperature solid oxide fuel cells appears to be independent from the current work. Therefore, there may not be much to build upon the current PEM cost analysis results, except perhaps the H2A methodology. If so, the project title should be modified to include the proposed future work.

Project strengths:

- This study provides useful, up-to-date baseline cost estimates for hydrogen production based on PEM electrolysis. Estimates for compression, storage, and delivery are also provided. The effort was a collaboration with industry and two national laboratories. This particular study should be reasonably accurate because PEM electrolysis is an existing commercially available technology.
- The capability to work with diverse communities representing universities, national laboratories, and industry is a significant strength in this project.
- The strength of the project is the partnership and collaboration component. Having actual performance and cost data from four PEM vendors gives valuable credibility to the analysis.
- The team was able to acquire the data and analyze them. The right collaborators were involved to produce the new version of the H2A electrolysis cases. Proposed work to add advanced hydrogen production cases to H2A will generate new and interesting information.
- The project had a great presentation. It covered the topics and summarized them succinctly, although it was a little busy on the slides with words, but it was a great job overall.
- The team members have engaged industry. They are breaking down the capital costs, which allows for the identification of where investments should be made.
- The project's good use of data from electrolyzer manufacturers was a strength.

Project weaknesses:

- The presentation never included assessment of the greenhouse gas implications of hydrogen production using U.S. grid mix or other electricity production schemes.
- The project completely ignored comparing their hydrogen cost results to established DOE targets, missing an opportunity to provide context of the real challenges. Had it stated the obvious, the project team could have rightly justified the case for PEM hydrogen for niche applications where there could potentially be a commercial success without meeting the current DOE cost targets. Again, this comment could be directed towards DOE headquarters.
- Higher-level analysis of the technoeconomic framework should precede implementation of the database-driven assessments to ensure efficient use of project resources.
- The team should have used actual data from NREL's electrolyzer work as available. With compression costs being so significant, a revisit on high-pressure electrolysis (above 1,000 psi) would be interesting. The team limited the scale to 1,500 kg/day, when the nearer term and most accurate numbers would be for smaller-scale systems. The team did not compare against DOE targets, or at least it was not reported in the presentation.
- The creativity and forecasting could have been better. Perhaps this was the result of DOE guidance regarding the presentation, or perhaps it was the approach of the PI; either way, it left the reviewer wanting a bit more insight. Also, CSD should have been deemphasized more clearly in the presentation because it was not entirely in the scope of the project.
- The basis for the predicted cost reduction in going from existing to current systems and from current to future systems should be described and justified. The exclusion of the existing cost case detracts from the overall usefulness of the study.

- The work done so far does not add much to the existing body of knowledge. The project does not develop the most "environmentally benign" and "societally feasible" technologies. The project does not compare environmental or social-economic impacts.

Recommendations for additions/deletions to project scope:

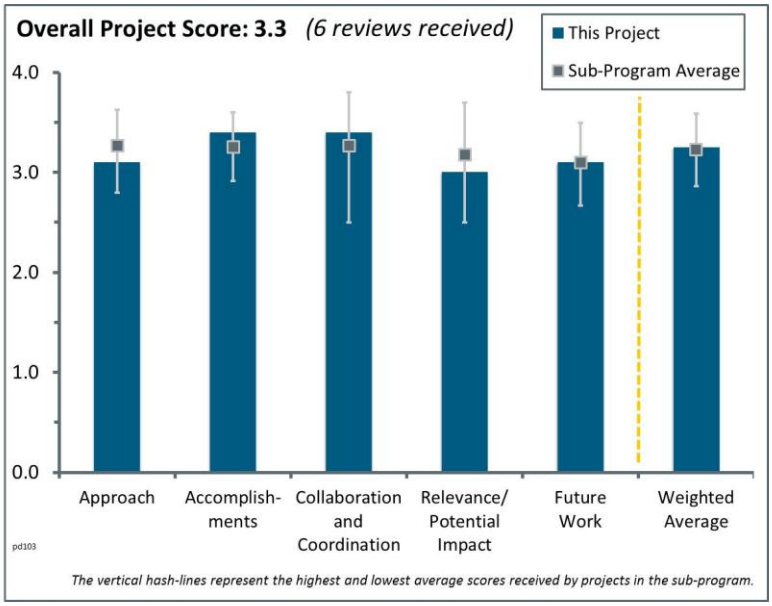
- The team should consider a better method for addressing the CSD aspect. Perhaps the recent independent panel review report—58564—would be a good reference. This was probably not available to this team at the time of the analysis.
- It is recommended that the team examine electrolyzer operating conditions to see if by changing the operating conditions, especially the voltage, the team could decrease the hydrogen cost. The DOE target is hydrogen costs. DOE provides a table of a way to achieve those costs, but if operating the electrolyzer differently from what is in the table results in lower hydrogen cost, the different operation would be preferred. The electrolyzer companies may not want to operate the electrolyzer at lower voltages because this would increase the capital cost and, therefore, decrease their sales. However, this would be important information for DOE to understand.
- The project should separate out the electricity use required for increased pressure in the future case.
- Project acceleration including studies of new long-term research projects would be valuable in getting early assistance in identifying research investment priorities at the project level.
- Changing the objectives of the project to reflect the accomplishments is recommended. The electrolyzer cases of the H2A model should be updated with data from manufacturers. The project should make sure that the team involves the right stakeholders to develop the next two scenarios.

Project # PD-103: High-Performance, Long-Lifetime Catalysts for Proton Exchange Membrane Electrolysis

Hui Xu; Giner, Inc.

Brief Summary of Project:

The objectives of this project are to develop advanced, low platinum group metal (PGM) loading catalysts for high-efficiency and long-lifetime polymer electrolyte membrane (PEM) water electrolysis, including improved mass and specific activity, and to evaluate the impact of newly developed catalysts on the PEM electrolyzer efficiency and cost through materials and system cost analysis.



Question 1: Approach to performing the work

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

- The development of advanced, low-loading catalysts for PEM water electrolysis is a promising approach to develop lower-cost electrolyzers and to increase the stack efficiency. This project is taking an excellent approach in developing electrocatalysts and integrating them into PEM water electrolysis.
- The researchers did a good job screening the catalysts. They have plans for some durability tests.
- The work addresses electrolyzer cost and efficiency. The project is focused on developing low-PGM loading oxygen-evolving reaction (OER) catalysts with improved activity, which will contribute to improving efficiency and reducing electrolyzer capital cost. The National Renewable Energy Laboratory's (NREL's) work focusing on Ir nanowires takes advantage of advances made in the Extended Thin Film Electrocatalyst Structures (ETFECS) program to develop low-Pt oxygen reduction reaction catalysts and has potential for similar gains in activity as seen for the Pt and Pt alloy nanowires from having extended surfaces and non-PGM cores. There is potential for large improvements in performance with supported catalysts (similar to those seen in PEM fuel cells going from Pt black to Pt/C) if stable conductive supports can be found. 3M's nanostructured thin film (NSTF) approach has potential to provide stable materials with high activities.
- The current approach has done an adequate job of scoping the NREL and 3M catalyst formulations.
- A better screening method is needed, possibly one designed on performance, fabrication costs, and durability. The approach to identifying high-performance, long-lifetime catalysts for PEM electrolysis consisted of developing and studying three types of current state-of-the-art Ir catalysts, i.e., (1) Ir on supports (titania nanowires and particles), (2) NSTF, and (3) Ir nanotubes. The project is well designed but ambitious because of the amount of development work required. The researchers did not expect positive results for the three types of samples. This is a good thing and a bad thing because parameters were not defined to limit the scope of work after the initial screening process.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- Significant progress was made during the past year. However, the different types of catalysts should be compared on the same basis. It appears that the state of development varied considerably; whether this was due to more difficult fabrication methods or more highly developed starting materials, such as for the

NSTF, was not discussed. Possible limitations were not mentioned. For example, one of the other speakers indicated that NSTF had durability issues. Fabrication difficulties and costs were not addressed. If skill was involved in the selection of potential materials, the accomplishments are excellent to outstanding. If luck was involved, then the accomplishments are good to excellent.

- Significant progress has been made in all fields of work. The high cell performance with low anode catalyst loading is especially impressive. The newly developed catalysts from NREL and Giner look very promising, but here in situ electrolyzer tests must show if the catalysts are comparable to the NSTF catalysts from 3M.
- The project has shown significant advances over Ir black and Giner's standard anode catalysts at lower Ir loadings. Initial support work has demonstrated improved Ir activity on W-doped TiO_2, with similar stability to Ir black (double the activity at 1.8 V). NREL Ir/metal nanowires have shown improved specific activity and mass activity compared to Ir black. Stability still needs to be improved. NSTF Ir-coated whiskers show high activity and good performance in membrane electrode assemblies (MEAs) at $1/16^{th}$ the standard Giner PGM loading. NSTF structure showed minimal transport losses at current densities up to $5.7 A/cm^2$. Initial tests suggest Ir NSTF has good durability, surpassing milestone durability.
- The progress made by partner 3M is notable. Overall project status is adequate but now needs to accelerate.
- The researchers have tested many different catalysts. It is not clear that the complicated catalyst synthesis can be done economically. The 3M catalyst performance was very good. Durability at 100 hrs is a good start. It will be interesting to see longer tests. They should try to better understand the catalyst and support interactions to better understand why their supported catalyst performs so much better. The speaker said it was improved dispersion. It seems that the doped Ti support is participating in the reaction in some way. They did a lot of rotating disk electrode (RDE) testing and not as much testing in real systems. RDE testing is good for screening, but testing in the electrolyzer is the real test of a catalyst's performance.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- Collaborations between Giner, NREL, and 3M appear to be working excellently.
- Excellent collaboration was evidenced in the presentation, and there was clear delineation of the work. However, it would have been appropriate to acknowledge the contributions of the collaborators on the pages of the presentation.
- The role and activities of the different team members was clearly communicated. There is good collaboration among the team members.
- Collaboration seems to be working effectively. Giner has been able to integrate 3M NSTF MEAs with its flow field and obtain good performance.
- It is difficult to gauge the level of interaction. The 3M technology description did not seem to demonstrate enough interaction.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- The major cost for electrolysis-based hydrogen production is electricity cost, so catalyst development to increase efficiency is very important. It is good to leverage the work done by the fuel cell team on catalyst development for electrolyzers. Decreasing the amount of PGM catalyst can decrease the costs, but the complex synthesis may offset some of the lower costs. Also, the reality is catalyst costs are relatively minor. What is really needed is a more active catalyst.
- Reducing PGM content to $1/16^{th}$ current loading will have a substantial impact on electrolyzer costs and reduce the cost of hydrogen from electrolysis. There do not appear to be any efficiency improvements in the MEA tests on Ir NSTF, so electricity costs will not be affected. RDE shows the promise of lower onset potential for Ir supported in W-doped TiO_2.

- The project is targeting the main challenges of the hydrogen production targets for electrolysis: reduction of the high amounts of expensive anodic PGM catalysts currently needed and reduction of electrolyzer capital cost by reducing the PGM loading and decreasing the hydrogen production costs by increasing the system efficiency (reducing anode overpotential).
- This project has the potential to address the goal of reducing Pt loading significantly, thereby lowering costs. However, as was pointed out during the presentations, the catalyst cost is about 6%. The reduction in Pt loading will have a relatively small effect on reducing the cost of hydrogen production. Unless DOE changes the cost target for hydrogen production or electricity costs are significantly reduced, this project has little relevance.
- This project reads on the goals of the Vehicle Technologies Office.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The proposed future work addresses relevant questions. Economic analysis will be important.
- There are well-defined, challenging milestones.
- The researchers should include the impact of long-term cycling to better understand the durability. This should be in addition to the 1,000 hr test.
- A down selection should be made as soon as possible. Electrodes have to be designed, fabricated, and tested. The cell performance is critical, and durability must be investigated.
- Although there was discussion of the future work, it was not clear why there must be continued comparison among all of the partner formulations. It seems with 3M offering superior performance, the others should be put aside, and all efforts should be aimed at maximizing the 3M formulation. This will require intensive cooperation among all partners.

Project strengths:

- Excellent research was done by the individual partners.
- The project has a good team with appropriate experience and expertise in catalysts that can be leveraged to optimize electrolyzer OER catalysts. There are good initial results.
- The researchers are leveraging catalyst development from the fuel cell work. 3M and Giner are a strong team.
- A formulation with improved performance has been identified.

Project weaknesses:

- There are no significant weaknesses. The project has a strong focus on anode catalyst development.
- There was a lack of focus on the best performing catalyst. Working with Ir puts the project at a cost disadvantage.
- The researchers need to better understand the catalyst support interaction. Longer-term testing is needed, as well as cycle testing. The cycle testing is particularly important because the electrolyzer will be turned on and off repeatedly. They need to focus more on increasing the catalyst activity.

Recommendations for additions/deletions to project scope:

- The RDE measurement should also be extended to NSTF catalysts if possible. TiO_2-supported Ir catalysts with a higher Ir-loading (>60 wt.%) should be tested.

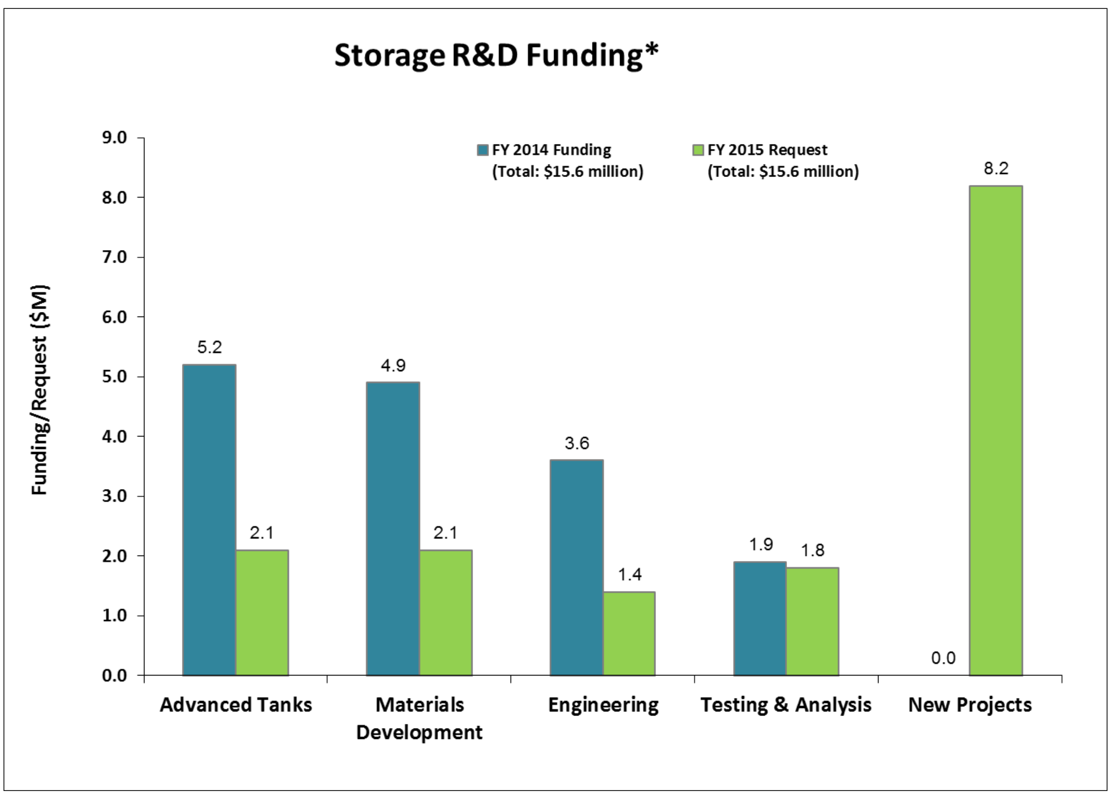
2014 — Hydrogen Storage
Summary of Annual Merit Review of the Hydrogen Storage Sub-Program

Summary of Reviewer Comments on the Hydrogen Storage Sub-Program:

In fiscal year (FY) 2014, the Hydrogen Storage sub-program portfolio continued its focus on onboard automotive and nonautomotive applications as well as increased its emphasis on new materials and novel concepts to meet performance requirements for portable power and material handling equipment applications. Reviewers commented that the sub-program is well managed and that there is good communication between U.S. Department of Energy (DOE) technology managers and project principal investigators. The reviewers also commented positively on the use of results from the Hydrogen Storage Engineering Center of Excellence (HSECoE) to help direct and focus materials development efforts. Reviewers remarked that the sub-program was underfunded, especially with the effort to address both near-term compressed gas storage and longer-term materials-based storage technologies. Overall, reviewers expressed concern that too much emphasis is currently being placed on near-term technologies at the expense of longer-term, potentially higher-payoff technologies. A reviewer recommended trying to structure future funding opportunity announcements to establish "Center of Excellence–like" collaborative efforts.

Hydrogen Storage Funding

The chart below illustrates the appropriated funding planned in FY 2014 and the FY 2015 request for each major activity. The sub-program received $15.6 million in funding in FY 2014, and it has a budget request of $15.6 million for FY 2015. In FY 2014, the HSECoE continued to be a major activity for the sub-program, although it has entered its final phase and has an anticipated end date in FY 2015. Additional efforts aimed at lowering the cost of compressed hydrogen storage were initiated in FY 2014. Work on hydrogen storage materials development is also an important part of the portfolio that will continue to be an area of focus, with three new projects initiated in FY 2014. New efforts on near-term compressed gas storage and advanced storage technologies are anticipated in FY 2015.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area.

Majority of Reviewer Comments and Recommendations:

The Hydrogen Storage portfolio was represented by 20 oral and 9 poster presentations in FY 2014. A total of 18 projects—via oral presentations—were reviewed. In general, the reviewers' scores for the storage projects were good, with scores of 3.5, 2.5, and 3.2 for the highest, lowest, and average scores, respectively.

Advanced Tanks: Three projects on advanced tanks were reviewed, with a high score of 3.2, a low score of 3.1, and an average score of 3.1. Reviewers considered the work to identify lower-cost precursors for high-strength carbon fiber (CF) manufacturing and efforts to demonstrate pathways to lower-cost advanced tanks to be highly relevant efforts that may have a significant impact. Reviewers commented favorably on the progress being made with both the textile-grade and melt-spinnable CF precursor projects. Reviewers praised the textile-grade precursor project for meeting the tensile strength target. However, one of the concerns raised included a CF manufacturer's purchase of the producer of the textile-grade precursor, which may potentially limit the impact the effort could have on the industry. For the tank cost reduction projects, reviewers commented favorably on recent and future efforts aimed to validate modeled predictions on cost reduction pathways through fabrication and testing of real systems. In general, reviewers recommended more detailed and validated technoeconomic assessments. Overall, the reviewers thought the efforts could have a significant impact on the industry.

Materials Development: Four materials-based hydrogen storage projects were reviewed, with a high score of 3.4, a low score of 2.5, and an average score of 3.1. Generally, reviewers commented on the high quality of the scientific work and capabilities of the research teams. However, they also commented that many of the materials currently under investigation would not be able to meet the full set of DOE targets for automotive onboard storage of hydrogen. However, for nonautomotive applications, which are the focus of several of the projects, they noted that significant impacts may be realized. Materials projects will continue in FY 2015, subject to appropriations, and new projects will be initiated. These projects will emphasize a stronger link and feedback route between the experimental and theoretical efforts as well as place more emphasis on meeting projected material-level property requirements to meet the system-level targets.

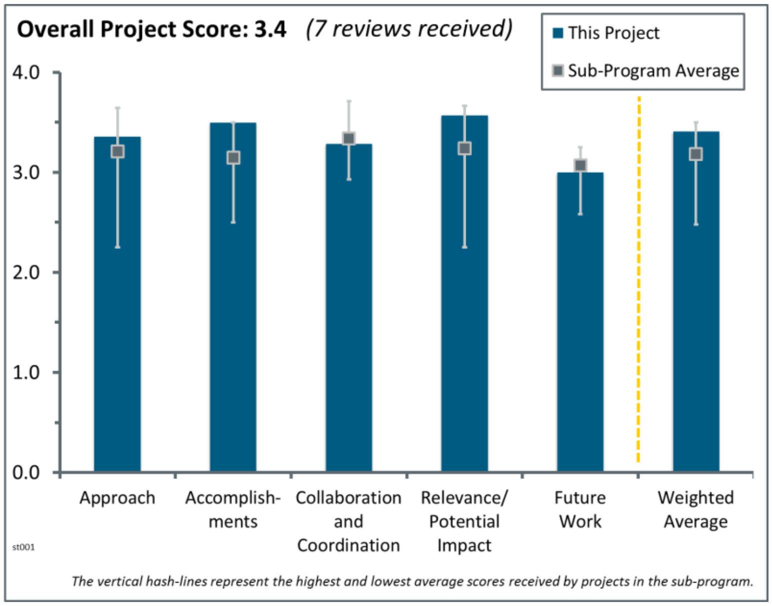
Engineering: Nine projects were reviewed on hydrogen storage engineering, with a high score of 3.5, a low score of 3.0, and an average score of 3.3. Reviewers stated that the HSECoE made significant progress in the past year and featured strong management, providing for good coordination and clear collaboration among the partners. The reviewers commented favorably on the development and use of integrated models on projecting system performance, especially for the relevant and important role in determining the material-level properties required to achieve the DOE storage targets. In general, the reviewers considered the individual HSECoE partner projects to be well thought out and well executed. The reviewers commended the HSECoE for its use of detailed milestones for tracking progress. The reviewers also appreciated the HSECoE providing the "lessons learned" for use by future collaborative efforts. It was recommended that more emphasis be placed on improving system performance for targets furthest from being met. Overall, reviewers thought the HSECoE and its partners were making good progress in evaluating materials-based storage systems and making decisions to meet DOE performance targets.

Testing and Analysis: Two projects related to testing and analysis were reviewed, with a high score of 3.4, a low score of 3.1, and an average score of 3.3. Reviewers stated that these projects are critical to the sub-program because they help develop targets and guide research to maximize impact. Reviewers commended the excellent collaboration and cooperation displayed in each project to ensure coordinated assumptions and efforts in the community. The project teams were commended for their use of the Design for Manufacturing and Assembly (DMFA) methodology, which was considered to be a powerful and appropriate analysis tool. Reviewers thought that the validation of models and analysis provides excellent information for DOE and researchers in targeting high-impact areas; however, they also suggested more validation of the results. Overall, reviewers noted that a strong team performed thorough analyses and emphasized the importance of these projects in improving the quality of research in the sub-program and providing clear insight to guide future research.

Project # ST-001: System-Level Analysis of Hydrogen Storage Options

Rajesh Ahluwalia; Argonne National Laboratory

Brief Summary of Project:



This project's objectives are to develop and use models to analyze the onboard and off-board performance of physical and material-based automotive hydrogen storage systems; conduct independent systems analysis for the U.S. Department of Energy (DOE) to gauge the performance of hydrogen storage systems; provide results to material developers for assessment against system performance targets and goals and help them focus on areas requiring improvements; provide inputs for independent analysis of costs of onboard systems; identify interface issues and opportunities, as well as data needs for technology development; and perform reverse engineering to define material properties needed to meet the system level targets.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- This project is directed analysis work in support of various DOE-supported projects. As such, by definition, it is in direct and indirect support of DOE efforts in surmounting barriers. Analysis work is always needed and valuable to understanding and quantifying barriers. It is difficult to rate the approaches of directed activities, but the techniques used in this effort are very sound.
- This project provides great support to several DOE research, development, and demonstration programs. The principal investigator (PI) deserves an award for his many contributions. The approach is sharply focused on technical barriers; it is difficult to improve on the approach significantly. The team appears to be very accommodating with tight timelines and provides constructive feedback to collaborative partners.
- The PI is collaborating with the Hydrogen Storage Engineering Center of Excellence (HSECoE) to provide models based on empirical data and systems (where available). The intent of the system modelling has shifted recently to provide a sensitivity analysis of what material and system performance is required to achieve targets. This has always been the key objective to come out of the entire engineering effort.
- The Argonne National Laboratory (ANL) approach considers the relevant technical parameters needed to assess the ability of a given storage option to meet both the onboard and off-board refueling performance targets. The ANL team collects and updates inputs from various sources to obtain reasonably complete descriptions of hydrogen storage systems, and the team's analysis methodology seems to be thorough and sound. The team has exchanged information with several partners of the HSECoE, as well as other organizations. The team performs trade studies to determine influence of various parameters to identify those with the most impact on achieving or limiting the performance targets. Unfortunately, this type of analysis does not directly lead to devising specific solutions to the problem areas. For example, it is not enough to continue showing that storage media needs to have greater gravimetric or volumetric capacities or fast reaction kinetics to overcome the barriers.
- Developing and using models is a good first step to gaining information about hydrogen storage systems. The necessary step to validate the simulation results is also addressed. It is not clear how the "Life-cycle-management" barrier was addressed.

- The PI and team's approach is generally sound. There are many different concepts covered, and it takes a sound understanding of the science to model each technology. There is a need to validate the result of the resin additive study because other researchers have had conflicting results, in that they have demonstrated nanofillers to increase mechanical properties. Tank improvements should be ranked or demonstrated in a waterfall to show the improvements and estimated impact on the tank. It is not obvious that the PI allows the experimental researchers to vet their results, and this would seem to be a key requirement since this is a reverse engineering effort. The project needs to do more than just obtaining input for the modeling. Variances should be incorporated in the projections as the values listed are not absolute.
- The approach in the analysis of onboard and off-board hydrogen systems using thermodynamic and kinetic models is effective but could be improved by further explaining the model transfer functions and references to empirical results. Additional assumption justifications and sensitivity analysis would be useful, such as recognizing a tolerance band.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- There has been excellent progress toward objectives to develop models to analyze onboard performance of physical and material properties. The project team was asked at a past DOE Hydrogen and Fuel Cells Program Annual Merit Review to provide a sensitivity analysis of parameters under model review. The back-up slides in the presentation show the message was taken seriously.
- Good progress has been made in resin development and vessel protection (impact resistance). The overview of cold gas systems with regard to volume, capacity, gravimetric capacity, and well-to-engine efficiency was good. On page 5, tasks and accomplishments should be clearly differentiated.
- The PI has provided the storage community with key "material" performance targets that are required to achieve overall system targets. This will undoubtedly cause a major rethink of the materials approach. The sensitivity analysis indicates that the required materials performance is beyond the theoretical maximum of known materials.
- ANL has continued its assessments of several storage systems, reporting results on mechanical properties of carbon fibers; low temperature compressed hydrogen gas storage; and reverse engineering on desirable properties of hydrides, sorbents, and chemical storage materials to meet the DOE performance targets. While the analyses are comprehensive and probably reliable, they do not predict promising new pathways to any significant practical improvements in properties of carbon fibers or compressed gas storage vessels, nor will the low-temperature gas storage option offer much better performance than ambient compressed gas. The requirements predicted for the sorbents to meet the DOE system targets almost certainly cannot be met with real materials.
- The overall accomplishments were very good, especially the reverse engineering of the material parameters for the sorbent system. The physical storage analysis indicated the resin properties did not affect the burst strength but could change the impact damage tolerance. This seems to contradict the results of others, such as 3M, which did achieve a burst strength improvement. The impact analysis seems to imply the addition of foam at 2.5 cm eliminates damage issues without the nanoparticles. The results of the cold gas storage analysis were useful based on the PNNL project, but the project should be careful in making generalizations about the high-density polyethylenes (HDPE) material because there are different grades of HDPE that could operate at lower temperatures.
- The results were generally sound. There is a need to validate nanofiller results, as there are questions.
- During the last year, the analysis team performed a number of tasks on physical, metal hydride, adsorbent and chemical storage components. These included very important well-to-wheel efficiency and reverse engineering to quantify required storage media properties. The modeling of composite impact damage for compressed hydrogen tanks is encouraging and should be especially useful. The reverse engineering calculations for the properties required for a cryo-adsorbent to meet the system target (120g hydrogen/kg adsorbent) will be an extreme challenge for the materials developers. All other work was clearly needed. This project provides clearly outstanding contributions to many DOE-funded projects.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- This project has excellent collaborations. Especially useful are its roles for, and interactions with, the HSECoE, the Storage System Analysis Working Group and other analysis activities.
- The project continues to have a high level of collaboration with Strategic Analysis (SA) for cost modeling along with many other organizations involved in the HSECoE and hydrogen storage development.
- The PI is working with the HSECoE, to obtain material and system characteristics. The PI is also supporting SA to assist with providing the cost analysis of these systems.
- There has been close, timely, and appropriate collaboration with other institutions; partners are full participants and well-coordinated.
- Contact to all relevant institutes has been established.
- ANL worked with SA in assessing both onboard and off-board costs for several storage systems. There were exchanges of technical information with a number of organizations within the HSECoE, although there appears to be significant duplication of effort with the reverse engineering assessments.
- It was not apparent that the collaboration was multi-way. In a multi-way collaboration, the analysis team would receive input from experimentalists to create the models/projections; the experimentalists would then review the results to vet them. This is a key part of how this relationship should work.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- This project has direct relevance and much needed impact on most of the Hydrogen and Fuel Cells Program goals and activities.
- The relevance of this project is high because it provides an independent assessment of various hydrogen storage system concepts.
- The ANL group provides valuable feedback to DOE and the PIs of DOE projects.
- This project is very relevant with regard for the evaluation of hydrogen storage systems.
- This systems analysis arguably provides more value than the fuel cell system analysis that the same PI conducts. While most original equipment manufacturers have their own internal analysis based on their own systems, there are not many complete materials storage-based systems to draw upon. This analysis provides key direction to industry, academia, and the DOE. It provides the system and materials requirements that in turn will guide material developers to take into account many material characteristics other than density. It will allow DOE and the tech teams to make better decisions going forward on funding and targets.
- ANL has provided in-depth systems analyses that supported the Hydrogen Storage sub-program with respect to the assessment of various storage approaches compared to performance targets for light-duty vehicles. While these results previously gave useful insights on the attributes and limitations of current configurations towards meeting technical and cost goals, more recent assessments appear to show levels generally below desired targets.
- This project can give DOE guidance and understanding into the impact of new technologies and is an important tool for the DOE Hydrogen and Fuel Cells Program; however, this should not be used as a standard for what is achievable.

Question 5: Proposed future work

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

- This effort is in its final stages. The PI will finish the promised deliverables.

- The plans build on past progress and generally address overcoming barriers. It is great to hear that there are plans to provide system-level support to new projects.
- There is little need for new assessments (of either high temperature or unstable hydrides as hydrogen storage candidates) because prior work by HSECoE has already documented the impact and imposed requirements rather clearly. Having new evaluations by ANL are unlikely to produce breakthrough discoveries of new materials. As there are no new attractive chemical hydrogen storage properties beyond those previously evaluated by ANL, there is limited potential for more “reverse engineering” of their materials properties or regeneration processes.
- Efforts in metal hydride should be reassessed. Relevant automotive applications should be discussed with DOE.
- The composite work should be continued and is needed to provide guidance. One suggestion would be to take a look at alane regeneration cost and efficiency based on the new project and recent results from Dr. Zidan at Savannah River National Laboratory.
- The future work list makes sense. The origins of the items on the list are not fully clear, but they presumably represent DOE’s priorities.
- The future work indicated a continuation of several items that were conducted during the past year. It is recommended that the project does not work on high-temperature metal hydrides in the future work. The addition of compressed natural gas in the future work is a good addition to further assess SA and validate the tank models.

Project strengths:

- The project provides the main analysis service for DOE. It is a versatile, valuable, and much-needed activity.
- The project continues to provide an excellent resource for hydrogen storage system modeling comparisons.
- The project provides guidance to academia, industry, and government as to the limits and possibilities of materials based systems.
- ANL has developed very comprehensive analytical tools for detailed engineering assessments of both the onboard and off-board aspects of hydrogen storage. The results appear reliable and robust from comparisons based upon current knowledge and others’ experience with available prototype and demonstration storage systems. Analyses appear to be based upon best available data from various sources.

Project weaknesses:

- The project should continue to pursue opportunities to validate its results based on empirical testing and existing hardware.
- The analyses ultimately need some validations. It is not always clear if and when such validations will be accomplished.
- ANL has been performing these assessments for a number of years and appears to have considered nearly all of the variations in the design and property parameters, but are still unable to specify storage options capable of meeting all targets simultaneously. This situation is especially true for the various materials options based upon available candidates. It seems unlikely that further tweaking of these assessments will be all that productive.

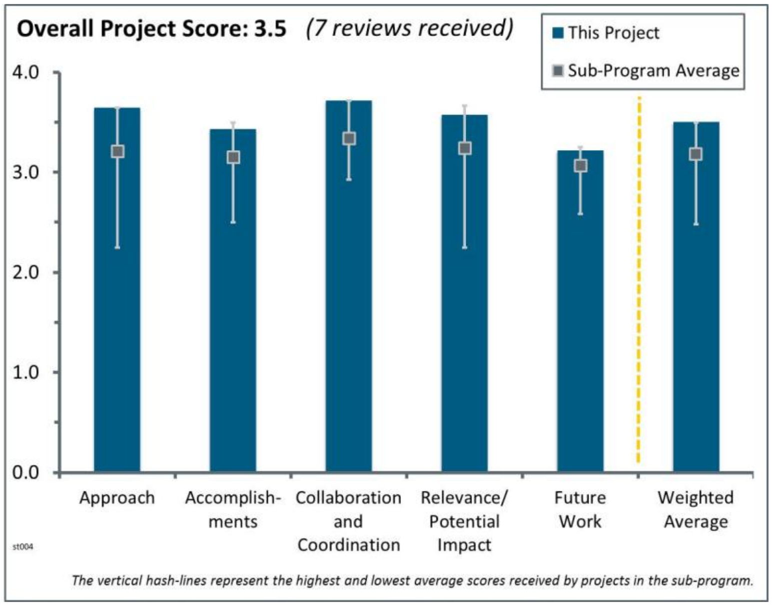
Recommendations for additions/deletions to project scope:

- The project could make a more significant impact on the overall hydrogen storage industry if certain basic models and/or transfer functions were released for the public to utilize. The project has resulted in several publications with excellent explanations of certain assumptions, but additional modeling functions would be useful for the industry.
- As the wrap-up effort, the PI should continue to strengthen the sensitivity analysis. Particular emphasis should be placed on providing guidance on the requirements of future materials to meet the overall systems targets. The PI should be careful to ensure that the message emphasizes all material requirements, such as density, heat conductivity, packing, etc., and provide multiple examples of the trade-offs of each material parameter.

- Keep publishing results in appropriate peer-reviewed journals in a timely manner. Continue to use experimental data whenever available to test and benchmark models.
- It is suggested that ANL either cease (or at least greatly curtail) further “reverse engineering” assessments of the hydrogen storage materials. It is now quite clear what properties are needed to meet the DOE targets from prior analyses by both ANL and the HSECoE, and there have been no new viable candidates identified recently. Further refining requirements via more analyses would be of limited value at this time. Resources should be directed elsewhere for more exploratory research instead of continuing these predictive assessments.
- Proceed as directed by DOE.

Project # ST-004: Hydrogen Storage Engineering Center of Excellence

Don Anton; Savannah River National Laboratory



Brief Summary of Project:

Using systems engineering concepts, this project will design innovative material-based hydrogen storage system architectures with the potential to meet U.S. Department of Energy (DOE) performance and cost targets. The objective for 2013/2014 is to design, build, and evaluate subscale prototype systems to assess the innovative storage devices and subsystem design concepts, validate models, and improve both component design and predictive capability.

Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- This project gets better each year. The management approach was good at the start and has improved annually. The presentation was a joy to listen to. The approach to overall team organization and interaction is excellent. The emphasis is on defining materials and component performance requirements to meet DOE targets for onboard hydrogen storage. In this regard, the researchers now have this aspect of the project at a point where the final outcome will be successful by most standards. The system/engineering validation models, software, etc. seem to be sufficiently comprehensive, and are now teaching the project participants about how the components should be designed /devised to obtain the best possible outcome. However, the project is also giving a fairly clear signal that some (but not all) of the DOE targets will be met. The decision to go with the metal organic framework-5 (MOF-5)-based adsorbent system, instead of a tank full of highly compressed hydrogen, is a vindication of the work done previously by the three older hydrogen storage Centers of Excellence. At least part of all what those centers worked on is in the final demonstration, even though it will not meet all the targets.
- The project aims at virtually all the important storage system parameters and barriers. It is organizational and coordinative in nature. Each storage medium (adsorbent and chemical hydride) and subtask is compartmentalized in a logical matrix managed by individual organizations and principal investigators (PIs). The structure, objectives, and management structure of the HSECoE seem outstanding.
- The general approach of simultaneous exploration of materials development and engineered systems concepts is excellent. A high level of collaboration is evident on this project, which forces additional scrutiny of the project approach to maintain cohesion. Division into technology areas and system topics (adsorption and chemical hydrides) is logical and seemingly effective.
- The Hydrogen Storage Engineering Center of Excellence (HSECoE) has undergone several down-selects to focus on two systems (different thermal management approach) on sorbents. The center is building scaled down (2L) systems to evaluate the advantages and disadvantages of each approach.
- Similar to one or two of the materials centers of excellence, the HSECoE appears to have hit its stride as the project prepares to end. The researchers have done a nice job of making difficult down-select and go/no-go decisions to arrive at Phase III. The overall management structure and decision-making processes that the HSECoE management arrived at in the end proved to be useful and supported the underlying technical projects. System Architects put into place a consistent basis for comparison of materials and systems. Use of spider charts, while cumbersome initially, appeared to, in the end, provide a readily understandable method of displaying progress toward meeting individual targets, as well as a ready

assessment of the remaining barriers. It was transparent as to how progress was achieved with time in this way. The approach that led to the “reverse engineered” materials properties for future chemical hydrogen storage materials should be the model for other similar activities.

- Although a few minor aspects of the research and development (R&D)/engineering approach can be debated, the overall approach, the strong engagement by multiple partners, and the positive trajectory of this project are first-rate. Over the last two years, the project has become keenly focused on the critical engineering and prototype development issues. The HSECoE has faced the exceedingly difficult problem of designing engineering prototype systems based on storage media that are sub-optimal. The team has adopted an approach that overcomes/mitigates this problem by exploring more general design options that are adaptable to new improved materials that may become available in the future. The results will undoubtedly be useful to the Hydrogen Storage sub-program well after the HSECoE activity has concluded.
- This project was the over-arching activity that had the responsibility of coordinating all other projects. It was effective in this task; hence the score of excellent. One area that is lacking is in a clear listing in how the $35 million in funding was distributed among the various partners.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- Progress on the adsorbent system materials (MOF-5), components (modular adsorbent tank insert [MATI], HexCell, tank, and filter), and system modeling has ranged from good to excellent. The researchers have all the parts, have tested some of the components, and are getting close to some real test data for the system as a whole, but it seems that will have to wait until next year. This reviewer is somewhat skeptical about the MATI until there is real evidence of the device performing as expected. Progress on the chemical system also ranges from good to excellent. The required materials properties are very well established. What is also well established is the fact that neither ammonia borane (AB) nor alane can meet all of the DOE targets. Some of the deficiencies seem insurmountable. The “lessons learned” part of the presentation was most enlightening, specifically the notions about earlier down-selecting and the need to identify and circumvent complexities up front. Also, the findings that would have accrued to a preemptive exploration of balance of plant (BOP) and forecourt issues for the various storage options might have given the team a better idea about how much BOP size, volume, and complexity were going to influence the system targets and costs in the long run.
- The HSECoE is on track to complete the tasks it was asked to do, namely incorporating materials into systems and fully understanding the tradeoffs.
- This is a large, seemingly well-executed project with many achievements. Sometimes it is hard to tell if the achievements are “old,” or were achieved in the past year. However, focusing solely on the past year, most activities have been on preparing designs of fabrication and preparation of test facilities. These are vital activities, but create a Phase III period of lessened output compared to periods of testing or conceptual design formulation. The detailed and extensive listing of SMART milestones is very effective. It provides both a structure to the efforts but also a very convenient way to brief on progress. Inclusion of the “lessons learned” slides shows introspection and insight and is commendable.
- After a few years of finding its way, the HSECoE has begun to really accomplish some very impressive results, and one must assume that in part, the HSECoE approach finally gelled, leading to a greater fidelity of accomplishment across the HSECoE’s quite diverse set of tasks. While progress at times seemed slow, it is now more apparent that aside from the usual project start up issues, a significant amount of effort was required just to get a high-fidelity set of materials properties collected. This must have been a significant challenge, given the great diversity of materials types and the R&D sources from which they were gleaned. Along the way, the HSECoE concept drove the project to be quite innovative in a few areas. Designing sorption systems where rapid fueling has been enabled is a very nice achievement. In the chemical hydrogen storage (CHS) area, which is a technically very challenging one from an engineering perspective, innovative approaches were made in a number of unit operations and were successes. The output of a set of CHS materials properties derived from “reverse engineering” the CHS systems will be a powerful tool for future CHS materials development efforts and will hopefully serve as model output for the metal hydride and adsorbent efforts as the team wraps up the research and compiles the final reports. Where there are still

remaining barriers to be addressed (as evidenced by the spider charts), one hopes that future DOE efforts will be directed at solving these remaining difficult and complex problems.

- Important results were obtained during this review period on systems utilizing adsorbent media and chemical hydrogen (non-reversible) media. The work on both the HexCell and MATI systems is on track, and preliminary results are promising. Although less promising results were obtained on the slurry-based alane and AB systems, the work established a good baseline for incorporation of either those systems or related media in a practical storage/delivery system. Although the slurry-based reactant delivery system can function adequately with certain materials, it is extremely complex. It seems unlikely that such a complicated system could actually be deployed in a practical transportation application. Cost and maintenance issues should be addressed. A particularly noteworthy aspect of this presentation is the technical and programmatic “lessons learned” discussion. This candid presentation provided a useful look at the critical issues, how they were addressed, and what might have been done to improve the project. For an effort with such broad breadth and scope, this information is invaluable to the reviewer and the DOE Hydrogen and Fuel Cells Program (the Program).
- This is an excellent example of a well-managed and highly productive DOE Center of Excellence. The presentation was excellent and full of valuable detail. There has been much progress on the adsorbent and chemical hydrogen storage materials synthesis, containment, and system integration, more or less on the original project schedules. The adsorbent MATI and HexCell storage systems have been developed to near completion and test stage, with much useful information developed. Many targets have been met, but volumetric system capacity and hydrogen retention properties remain well below goals. The chemical hydrogen storage materials and systems studies have been excellent in their science and engineering. Although alane was found to have some advantages over AB, both media were shown to have serious problems with cost, efficiencies, and gravimetric densities. Work on chemical systems was phased out at the 2013 go/no-go point. Even in the unlikely event that much improved materials lie in the wings, the efforts pursued by this CoE are very unlikely to meet all the targets. Either approach (adsorbent or especially chemical hydrogen storage) is turning out to be excessively costly and complicated from operational points of view, relative to high-pressure hydrogen storage. However, from the beginning it was not really meant to do so; it was intended instead to develop the engineering techniques to optimize a reasonable onboard storage system should better materials come out of the materials projects. The HSECoE should be highly complimented for expertly and objectively developing the base techniques for future use. It seemed some duplications of effort were possible, e.g., demisting and purification efforts for the hydrogen derived from liquid chemical slurries. The presentation was not always clear in precisely differentiating nominally similar efforts.
- In fiscal year (FY) 2013 the project was 70% complete. In FY 2014 the project was 90% complete. This is good progress for this year, but not excellent. Also, it appears that the project was granted a no-cost extension until 2015. Although significant progress has been documented with this presentation, there also has been some slippage in meeting the original milestones on time.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.7** for its collaboration and coordination.

- Collaboration and cooperation among all partners in the HSECoE are exemplary. This is a multi-disciplinary, broad-based endeavor that is well-managed and fully coordinated. The strong and beneficial interactions among the partners underscore the importance of the “Center of Excellence model” for tackling problems of this magnitude.
- The PI did an outstanding job in coordinating this activity. There were13 different partners, and they all seem very satisfied with the PI’s effort in coordinating the work.
- The HSECoE has developed into a very collaborative set of researchers. This is excellent! Collaborations external to the HSECoE were less well-described during the review.
- The Center is working with the appropriate partners from industry, academia and government.
- The communication forums that tie the team together across institutional and geographic boundaries are well established and are working effectively. There are very few overlaps in responsibilities and tasks. All the ongoing work is essential to the remaining goals of the HSECoE.

- The program has many collaborators and entity interactions, almost to the point of unwieldiness. However, the structure maintains focus and is made to work.
- By the nature of the HSECoE, the collaborations are very extensive and clearly productive. It is difficult to see how all this organizational structure can be fully managed and coordinated, but it seems to have been done very well. There were no real indications of serious organizational problems. It would have been useful if there were preliminary statements from the two original equipment manufacturers (OEMs), GM and Ford, as to whether such relatively complex materials/engineering systems have much potential for fueling a fuel cell vehicle relative to high-pressure hydrogen storage systems. Some comments on this matter were briefly covered in the Q&A period.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- Development of engineering solutions to onboard storage issues is critically important. The logical and well-run execution of the HSECoE greatly aids in progress toward the DOE's R&D objectives.
- At this point in time, the HSECoE is as well-aligned with the Program goals and objectives as it could be. In fact, the HSECoE comprehensive final reports will undoubtedly provide much of the definition for the Program goals/objectives in the years to come. Utilization of the models and validation codes on the HSECoE website will undoubtedly contribute to future program planning.
- This project is vital to the success of the Program. In addition to providing important information that will be useful for optimized system development, the project is providing researchers engaged in new materials development efforts with a solid set of system requirements and guidelines that must be satisfied by the materials systems.
- The major products of the HSECoE are a refined look at what materials properties must be to successfully build a system, as well as the various models that describe the engineering properties of the system-level outputs. Currently, the HSECoE is contemplating having only a drive-cycle fuel cell storage system integrated model. To serve the R&D community more broadly, it may be necessary to provide the storage system models as standalone models. There were several comments from audience members that support this vision. Lack of a plan to maintain the models once the HSECoE project ends is a potential weakness and could limit the future impact of this piece of R&D.
- The HSECoE work has had high relevance and potential impact towards the Program goals. The results may not be what was hoped to solve the storage problem right now, but the work has clearly been useful for the future. Even negative technical limitations and barriers must be known and understood. This was an outstanding effort and the work will retain its value.
- The project's overall goal was to design and engineer various onboard hydrogen storage systems. To this end, the project did a good job and made a significant impact toward meeting the Program's multi-year goals for hydrogen storage systems.
- The HSECoE is evaluating storage systems with materials known for being unable to meet the targets. The HSECoE has been careful to proceed in a manner that provides guidance for future materials and to provide results that are universally applicable as much as possible.

Question 5: Proposed future work

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

- The project should continue to develop sorbent systems (complete work), evaluate, and possibly integrate with future work on compressed natural gas solid state storage. Efforts should be reduced on chemical hydrogen storage systems, as they are very difficult materials to deal with, proposed solutions will not be applicable for automotive systems (augurs, separators, etc.), and systems are mostly materials-specific based. Many of these materials have inconsistent handling properties; therefore, they will each need their own unique systems.

- Technical work is defined by the Gantt Chart and the detailed milestones. Extensive use of detailed milestones makes it easy to track progress and explain future work to all team members.
- The plans for the coming no-cost extension year seem to be precisely what is needed to bring the project to a logical and successful conclusion. The HSECoE team is fully focused on the path identified for meaningful testing and validation of the adsorbent system.
- The future work for the last year of the project is directed toward achieving “Technology Readiness Level 4: System Validation in a Laboratory Environment.” This is clearly the logical and reasonable progression for this project. However, more detail about the future work would have been helpful. Specifically, several system deficiencies are apparent in the spider charts (especially the loss of usable hydrogen in the adsorbent systems). Specific work to be done (or recommended) to address those issues should be determined. The material requirements derived from the system analyses and prototype development work should be made available to the material science community at the conclusion of the project.
- The close out of the overall project seems to be pointed in a reasonable direction, with the caveat that stand-alone system models be included in the final output.
- The HSECoE is in its last year. The final work planned is fine and is appropriate for completing the effort. The prototype storage unit(s) should be completed, models finalized, and all significant work made public. The time to accomplish all of this seems short.
- The project is 90% complete. Its accomplishments are many. Future work lies in documenting the progress over the past five to six years. To be most helpful to the Program, the final reporting package needs to stress “lessons learned” and identify the “show-stoppers” for the various hydrogen storage systems under study. Future R&D in hydrogen storage systems should start where this project ended by addressing unresolved challenges in meeting DOE’s hydrogen storage targets.

Project strengths:

- The project represents an excellent collaboration of diverse experts with excellent management.
- The project provided the storage community with a strong understanding of materials and systems limits. It provided interesting insight on how to handle thermal management of materials (e.g., anisotropic characteristics of compressed materials).
- Excellent management and program execution are the project’s largest strengths. Use of detailed milestones is very effective in guiding the program.
- There is strong leadership at the top, along with thoughtful planning and skillful execution of the research. Good go/no-go decisions have been made down the stretch.
- This project is serving as a model for large-scale collaborations that address the daunting challenges faced by the Program. The project is well organized and managed, and the technical effort is keenly focused on the key technical areas that underlie the successful development of a prototype system. The HSECoE team has done an excellent job of taking a rather loosely structured set of material properties that were initially provided and translating and refining them to the point that the team had meaning and relevance to the development of a workable system.
- The project is well-structured, collaborative, and has good communication. It is a well-focused effort at this point. Some areas exhibited some quite innovative engineering thinking. It has a good, capable team. There was a good set of lessons learned, from both technical and project management perspectives. This would be valuable to DOE in the future.

Project weaknesses:

- There are no glaring weaknesses at this point. The project has improved over past years. A potential weakness is determining how the models on the website will be maintained once the funding is gone. In a perfect world, these models would be “living”, and someone would be tasked to keep them up to date with improvements as new results from the community emerge in the future.
- Participation of so many collaborations presents challenges (as well as benefits).
- The HSECoE is always subject to criticism from outsiders due to the nature of the materials being used. Everyone involved knows that the materials are inadequate to meet targets. The work on the chemical hydrogen storage material systems may not be universally applicable to future chemical hydrogen storage materials (most chemical hydrogen storage materials have fairly unique handling characteristics).

- The greatest weakness of this project is the fact that the three original hydrogen storage centers failed to provide a material that (1) had a chance of meeting the original DOE gravimetric and volumetric hydrogen storage system targets, or (2) gave enough margin in performance to meet even the subsequently down-graded targets.
- It is unfortunate that work on the chemical hydrogen-based systems will not be continuing. Although regeneration remains a serious issue, the chemical hydrogen systems may hold the best promise for meeting (or at least approaching) the challenging DOE hydrogen onboard storage targets. Problem areas and technical obstacles identified by "white spaces" in the spider charts have not been addressed consistently.
- The project has extremely difficult targets, combined with complex storage systems and very limited choices of adequately promising storage materials.

Recommendations for additions/deletions to project scope:

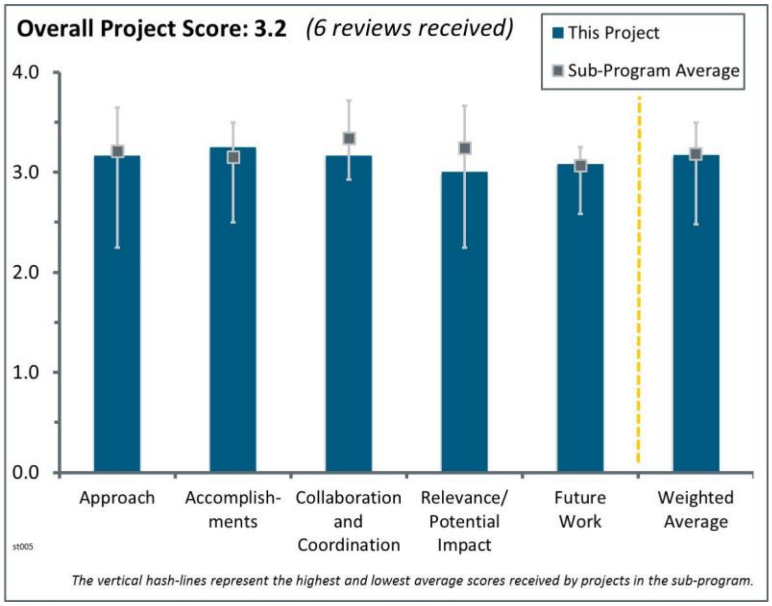
- More explanation as to why the helical thermal management system was down-selected would have helped reviewers.
- The gas-to-liquid separator seems complicated and expensive. Fuller consideration of other approaches is needed. Further attention could be paid to explaining a long-term vision for what the onboard system components might look like. For instance, the HexCell/MATI concepts use a common two-part containment system. [*DOE comment: the referred to containment system is only being used for the prototypes to be able to investigate and modify the systems for evaluation and test purposes; it is not proposed for real systems for use in actual automotive applications.]* It should be determined whether that is considered a long-term feature, as well as exploring whether other perhaps more technically risky (and thus not selected) design concepts emerged that might possibly be a pathway to future development. Further definition of a future aspirational system might serve to focus R&D activities. For example, development of a neat chemical hydrogen storage material (as opposed to a slurry) would perhaps simplify both the storage tank design (settling would not occur) and the gas-to-liquid separator. Quantification of the benefits would then help to gauge whether they should be R&D priorities.
- It should be questioned how much more effort should be spent on the chemical system in the coming year beyond final reporting. Whatever is left of planned but yet to be completed experimental work may not be all that necessary. The best path to a successful final outcome seems to be a focused effort on achieving the best possible demonstration of the absorbent system.
- Greater emphasis should be placed on dealing with the problem areas and technical obstacles identified by "white spaces" in the spider charts. A comprehensive set of material requirements based on system needs should be published in a journal that is widely read by researchers engaged in new material development.
- A key component of the final report should be statements from the OEMs as to the practical potentials they see for the materials and containment designs developed in this project. Whether or not these designs are reasonable for achieving commercial reality needs to be determined.

Project # ST-005: Systems Engineering of Chemical Hydrogen, Pressure Vessel, and Balance of Plant for Onboard Hydrogen Storage

Kriston Brooks; Pacific Northwest National Laboratory

Brief Summary of Project:

This project addresses the engineering challenges for materials-based hydrogen storage and provides feedback and recommendations on materials requirements. Project results impact identification, development, and validation of critical components for storage materials in light-duty vehicles, as well as development of models and simulation tools to predict materials performance and development of engineering methodologies, analysis tools, and designs applicable to stationary storage and portable power applications.



Question 1: Approach to performing the work

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

- The project's approach is clearly defined.
- This project team went through a rigorous go/no-go process on chemical hydrogen slurry systems and has quickly adapted its approach, including new tasks in the adsorbent area following the no-go decision on chemical hydrogen storage (CHS). The team performs solid engineering analyses and engineering assessments that are relevant to addressing the barriers the projects present them with.
- The approach is consistent with U.S. Department of Energy (DOE) needs, in particular support of the Hydrogen Storage Engineering Center of Excellence (HSECoE) in the general area of systems engineering. In addition, the project indirectly contributes to high-pressure cryogenic gas storage efforts. In view of the 2013 ending of chemical hydrogen work, the project has expanded efforts on adsorbent tank concepts.
- The approach has been given a score of good in that it was generally effective in addressing the various barriers associated with designing advanced hydrogen storage tanks and estimating balance of plant weight and volume.
- The team combines expertise on hydrogen storage materials, storage tanks, coat modeling, and manufacturing to provide potential hydrogen storage solutions based on various concepts, such as liquid slurry, tank exchange, and compressed hydrogen storage.
- Development of cost and performance models and weight-saving strategies are reasonable approaches for optimizing pressure vessel design and fabrication. Ideally, off-board factors would have been included, but this was out-of-scope.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The Pacific Northwest National Laboratory effort has made useful progress in the areas of reactor modeling and model validation, system parameters (cost, volume, and mass) and several other contributions. The analysis of alane should be especially valuable to the HSECoE by assessing the relative merits of alane (AlH_3) versus. ammonia borane (AB). Extensive progress was made on containers for AB slurries before the chemical storage effort was terminated, as well as on adsorbent work. The project has accomplished weight, volume, and cost reductions by balance of plant optimizations. Cost estimates on alane are very useful. The thermos container concept is simple and should help with general DOE efforts on adsorbent storage concepts.

- Progress was good this year, going from 65% complete in fiscal year (FY) 2013 to 80% complete in FY 2014. The only task at risk is the adsorbent cost analysis. The presentation failed to discuss why this cost analysis is at risk.
- The team was able to suggest some potential solutions based on system-level thinking and simulations. The specific accomplishments include (1) combining exothermic and endothermic thermo-bottle system with much reduced foot print and (2) providing proof-of-concept laboratory-scale demonstration. The knowledge gained will be very valuable for future hydrogen storage system design.
- Following the no-go on slurries, the team has picked up on the thermos bottle concept, and has quickly demonstrated proof of principle. The team's continued work on finishing up on the CHS system model is important. While the current CHS system is highly complex, and still has several significant remaining barriers to be addressed, the model being developed will be very important for future efforts that will hopefully allow the field to take advantage of the high volumetric capacity that CHS enables. The work on identifying opportunities to reduce the gravimetric and volumetric contribution of balance of plant (BOP) components has progressed and impacts many other areas across the Center's activities. The team's work and progress on the "tankinator" model was noted by other project presenters.
- Cost analyses results are useful. System mass and energy modeling allow parameter optimization. Reductions and improvements in BOP components resulted in significant weight and volume savings. The thermos bottle is a rather naive approach to heat exchange. More traditional, well-known heat exchanger concepts with internal coolant circulation might have been a better approach.
- The flow schematics with small print and exceptionally thin lines are hard to read. Clarity of system operation is lost as a result of the diagram. Alane cost results are interesting and quite high. Even when not considering the high media cost, the reactor system itself appears cost prohibitive (even at 500,000 systems peryear). No description of cost analysis methods or of pathway to lower cost is discussed. The alane reactor performance model does a decent job of matching experimental data. However, there is no description of type of model, i.e., whether it is empirical or first principals. Reduction of BOP mass and volume is an interesting conceptual exercise and probably worth doing. However, it is important to note that the BOP mass savings achieved is minor compared to the mass of the overall storage system. Thus, resources are being spent on a small part of the weight problem, not the main one. Also, the volume savings probably do not lead to additional usable volume for the system, i.e., the "saved" volume is in unusable locations. It is not clear if the cost of the redesigned BOP unit is considered. The testing of thermos bottle concepts seems well-considered and well-executed. Tests results indicated unexpected zones of high cooling rate. Mixed/alternating use of liquid hydrogen and liquid nitrogen for cooling is an interesting idea. The general amount of cryogens consumed seems to be high. The cost implications of using that much cryogen should be determined.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- There were excellent collaborations with HSECoE and other entities. Such collaborations are vital as the project continues into the remaining phase.
- There appears to be excellent collaboration among HSECoE members and others.
- As with all projects associated with the HSECoE, the collaboration and coordination between researchers was excellent.
- There are numerous good collaborations within the HSECoE, Storage System Analysis Working Group, and others.
- Adequate collaboration was demonstrated with the CHS partners and across the HSECoE as other areas have received greater emphasis.
- There has been good coordination within the HSECoE, but there is little evidence of collaboration outside of it.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- This team's approach is highly relevant to achieving overall project goals across several different storage system scenarios, and it has made good progress toward achieving the goals of their specific tasks. The impact of having the best available CHS system model is high, as it will provide future efforts a very solid foundation on which to build. The team's work in cryotank modeling, design, and cost modeling will have an impact in several areas. The thermos bottle' approach may contribute to reducing fill time, which is a high priority target.
- There is a good match to the DOE Hydrogen and Fuel Cells Program goals and objectives for onboard hydrogen storage, mainly via supportive activities for the HSECoE.
- The impact of this work is good. This year, the project experimentally measured cool-down rates for the term's bootleg prototype hydrogen storage tank. The project also addressed methods for reducing the BOP mass and volume by integrating key components.
- This project is very important to understanding/simulating various storage options.
- The relevance of engineering optimization for materials that will never meet targets is questionable.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- This project includes an adequate set of tasks to ensure that the close-out phase of the project results in the preservation of the documentation for future efforts.
- Work for this project is almost complete.
- The list of future activities is reasonable to complete the story. DOE has ended the chemical hydrogen storage efforts, but the models should be completed and published.
- The proposed future work is good since it addresses the remaining tasks of this work. These tasks are the design and dormancy testing of a two-liter vacuum thermos bottle, as well as costs modeling for the modular adsorbent tank insert and Hexcell storage tanks.
- The proposed plan is very reasonable. Adsorbent work will take priority in the coming months. It is very important to document the results in open publications and digital models.

Project strengths:

- Comprehensive engineering and cost analysis of systems supported by experimental proof of principle testing are project strengths.
- Project strengths include cost modeling, the tankinator model, and the thermos bottle concept evaluation.
- There is good work on BOP volume and mass reductions.
- The project provides good input to the HSECoE in the area of systems engineering.
- The team has the right expertise and excellent communication/collaborations both internally and externally.

Project weaknesses:

- There is the potential to become distracted with new adsorbent and tank-based tasks and neglect providing the highest fidelity CHS model possible from the existing information.
- The project emphasis was originally on chemical storage without reasonable materials needed to approach DOE targets.
- Feasible hydrogen storage materials are not available for more practical simulations/modeling. The team is asked to perform simulations on materials and concepts that will not be of practical use.

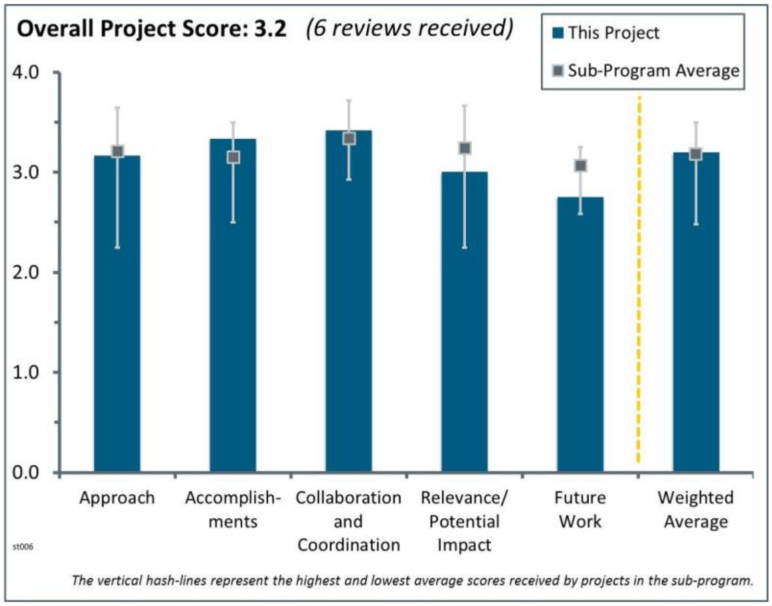
Recommendations for additions/deletions to project scope:

- The project scope is appropriate for the end stages of the HSECoE.
- Include economic results for the costs of coolants needed to cool down a tank.

Project # ST-006: Advancement of Systems Designs and Key Engineering Technologies for Materials-Based Hydrogen Storage

Bart van Hassel; United Technologies Research Center

Brief Summary of Project:



The objective of this project is the design of materials-based vehicular hydrogen storage systems that will allow for a driving range of greater than 300 miles. To accomplish this goal, United Technologies Research Center (UTRC) will leverage in-house expertise in various engineering disciplines and prior experience with metal hydride system prototyping to advance materials-based hydrogen storage systems for automotive applications.

Question 1: Approach to performing the work

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

- UTRC plays an important collaborative team role with the National Renewable Energy Laboratory in the development of the graphical user interface (GUI) architecture for the Simulink Model efforts. These frameworks are one of the critical outcomes from the engineering center to facilitate research on new materials.
- The recent and current focus of UTRC has been in two distinctly different areas: (1) developing and demonstrating effective components that deliver purity-free hydrogen gas from the adsorbent and chemical hydrogen systems, and (2) developing and implementing storage system modeling tools that can be utilized not only within the Hydrogen Storage Engineering Center of Excellence (HSECoE), but also by external researchers to clarify materials properties required to meet performance targets. Both of these topics are relevant to the development of viable storage systems for passenger cars, although they do not offer immediate solutions to all problem areas.
- The team has done a nice job of absorbing the no-go decision on chemical hydrogen storage (CHS) systems within the HSECoE and bringing those tasks to an orderly completion. Participating in the team that is developing the GUI for the integrated framework models so that the public release version will be ready by the time the HSECoE ceases work is a very important task for UTRC. The approach to bringing the CHS system components (such as the gas-liquid separator [GLS] and the purification train) to a logical conclusion is important, as this helped to provide proof of principle that even though it is complex, the CHS system can achieve noteworthy volumetric capacity. Thus, the efforts of UTRC will be important for consideration of potential future CHS materials development, and for potential portable power applications. UTRC’s work in particulate clean-up for adsorbent systems was approached in a logical manner.
- The project approach fits in with the HSECoE objectives and thereby addresses several of the problems and barriers. In particular, the specific problems with particulate control in the adsorbent and liquid phase control in CHS beds are covered in this project. In addition, UTRC is also leading the development of the GUI architecture. Importantly, this will allow the models developed by the HSECoE to be made available to the public.
- The HSECoE has taken a reasonable approach to addressing hydrogen quality issues. The project has adopted existing commercially available technologies to address problems.
- The effort is reasonably well defined. The work approach needs to be better correlated with how barriers are being addressed.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- Outstanding progress has been made in all the contractor's areas of responsibility. The principal investigator's presentation was clear, direct, and appropriately detailed. The work on liquid management is apparently finished. With the virtual termination of chemical storage work during the 2013 go/no-go decision, the project has focused more on adsorbent storage problems. Metal-organic framework (MOF) particulate filtration phenomena have been nicely studied. Reductions in balance of plant costs have been achieved. The project has done some outstanding vehicle simulation work, and models were developed. Some of these models have already been placed on the website.
- There has been very impressive work on the GLS, including the design, enabling the reduction in mass and volume, and the testing to validate some of the model predictions. Even if the CHS materials are not going forward in Phase III of the HSECoE the results from the GLS will be valuable, as other liquid materials are considered for vehicular and non-vehicular applications.
- UTRC's work on integrating storage system models into a framework that is readily usable and accessible is an important accomplishment (still in progress) that will be important to DOE goals of providing the storage community and public with usable access to modeling capabilities. The team's work on the CHS major balance of plant (BOP) components, such as gas cleanup and the GLS, was important to demonstrate proof of principle that such complexity could be successfully integrated into an onboard storage system while achieving the target for overall system volumetric capacity. This is a noteworthy accomplishment. The CHS system team, of which UTRC is a member, did a good job of investigating the slurry concept to the point where it was demonstrated to be difficult to implement for CHS. This is a valuable contribution, as knowing where not to go in the future is crucial to future DOE efforts in onboard hydrogen storage. The team's work on particle filtration for adsorbent systems will have an impact on other system types as well, and is an important contribution to achieving DOE goals.
- The modeling effort is clearly valuable in supporting technology development and addressing barriers. It is not clear what the criteria are for an acceptable gas liquid separator.
- Given the no-go status for further chemical storage work in 2013, UTRC satisfactorily completed improvements for a prototype GLS system that included experimental demonstration of its effectiveness, along with major reduction in mass and volume compared to an initial conceptual component that would have been prohibitive. The team also met milestones for ammonia and particulate filter systems that should satisfy targets. The team is making excellent progress on completing web-based system analysis models for the chemical and adsorbent systems that will be available to allow public and independent assessments of alternative candidate materials for comparisons with performance targets.
- Developing a computational fluid dynamic model to allow sizing of GLS was a promising step, but it does not appear to have predicted experimental behavior. Improvements in system capacity are significant steps. The GUI should enhance the model's usability and encourage wider use.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- There was close, timely, and appropriate collaboration with other institutions; HSECoE partners are full participants and well-coordinated. The collaboration with the Los Alamos National Laboratory and the Pacific Northwest National Laboratory to come up with material targets based on engineering needs was fantastic.
- The project worked closely and efficiently with various organizations and individuals on implementing and testing the storage systems user-interface software for the web-based models. UTRC also collaborated closely on issues relating to purification of the chemical and adsorption storage materials.
- The project interacts with a number of high-quality collaborators from both industry and national laboratories.
- Collaborations are excellent, but almost entirely within the extended network of the HSECoE.

- The level of collaboration appeared to be adequate, but did not appear to be reported very well. There must have been more effective collaboration within the CHS system team for UTRC to have accomplished what it did. Collaboration will be extremely important in rolling out a usable and effective GUI for the integrated framework models.
- There was adequate coordination within the Center, as well as work with third-party vendors to acquire devices.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- This project provides great support to several HSECoE research, development, and demonstration projects. This year, contributions include an impressive reduction in the mass and volume of BOP components for the GLS and the ammonia filter for the CHS approach, as well as some initial work on particulate filters for the sorption approach.
- This project is of significant impact, as it has demonstrated that even in systems of high complexity, such as the CHS, there are innovative ways to address gravimetric and volumetric capacity challenges in designing and fabricating critical BOP components. The UTRC results helped to formulate what the materials properties must be for future CHS efforts, particularly in the area of minimizing impurities to reduce the gas cleanup BOP budget, and in particulate cleanup for solid systems as well.
- This project supports the HSECoE in several practical and modeling areas not covered elsewhere, and is thereby directly supportive of the DOE Hydrogen and Fuel Cells Program's goals and objectives.
- UTRC has developed promising concepts and prototypes of purifiers for improving the quality of hydrogen supplied by various chemical storage materials. While these improve the potential for this storage option, they cannot override the significant regeneration issues. The development of robust and flexible analysis models for Internet-based usage could be useful to focus future researchers on candidate materials that address more requirements for the total storage system.
- The effort is contributing to reaching DOE targets. It is not clear what the specific contribution toward reaching targets is attributable to gas liquid separators and filters.
- Work to optimize auxiliary systems for materials that will never see commercialization cannot be justified. Modeling work is probably justified to allow future investigators to build on existing work.

Question 5: Proposed future work

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

- Plans to finalize the Simulink framework models are appropriate, given the remaining time of the project. Hopefully, there are sufficient time and resources to submit a peer-reviewed paper or two on the team's nice work in addition to a final report.
- Final activities are largely centered on the finalization of the Simulink framework development and completion of some of the adsorbent modeling. Plans for the remainder of the project are reasonable for the short project time remaining.
- Future work for simulation portion is reasonably well defined. Future work for GLS and hydrogen quality is not identified.
- For the amount of unspent project funds, (i.e. ~$1 million as of 3/31/14, as shown on slide 2), having only the major technical task to be adding the chemical and adsorption systems to the models for the framework website seems rather costly, along with just performing management activities and preparing the final report. More efforts would have been expected toward either the particle filter assessment or some other support activity to the cryo-testing with at least a portion of these funds.
- It is very important to achieve a practical user interface well in advance of the completion of the HSECoE's work.
- Modeling work should be completed and will add value. The value of continued experimental work is dubious until promising chemical hydrogen storage materials are found.

Project strengths:

- The project has been very responsive to DOE's and the Technical Team's suggestions and directions.
- UTRC has consistently provided diverse, high-quality expertise by developing innovative purification systems, especially for the chemical storage materials, over the past couple of years. It was able to verify experimentally components that greatly improved purification of the hydrogen gas. UTRC has had talented and dedicated staff members contributing to the HSECoE tasks who made substantial contributions over the past five years with both experimental work and system analyses and modeling.
- There has been good integration with the System Architect's vision for the CHS system. While not viable with the slurries, the CHS system team, including UTRC, has provided very valuable information for both potential portable power applications as well as future CHS materials development efforts.
- The simulation development and its migration to a public platform are valuable.
- The project adds important engineering and simulation components to the overall HSECoE.

Project weaknesses:

- The UTRC project did not demonstrate any significant weaknesses in supporting DOE goals.
- There needs to be a clearer quantification of how GLS and hydrogen quality efforts contribute to reaching DOE targets.

Recommendations for additions/deletions to project scope:

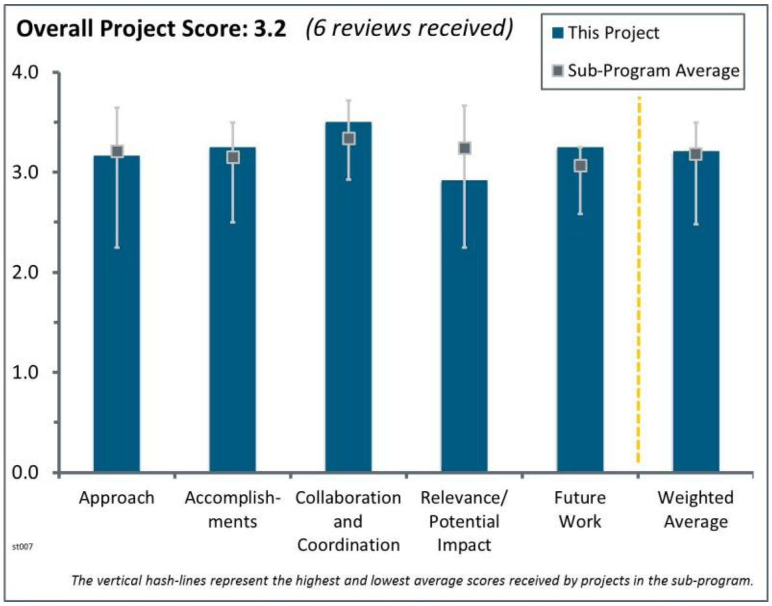
- Particulate filtering of the MOF-5 bed still seems somewhat problematical. Some particles are still passing through the filter and may pose a problem for downstream valves. More mitigation work may be necessary.

Project # ST-007: Chemical Hydrogen Rate Modeling, Validation, and System Demonstration

Troy Semelsberger; Los Alamos National Laboratory

Brief Summary of Project:

The objectives of this project are to develop chemical hydrogen storage system models, develop chemical hydrogen storage (CHS) material property guidelines, and develop and demonstrate advanced engineering concepts and components for hydrogen storage systems. This project will provide a validated modeling framework to the energy research community and provide viable material properties that meet U.S. Department of Energy (DOE) 2017 system targets; identify and advance engineering solutions to address material-based non-idealities and identify, advance, and validate primary system level components; and provide an internally consistent operating envelop for materials comparisons.



Question 1: Approach to performing the work

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

- The project was well-designed and integrated with other efforts on CHS systems.
- The principal investigator (PI) took on many of the key components required for a CHS system, such as liquid carriers, gas-liquid separators, reactors, augers, and bladder tanks. These are very difficult components to provide a universal design for all potential future materials. The PI designed flexible models where possible to make the work more relevant to future materials.
- This project has mainly concentrated on CHS, a clear specialty of Los Alamos National Laboratory. It has recently focused more on alane (AlH_3), in particular slurry-based properties and systems. There is a valuable modeling component, along with system optimization. All the project activities are directed at DOE barriers and needs.
- The approach for system architecture was competently thought out and executed. The fact that a reactor shows different activities for the same space velocity but different auger rates suggests that it is not well-characterized and should not be used for fundamental kinetic measurements. The reactor is not operating in either a continuous flow stirred-tank reactor or plug flow regime. The PI should start with a reactor that it is well-characterized before attempting to measure kinetics that can be interpreted in a meaningful way.
- Adequate approaches have been presented for overcoming barriers raised for chemical hydrogen storage systems.
- The team used its system-level expertise in simulation to help address the various materials property trade-offs and balance of plant requirements. The approach is sound; unfortunately, the unavailability of really workable hydrogen storage solutions forces the team to employ various hard-to-implement strategies.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- Progress on systems, components, materials properties, and modeling is very impressive and clearly contributes to the understanding needed for practical systems. It is not always clear when the cited progress was made, i.e. whether it was within the last year or earlier. The materials properties required to meet DOE system targets have been better defined. The work has clearly shown difficult problems for both ammonia borane (AB) and alane in the areas of fuel cost, system cost, efficiency, and gravimetric density. These properties are far from the DOE targets, and it is difficult to see how materials improvements can be made to counter these problems in the near to mid-term. Such negative impressions are indeed important to know. This activity, along with others, has led to the discontinuation of chemical hydrogen work. However, system designs developed during this project for liquid slurry and solution approaches should be applicable to DOE storage projects in general and to chemical hydrogen-storage activities that may be revived later.
- System design, construction, and validation have been well done.
- The project provided a good summary of the system design examined and lots of new information on the endothermic CHS system with the alane results.
- There have been numerous component and system design concepts delivered for CHS components.
- The establishment of critical parameters required for hydrogen storage materials provides a benchmark that will allow researchers to quickly assess the potential of their materials for meeting the DOE targets.
- The PI tried several approaches (particularly slurry formulation) to achieve desired formulations; however, the materials are difficult to work with.
- Considering the unavailability of really feasible solutions, the team did the best it could in (1) developing chemical hydrogen storage system models for alane and AB slurries; (2) providing guidance through system-level analysis in terms of materials properties for slurry stability, impurity quantification, and kinetics; and (3) demonstrating laboratory-scale systems and components. The models and experience significantly enhanced the knowledge on how to deal with chemical hydrogen storage options.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- This was a part of the Hydrogen Storage Engineering Center of Excellence (HSECoE) and is an excellent model that other teams should follow. The PI worked with the appropriate industry, government, and academic partners.
- There was successful collaboration with United Technologies Research Center (UTRC) and Pacific Northwest National Laboratory (PNNL) to develop guidance for future materials development.
- There is a wide range of outstanding collaborations, both within and beyond the HSECoE. There is good communication between the team and various stakeholders who provided inputs to the models.
- There was good collaboration within the HSECoE, but little work with non-HSECoE members.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.9** for its relevance/potential impact.

- The relevance and impact of this project on the DOE Hydrogen and Fuel Cells Program is clear and substantial. It has contributed to the termination of chemical hydrogen storage work. This is a valuable output of the project, even if it is disappointing.
- This project is directly relevant to the hydrogen storage requirements of DOE.
- This project worked with UTRC, PNNL, and other members of the HSECoE to develop a system model recommending material properties for chemical hydrogen storage approach. This was one of the most

important expected outcomes from the HSECoE, and they teamed up to accomplish this in a timely manner.
- It is unlikely that these systems (CHS) will make it into automotive use. The complexities of moving liquid slurries are very difficult to manage, particularly on automotive systems. Many of the components worked on will likely not be suitable for future materials because of the specific material handling requirements of most materials.
- Work on materials that have no chance of commercialization is not a responsible use of funds.

Question 5: Proposed future work

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

- Considering the short amount of time left for the project, the proposed future work is reasonable and achievable.
- The project is largely complete.
- Because of the down selection, the PI is winding down work and providing the final reports. Future work should concentrate on developing neat liquid systems and robust bladder systems.
- Peer-reviewed manuscripts will be a great complement to the models available through the Internet.
- The project is near its end. The remaining activities are naturally limited, but appropriate.

Project strengths:

- Mapping of materials properties needed to meet DOE targets was a project strength.
- The project provided very useful expertise on chemical hydrogen carriers and component and system engineering.
- Models are well-developed and seem to be working for each system.
- Project strengths include good team expertise and system-level simulation experience.
- Developing the scalable model for the liquid-gas separator is a useful tool that can be used for future materials and systems. The PI should have spent more time on describing the bladder system that was developed; this was not a trivial exercise, and it could have applicability for future systems.

Project weaknesses:

- Progress was always hampered by the breadth of components that needed to be worked on and their lack of universal suitability for all materials.
- Kinetics measurements were questionable.
- It is questionable whether the models will work for materials/systems that are really useful.
- Not many good solutions were available to the team.

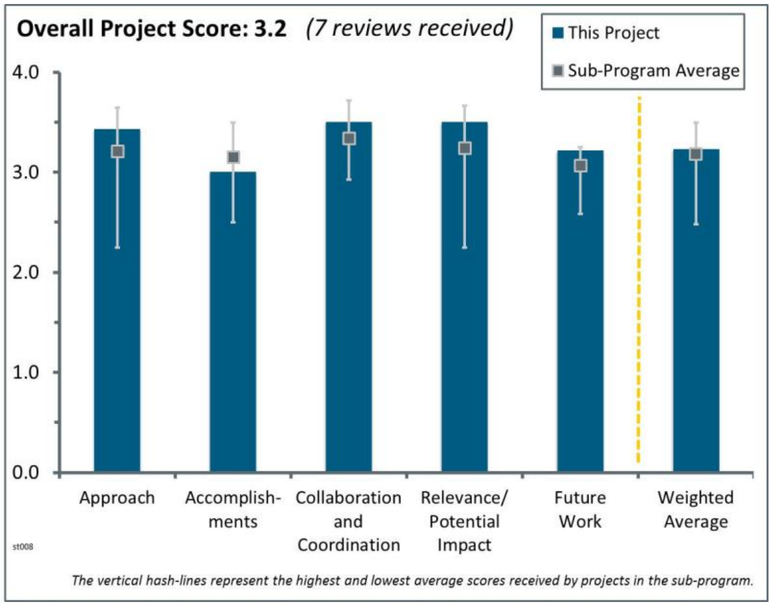
Recommendations for additions/deletions to project scope:

- In order to achieve a neat liquid that does not hurt system density, novel and innovative ideas are required to develop solutions that are stable, light, liquid at all conditions, and support the regeneration efficiency of the materials. Future materials studies on chemical hydrogen storage materials need to place equal emphasis on the carrier materials and the storage material itself.

Project # ST-008: System Design, Analysis, and Modeling for Hydrogen Storage Systems

Matthew Thornton; National Renewable Energy Laboratory

Brief Summary of Project:



Objectives for this National Renewable Energy Laboratory (NREL) project are in support of the Hydrogen Storage Engineering Center of Excellence (HSECoE) with system design, analysis, modeling, and media engineering properties for materials-based hydrogen storage systems. Vehicle performance research will develop and apply a model for evaluating hydrogen storage requirements, operation, and performance trade-offs at the vehicle system level. Energy analysis focuses on coordinating hydrogen storage systems well-to-wheels to evaluate off-board energy impacts. Media engineering properties research assists in the identification and characterization of adsorbent materials that have the potential for meeting U.S. Department of Energy (DOE) technical targets for onboard systems. The project leads the effort to make select HSECoE wide models available for use by other researchers via a web-based portal.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- Providing vehicle-level modeling is necessary to provide the correct operating framework for the storage systems. Even a simplified system can provide good guidance to the HSECoE to develop suitable and relevant systems.
- The team has made good use of past experience and expertise to facilitate Internet access of the HSECoE models.
- Analysis of correlation between vehicle performance and storage performance is very helpful. Availability of models on an Internet portal will help researchers worldwide to define their projects. There was a good response from 2013 comments with regard to fixed volumes.
- The project has a logical approach that generates insight into the usable hydrogen based on standard vehicles drive cycles.
- Development and application of hydrogen storage system models and storage system trade-offs can have significant impact on improving future engineering system design and development. System models that encompass metal hydride, chemical hydrogen, and adsorbent systems are being (or have been) developed on the project. These models could ultimately provide important predictive capabilities. The use of a web-based portal will ensure that the models are available to the entire hydrogen storage and system engineering communities. The emphasis has been placed almost entirely on model development; insufficient attention has been paid to model validation.
- Providing a graphical user-interface for HSECoE models will increase usability tremendously and allow generalists to access the models. Internet access will allow users without MatLab/Simulink access or experience to use models. The approach is appropriate and well-defined, and it integrated well with a number of other efforts.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- The project is on a good track. A metal hydride model, physical model, and cost model are complete. The 2014 presentation does not show whether the open milestones in the 2013 presentation are complete or still open. The status of the open 2013 milestones should be clearly documented in the 2014 presentation.
- Good progress has been made on incorporating Phase II performance data into the system models and on developing plans for making the models available to the public. Solid work has been performed on developing generic user-interfaces that should make the models more accessible to the user community. Useful information has been provided about user analytics; this should help to guide and fine-tune future embodiments of the models. Very little emphasis seems to have been placed on model validation. That work should be a critical element of the project going forward. Without experimental confirmation, it is exceedingly difficult to ascertain whether the models accurately (or adequately) address hydrogen storage issues in real-world situations.
- There is good progress on releasing models. The download page should include requirements for running model (such as Excel, MatLab, etc.).
- Current efforts to expand the user base are generally beneficial and should help to refine the models and the environment/interface.
- Work on the graphical user interface and populating the models with data seemed behind schedule, especially considering that the HSECoE is nearing completion. Much of this work could be done in parallel with other HSECoE functions.
- Given the reduced resources, NREL is more in a support role than in a lead role for technical accomplishments.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- Extensive and beneficial collaborations with HSECoE partners are evident. Especially noteworthy are the collaborations between NREL and Pacific Northwest National Laboratory/Savannah River National Laboratory on coding and conversion to Simulink framework, as well as collaborations with United Technologies Research Center (UTRC) on development of generic user interfaces and collaborations with multiple partners on model documentation.
- There has been close, timely, and appropriate collaboration with other institutions; partners are full participants and well-coordinated with the HSECoE and UTRC, in particular.
- It seems that all relevant partners/institutes are involved.
- It is clear that appropriate collaboration within the framework of this project exists.
- The principal investigator (PI) is working with all the appropriate HSECoE partners. Input from the vehicle original equipment manufacturers is critical to the success of this project.
- The project encompasses participants from industry, national laboratories, and academia, with important contributions being made by the collaborators.
- There is significant collaboration within the HSECoE, but limited interaction with non-HSECoE entities.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- Publishing models on an Internet platform is very good. Models for all storage systems will be available to be used by everybody.
- The project helps guide the development of materials with potential of meeting the DOE targets.

- Development and implementation of robust models capable of analyzing HSECoE vehicle performance, cost, and energy balance, as well as hydrogen storage system performance and trade-offs, are important to understanding and improving current future storage systems. The development of models with accurate predictive capabilities addresses most of the technical barriers identified in the DOE Hydrogen and Fuel Cell Program (the Program), and it is clearly relevant to the Program needs.
- The methods and models developed here will be useful to future investigators.
- Vehicle performance modeling provides storage system developers with valuable information to help direct development efforts.
- Providing vehicle-level modeling is necessary to provide the correct operating framework for the storage systems. Even a simplified system can provide good guidance to the HSECoE to develop suitable and relevant systems.
- The NREL project is small in size, but it is obviously providing a valuable service to enable the outcome from the HSECoE to be made available to the scientific public. The project provides great support to develop and apply models for evaluating hydrogen storage requirements at the vehicle systems level.

Question 5: Proposed future work

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

- The future work is well-defined and incorporates updates from collaborators. Trade studies are of particular value.
- The proposed future work is a direct extension of the solid modeling work already conducted on the project. Additions of the chemical hydrogen and adsorbent systems to the modeling framework are important. The presentation (slide 26) states that the focus of future work will be on “Model Validation and Model Web Access.” The former is vital; however, no details are provided concerning the approach for experimental validation of the models. A clear and compelling statement about plans for model validation is needed. Likewise, a statement about how (or if) the models will be maintained after the project is completed would be helpful.
- The list of future work is reasonable.
- It will be necessary to adjust the model based on the Phase III results that will be generated from planned HSECoE efforts. However, it is unclear whether this will be possible within the duration of this project.
- It would be useful if models could be made internet-accessible.
- The PI needs to catch up on previous slow progress and finish the models as proposed. This will be an excellent tool for future systems and materials developers to use to understand the operating environment of their materials and systems.

Project strengths:

- This is a useful and highly relevant project that is being conducted by a capable team that is well-coordinated and closely connected with other partners in the HSECoE.
- The project generates very useful information that related basic material properties all the way to standard vehicle drive cycles. This is expected to guide the researchers and engineers as they propose new/improved hydrogen storage materials/systems.
- The relationship between storage system characteristics and vehicle performance/operability is a valuable tool for storage system developers.
- The graphical user interface will provide a useful tool to visualize a complex system operation.
- The project has been very responsive to DOE and U.S. DRIVE Hydrogen Storage Technical Team suggestions and directions.
- Good model development is a project strength.

Project weaknesses:

- The project relies on the results generated from Phase II within the HSECoE program. Although there exists a plan to adjust the model per the results produced in Phase III, it is unclear how this could be possible within the remaining duration of this project.

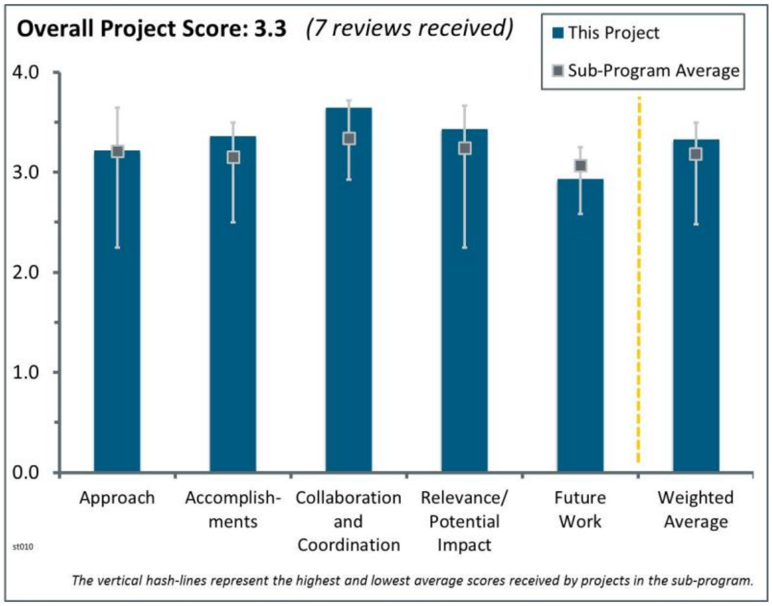
- Progress seems slow. It is unclear how much can be done in parallel with other HSECoE activities.
- There is a lack of clear and compelling plans for model validation. More attention seems to have been paid to web-based access and evaluation of user analytics than to the crucial task of model validation.

Recommendations for additions/deletions to project scope:

- Anything that can be done to make the modeling more widely accessible and user friendly will be invaluable.
- After the project ends and the models are completed and uploaded, all assumptions, material properties, and specifications should be updated on a regular basis to make sure that these models can be used in the future as well.
- It will be necessary for the sorbent model to be adjusted per the results produced in Phase III.
- A description of plans for maintaining the models after completion of the project would be helpful. A more robust validation plan is needed.
- As the public rollout continues, mechanisms to gather user feedback regarding utility and usability should be established.

Project # ST-010: Ford/BASF-SE/UM Activities in Support of the Hydrogen Storage Engineering Center of Excellence

Mike Veenstra; Ford Motor Company



Brief Summary of Project:

Material-based hydrogen storage systems have higher potential to meet U.S. Department of Energy (DOE) targets but have increased complexity over physical-based storage options. This project, led by Ford, is focused on three technical tasks that contribute to the overall Hydrogen Storage Engineering Center of Excellence (HSECoE) mission. Task 1 is to develop a dynamic vehicle parameter model that interfaces with diverse storage system concepts. Task 2 is the development of robust cost projections for storage system concepts, and Task 3 is to devise and develop system-focused strategies for processing and packing framework-based sorbent hydrogen storage media.

Question 1: Approach to performing the work

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

- This project is a key contributor to the HSECoE. It is well-integrated and provides valuable research and development and an automobile perspective to the onboard storage challenge. In particular, the project provides key input on adsorbents, e.g., metal organic frameworks (MOFs) and vehicle integration of hydrogen-storage systems. It aims to provide very valuable original equipment manufacturer (OEM) input.
- The project has clear goals and provides extensible inputs for modeling to account for variations in potential MOFs, should they become available. Unfortunately, MOF-5 will ultimately fall short of the DOE targets, but the efforts to maximize MOF-5 give perspective on perhaps a better MOF (some have been identified).
- The accomplishments are very good. The analysis and characterization techniques have been well-developed. However, it would be nice to see the HSECoE apply these developed methodologies to some of the new materials sets that have been developed since the HSECoE was initiated. There are limited materials that can be synthesized in the quantity needed, but the processes should still be validated for another framework material.
- The Ford/BASF/University of Michigan (UM) team plays many important roles in the overall HSECoE program. This includes the system architecture for the adsorbent system, most of the manufacturing and testing associated with MOF-5, coordination of failure mode and effects analysis (FMEA), and performance/cost-model development. The automotive industry perspective provided by Ford is invaluable. At the present time, this particular project seems to be a nerve center for the entire HSECoE because it performs (or interacts closely with those performing) pivotal aspects of the HSECoE's overall effort. The focus of work throughout the HSECoE (including this project) has reached the point where lack of relevance to critical barriers is a non-issue. The entire HSECoE effort is locked in on a successful demonstration of an adsorbent-based onboard hydrogen storage system.
- The approach is well-formulated and includes optimization of MOF performance in an adsorbent-based hydrogen storage system, scale-up of the MOF-5 manufacturing process, and failure mode analysis of MOF-5 under real-world operating conditions. The approach addressed important remaining issues in the successful development and deployment of a prototype system from an OEM perspective. An important addition to the overall effort was the assessment of the properties of alternative (known) MOFs that may

have volumetric and gravimetric capacities superior to MOF-5. This effort directed toward “enhancement of MOF performance potential” represented a mid-course correction to the original approach, and it has produced valuable findings that could significantly impact the Hydrogen Storage sub-program.

- The approach is generally effective but could be improved. It contributes to overcoming some barriers. The main barrier is the storage capacity of the absorbent. The choice of MOF-5 is driven by material availability. At the same time, (see slide 9 of the presentation) MOF-5 volumetric versus gravimetric capacity is close to optimal, which leaves relatively little room for further improvements.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The extent of accomplishments reported for the past year and the degree to which milestones were met is most impressive. Many technical issues concerning MOF-5 have been resolved. Fully formed MOF-5 pucks with enhanced thermal properties have been delivered to Oregon State University (OSU) for modular adsorbent tank insert (MATI) fabrication. FMEA and safety assessments have been completed. Ford has continued to serve the HSECoE in regards to research planning and execution, modeling and analysis, and being an effective liaison to the external community.
- Most milestones and extra work have been performed successfully working towards developing an understanding and methodologies that could potentially take advantage of a better MOF material. Methodologies and experimental tests have been developed to quantify properties. The system architecture has been developed significantly, and new efforts to identify potential MOFs to replace MOF-5 are promising. There are several items in the milestones that are underway and maybe on target for completion. For instance, only initial cycling has commenced on evaluation of degradation, and the FMEA plan has just been initiated. The scale-up and characterization of MOF-5 are the most developed aspects of the project, and additional MATI puck issues have been addressed.
- The approach is effective and contributes to overcoming most barriers. The main remaining barrier is the availability of an absorbent material with good hydrogen storage capacity, which is outside of the project’s scope.
- Significant progress has been achieved. However, on slide 16, relating to the MOF-5 scale up, while there may be “performance” metrics within experimental error, the size distribution has been significantly altered. There may be deleterious or beneficial effects from long-term cycling that should be addressed in future work.
- Solid progress has been made on all tasks during the present reporting period. Especially noteworthy is the new work on enhancing MOF performance potential. Four new MOF candidates having gravimetric and volumetric capacities superior to MOF-5 were identified. This is an important result that could have positive impact on the adoption of MOF materials as viable media in a practical hydrogen storage system. The manufacturing scale-up to produce 9 kg of MOF-5 with properties similar to laboratory-scale material is an important accomplishment. Likewise, the identification and analysis of possible failure modes for the adsorbent system in Phase II has been an important accomplishment that is leading to the development of robust strategies for mitigating risks.
- A lot of good engineering and associated work has been done. Numerous potential new adsorbents have been surveyed theoretically, showing some potential for improvement. Manufacturing scale-up, cycling, and integration of MOF-5 have been mostly worked out. The failure mode work is very useful. Although MOF-5 is better understood, it is still not evident if this material (or any adsorbent) has significant medium-term chances of leading to a practical onboard vehicular hydrogen storage system that will compete with high-pressure gas. There is a lack of preliminary OEM (Ford or General Motors) perspective on this important question.
- One key accomplishment was the production of 9 kg of MOF-5, thereby allowing manufacture scale-up and material improvements in thermal conductivity. The project also presented data on puck formation, but did not discuss how the pucks were made and if there were any difficulties in making MOF-5 into pucks.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- There has been close, appropriate collaboration with other institutions; HSECoE partners are well-coordinated.
- Collaboration and coordination seemed to be well-handled throughout the HSECoE as a whole.
- As with every Center of Excellence, collaboration is at the heart of its success.
- The importance of the active involvement of a major OEM in the HSECoE cannot be overstated. The Ford/BASF-SE/UM team is well-coordinated with other partners in the HSECoE, and that close collaboration and attention to “real-world” issues have become extremely valuable elements in the success of the HSECoE.
- There is excellent collaboration with the HSECoE members and a few others.
- This team (led by Ford) is as well-connected to the greater HSECoE as any among the pack of aligned contributors. The principal investigator (PI) projects a level of leadership that clearly benefits the entire HSECoE and fosters a much-needed inspirational connection to the automotive industry. The interest of that industry in the work of the HSECoE should be important to and much appreciated by the DOE.
- Collaboration exists between partners, but it was not clear who will make the MOF-5 pucks for later testing at OSU and Savannah River National Laboratory.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The project is well-designed and implemented. It is moving forward in parameterizing the laboratory-to-OEM production and implementation of a physisorption-based solution. It is clear that many of the obstacles and solutions generated here will impact any MATI-puck storage solution.
- The HSECoE system models and knowledge gained from MOF-5 will be extremely valuable to materials scientists as the researchers look to design and develop new materials for transportation applications.
- The relevance and potential impact of this project are fully aligned with the Phase III goals and objectives of the HSECoE. This is essentially true of all the remaining ongoing projects that collectively form the HSECoE. Their targets for Phase III are clearly defined, and all teams are properly focused. It is clear that all these efforts form one well-oiled machine, which is good news. The problem is that the Phase III targets are not nearly ambitious enough to justify a compelling argument for a higher level technology demonstration sponsored by, e.g., the DOE.
- This project is addressing important problems in the development of an engineering system based on adsorbent media. The OEM perspective incorporated into the HSECoE by the investigators from Ford contributes greatly to the success of the technical effort. The project has significant positive impact on advancing progress toward meeting DOE research, development, and demonstration (RD&D) goals, and it is a vital component of the overall HSECoE project.
- This project has direct relevance to the DOE Hydrogen and Fuel Cells Program goals via expert participation in the HSECoE. These partners have participated well.
- The project is aligned with DOE RD&D objectives by developing material-based adsorbent hydrogen storage systems. The project should have pointed out that the MOF material volumetric capacity needs to be doubled from 20 g/L to 40 g/L to meet DOE target. *[DOE note: the 40 g/L target is a system-level target and not at the material level.]*
- The impact of this project may be seen in multiple areas that may not be directly related to hydrogen storage for automotive applications.

Question 5: Proposed future work

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

- The project is entering its last year. The remaining planned work seems reasonable.
- The project appears to have a one-year extension. The future work plan for fiscal year 2014 completes this project.
- There are reasonable plans for wrapping up the project.
- The project is quite linear and does not seem to rely on contingency plans and risk management in most of its fact finding/understanding role. Several options may prove themselves as the Phase III tasks progress, but they are not discussed.
- The role of Ford/BASF/UM in Phase III mostly involves (1) completing ongoing MOF-5 optimization/testing, FMEA activities, and related tasks based in part on results from Phase III experiments, and (2) supporting the Phase III modeling and validation efforts. It is likely that most of this work during Phase III will be done by Ford.
- The proposed future work is clear and succinctly stated, and it is a logical extension of the excellent work that has been conducted thus far on the project. It is unclear if support exists for additional work on enhanced MOF performance (new material candidates). That will be an important issue for the DOE Fuel Cell Technologies Office to consider.
- It would be beneficial to see another material evaluated beyond MOF-5, as well as more long-term cycling of larger batch materials.

Project strengths:

- This was an overall strong project performed by a good team of researchers. The potential impact of the project's results, outside of hydrogen storage, is one of its major strengths.
- The project consists of clear goals, actionable items, and a good balance between university/laboratory and industry partners.
- The project features a strong team.
- There has been strong leadership from the PI. The team is productive and skilled in the disciplines required to perform the proposed research.
- A well-coordinated project team is conducting work that is vital to the overall success of the HSECoE effort. The technical effort on this project has produced important new results that are directly relevant to the development and implementation of a practical hydrogen storage prototype system. The investigators should be commended for seriously considering the recommendations from the 2013 DOE Hydrogen and Fuel Cells Program Annual Merit Review and expanding the technical effort to include the valuable new work on identification of MOFs with potentially enhanced gravimetric and volumetric capacities.
- The project demonstrates excellent expertise on MOFs and other adsorbents.

Project weaknesses:

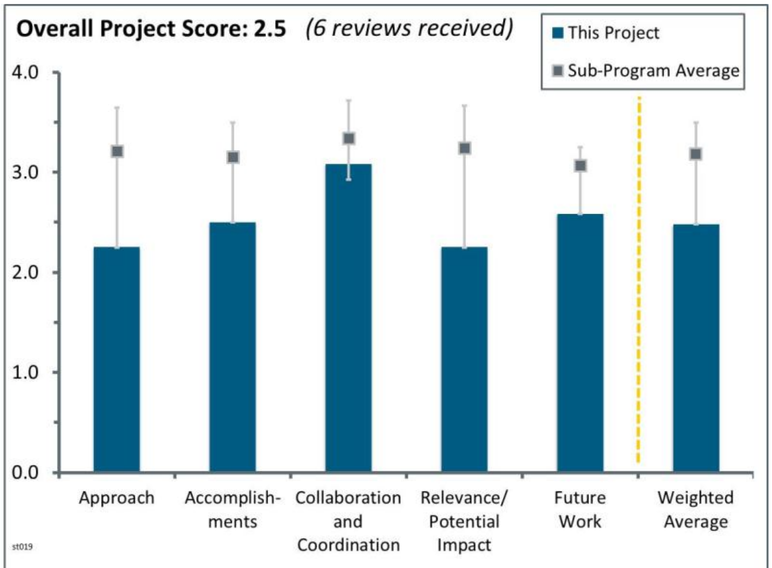
- This team has no obvious weaknesses.
- This is a strong project with no notable deficiencies. The only criticism is that the HSECoE in general (and this project specifically) has failed to adequately address the important system problem related to loss of usable hydrogen in the prototype design.
- Using MOF-5 as the tested hydrogen storage material is a weakness.
- The project is unlikely to develop a real physisorption solution that meets DOE goals.
- The limitation of the project to MOF-5 is a weakness. There are limitations instilled by project milestones-deliverables etc., but another materials set would be beneficial, even though it is not possible at this late date in the project.

Recommendations for additions/deletions to project scope:

- Make sure the PI stays deeply involved. When the work of the HSECoE comes to end next year, someone will have to go before the Technical Team, the DOE, and perhaps others to convince them that follow-up to the HSECoE is warranted.
- It will be important for the project team to work closely with the Hydrogen Storage sub-program to consider how the work concerning synthesis and testing of new candidate MOFs can be expanded/extended.
- There should be frank judgments from OEMs on the real-world practicality of adsorption systems for automobiles.

Project # ST-019: Multiply Surface-Functionalized Nanoporous Carbon for Vehicular Hydrogen Storage

Peter Pfeifer; University of Missouri



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Brief Summary of Project:

The objectives of the project are to (1) fabricate boron-doped nanoporous carbon (particulate and monoliths) for high-capacity reversible hydrogen storage, and (2) characterize materials and demonstrate storage performance.

Question 1: Approach to performing the work

This project was rated **2.3** for its approach.

- The University of Missouri (UM) project continues to explore the feasibility of enhancing hydrogen adsorption in high-surface carbons by partial substitution with boron (B) into the graphitic lattice via decomposition reactions using either liquid or gas phase decaborane ($B_{10}H_{14}$) followed by decomposition at elevated temperatures. While first principles computations are used to rationalize possible mechanisms for stronger chemical bonds for hydrogen molecules on boron-carbon (B-C) surfaces, most of the project's efforts have been empirical variations of the processing steps to search for improved storage capacities and higher heats of adsorption. The goal is to surpass behavior of more traditional adsorbents to allow greater capacities near room temperature. However, it does not appear that this approach produces a high fraction of the desirable B-C bonds or especially large surface areas for greater storage capacities. It is also unclear whether truly high volumetric capacities are realistic in spite of the extrapolations shown on slide 16.
- The approach seems good, but whether the boron atoms are in the right positions and are uniformly distributed is still questionable. Nevertheless, the team's initial idea/approach is good.
- The presented results and approach are primarily geared towards improving the physisorption amount at relevant temperature and pressure for onboard vehicular storage. Due to the nature of the materials being researched, the researchers will also potentially address thermal management, charging/discharging rates system cost, and system weight and volume. The efforts to incorporate boron into carbon frameworks is primarily pushed by the theoretical predictions that this can meet and exceed material goals at room temperature of which their previous work shows potential to achieve these goals. The researchers have changed their methodology to incorporating boron through gas-phase delivery of $B_{10}H_{14}$. Experimental characterization and uptake properties are relevant to benchmark the materials and compare to DOE goals.
- The principal investigator's (PI's) understanding of B-C chemistry is limited. This leads to an interpretation of results that is artificially optimistic. It also limits the advancement of the project. The interpretation of the x-ray photoelectron spectroscopy (XPS) data and the resultant assumptions about how much BCx (boron-carbon material) and other boron containing materials are significantly skewed from the reality of the materials set investigated. Considerable efforts need to be made to improve the focus of the researcher's efforts to more promising avenues. With this rudimentary understanding and its effect on their effort, it will be almost impossible for them to succeed or disseminate useful information to the scientific community at large.
- After nearly six years and numerous changes to the project's focus, it seems like focusing on boron substitution in carbon a few years ago should have resulted in much more progress. However, the lack of progress can be directly attributed to the very poor synthetic approaches chosen and the inability of the project to develop and perform accurate measurements for hydrogen storage and materials characterization, including the exact amount of sp^2 hybridized boron bonded in the carbon lattice.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.5** for its accomplishments and progress.

- The team performed computer simulation, experimental synthesis trials, and characterization. The results are impressive in comparison with the accomplishment of last year.
- The team is working toward increasing the enthalpy for hydrogen adsorption, while maintaining a high surface area. The results of the team's best materials are marginally better than MSC-30 activated carbon due to the reported small amounts of the correct boron-character being incorporated. It is unclear how definitive the XPS results for the B-B and B-C components are, but there seems to be boron distributed throughout the samples and lower oxygen content in the current materials. The higher temperatures needed to incorporate nominally increased boron content results in just a smaller population of similar pore volumes, but essentially no increase in surface-area normalized uptakes. Despite the apparent low sp^2 B content, there are some effects (perhaps also due to morphology changes) on the isosteric heats of adsorption. Slide 13 details the results for a pristine carbon- and boron-doped system where there is a significant increase of enthalpy for the latter. This is a surprising effect if only 2% of boron is incorporated in to the carbon. There are some concerns about the enthalpy data presented, in part because of the aforementioned discrepancy, but also due to the scatter in these data points that are much larger than the 0.2 kJ/mol indicated by the PI in person. Slide 26 details the methodology for obtaining these results from the isotherms of various temperatures, and this seems quite reasonable. Surprisingly, the data on slide 26 does not appear to present such a large variation with adsorption. Slide 16 indicates a very high film density (over 50% larger than the liquid hydrogen), which seems unrealistic. A scenario of such dense packing is not presented. A detailed description of these plots would be welcome as the captions are somewhat cryptic.
- After several years of effort, the UM team still finds rather low levels of B-substitution for its higher surface area materials. While its XPS results do indicate some improvement in the amount of B-C bonds during 2014 processing, there is still a substantial amount of B-B and B-O bonds, which are not useful for improving the hydrogen adsorption properties. Furthermore, while stronger binding with hydrogen is indicated at very low contents, these energies fall rather quickly and seem to be similar to conventional graphitic surfaces at 1 wt.% or higher. It also appears that the UM researchers have had difficulties with obtaining accurate and reliable hydrogen capacities from their Hiden Analytical, Ltd equipment that negated several of their prior claims for enhance performance.
- The PI has to be given credit for his efforts on validating and correcting his capacity numbers. He did spend significant time working with researchers at the National Renewable Energy Laboratory (NREL). It was refreshing to see that his numbers on standard materials were validated and that his approach was significantly altered after working with NREL. It would be of value to see this same level of effort on establishing reproducible materials sets. The limitation seems to be in the B-C chemistry. The overall concept of the proposal is sound; the interpretation of the characterization of the materials and how to apply those results to a future approach is the limitation.
- There does not appear to be any demonstrated advantage of the B-doped carbons over commercial carbons such as MSC-30.
- After nearly six years, and the project ending in November 2014, the team has failed to meet nearly all of its objectives and has come nowhere close to meeting the DOE hydrogen storage targets for light-duty vehicles. This is evident in several areas and for several reasons including the following:
    - The graph on slide 3 showing the predicted hydrogen storage capacities reaching over 12 wt.% at 77 K with 10% boron is clearly wrong and based on using only a "Langmuir"-type approach for modeling the isotherms. At low temperatures (77 K), the gravimetric excess capacities might go up a little, but because of the cold temperatures, the surface will become saturated whether boron is there or not. The only thing boron will do is perhaps increase the density of hydrogen on the surface a little. This minor increase should have virtually no effect on the total capacity difference with different concentrations of boron at 77 K. The main increase in capacity should only be observed at higher temperatures, i.e., 298 K. This means that the project's projections for volumetric capacity are also wrong throughout the presentation. For example, see the figure on slide 4.

  - While better than in the past, the authors continue to report excess adsorption numbers that are too high, e.g., slides 6 and 11. This probably also impacts their isosteric heats of adsorption results as well (i.e., slide 13). Clearly, the error in this data and the fact that the low amount of adsorption for the higher heats can be completely attributed to structure rather than boron is indicative of the poor data being taken. The authors also continue to report "total" adsorption numbers that are way too high due to wrong calculations. They need to work with others to get their calculations correct (e.g.., see the NREL presentation at this DOE Hydrogen and Fuel Cells program Annual Merit Review).
  - There are numerous papers in the literature that specifically identified real ways to increase the amount of sp^2 hybridized boron coordinated in carbon lattices. There is no recently demonstrated way to do this through chemical substitution from a vapor that results in high boron concentrations (i.e., 10 wt.%). The impetus for this project to spend several years working on this approach seems very unreasonable, considering the general understanding that the higher the processing temperature, the lower the boron saturation level becomes. It is amazing that the project even got 1% to 2% loading. But based on their interpretation of their XPS results, it is a good bet that they did not achieve even this level of loading. Having virtually no sp^2 coordinated boron would explain their anomalous results that showed no difference in hydrogen storage based on boron loading. Clearly other literature results demonstrated such dependence, but the literature results also indicated that achieving more than 1% to 2% sp^2 coordinated boron loading was very difficult.
  - On page 7, the authors provide conclusive proof that their approach used temperatures that are way too high to get boron into carbon. Instead of accepting the data, the authors claim to actually demonstrate new chemistry. However, they base this on conclusions from data like that on slide 12, where there is no obvious peak due to sp^2 coordinated boron. Published XPS results of sp^2 coordinated boron have clear peaks and thus, at best, the author's interpretation of 20% boron in the data and 1% to 2% boron in their samples is a gross overestimate.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The UM team has interacted very well with a number of outside organizations, especially for structural and spectroscopic characterization of their B-dope carbons. The XPS, transmission electron microscopy (TEM) and electron energy loss spectroscopy (EELS) results are most welcome additions to show chemical compositions of the samples. It was also useful and informative that hydrogen capacities were determined at NREL, revealing some issues with prior measurements done at UM.
- The team has a great mix of expertise.
- Working with NREL was a good step forward. Taking a new approach to data evaluation was a direct result of this collaboration.
- Collaboration with NREL should be helpful.
- There are several partners in the project that are mostly contributing to the goals. The monolith and industrial scale collaborations did not seem to contribute to the presentation, nor was any prompt-gamma neutron activation analysis (PGAA) data presented.
- The project should have worked more closely with a number of institutions that had previously performed a large amount of this work to minimize duplication.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.3** for its relevance/potential impact.

- Such exploration is the kind of idea deserving support from DOE. Hydrogen storage remains a daunting challenge except for tanks.
- The development of B-substituted carbons has the possibility of improving the potential adsorption storage properties if greater gravimetric and volumetric capacities were achievable above cryogenic temperatures. However, the associated larger heats of reaction would complicate the engineering issues associated with thermal management during both adsorption and desorption of hydrogen, and it is not fully evident whether this is really that much of an advantage.
- The limited advances in boron incorporation have restricted the impact of the project, but the materials development goals are well aligned with the DOE goals.
- The materials have potential, but it will not be realized with the current approach at UM
- After nearly six years of trying, it seems unlikely that this project will make a significant impact.
- The project did not improve any materials or successfully meet any targets.

Question 5: Proposed future work

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

- The tasks described on slide 19 are all relevant to addressing some outstanding issues with the nature and distribution of B-substitution in the UM carbons and their subsequent impact on observed hydrogen storage properties. With only a few months remaining for this project, it is not clear how much will actually get accomplished. The PI is strongly encouraged to complete as much of the planned experimental efforts as possible and to report the findings.
- There is limited time left for this project. There are several items listed that the researchers will pursue, but prioritization, proportioning of efforts, and risk mitigation are not discussed.
- There are only a few months left and thus it is uncertain if the proposed list of future work is realistic. The best course of action is to do a good job in summarizing the results in publications.
- With the short time left, there is virtually nothing the project can do to achieve its goals. The project has not developed any materials with significantly improved capacities.

Project strengths:

- The idea is good and the team is strong.
- The UM team has involved personnel with great expertise in boron chemistry, as well as equipment and experience for working on the adsorption properties of porous carbons and other materials. These capabilities have contributed toward using $B_{10}H_{14}$ for adding boron to carbon hosts.
- The project has a combined theoretical and experimental approach. There was broad team experience with materials development and characterization efforts.
- The current understanding of how to determine the gravimetric capacities of the materials is a project strength, as was the PI’s willingness to work with DOE to better evaluate the materials. The PI has shown that the B-doped materials have promise; unfortunately, the current approach has not led to a more in-depth understanding of the possibilities.

Project weaknesses:

- Throughout the life of this project, there has been considerable disorder in the approaches used to select and investigate the C-B candidates. Synthesis methods and characterization techniques seem to have been selected because they were either convenient or available at the university or research group. There was little discrimination on whether they are the most appropriate to address the stated objectives and needs. It appears that there have been improvements over the past year or so. In particular, there finally seems to be more appreciation on the challenges of accurately measuring the hydrogen storage parameters.

- There have been limited efforts to mitigate risks or try alternative methodologies for boron incorporation. Several mistakes in previous isotherms are discussed at the end of the presentation (even though these discrepancies have been identified), indicating poor data management practices in the team at some points in the past.
- The interpretation of the spectroscopic and thermal characterization of the materials is a project weakness. The volumetric capacity calculations need to be updated.
- The PIs have shown (slide 11) that the addition of boron does not result in an increase in hydrogen-binding energy and lowers the surface area. Further work on these materials is not recommended.
- The project did not meet any of its goals.

Recommendations for additions/deletions to project scope:

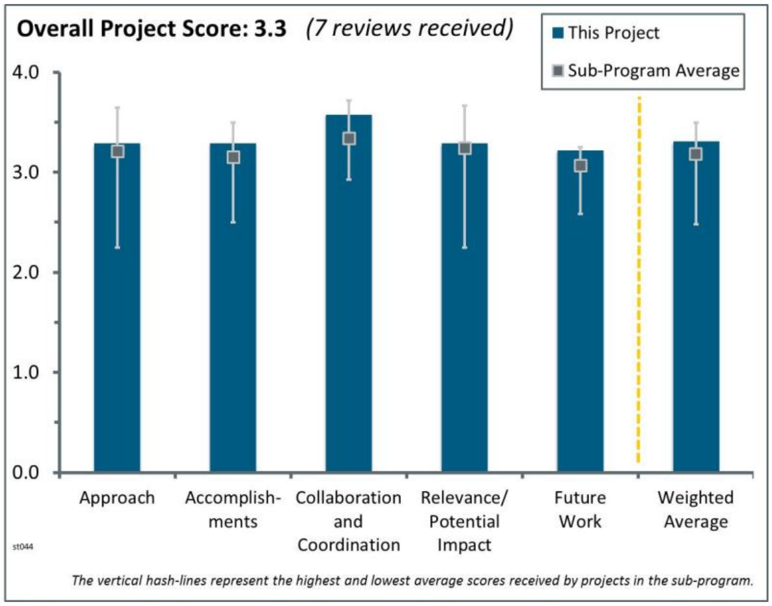
- The UM researchers should concentrate on completing the proposed experimental assessments that address the composition and structure of their B-C adsorbents, as well as perform careful measurements on the hydrogen storage properties on the most promising candidates.
- With only a few months left, the authors should use techniques in the literature to make sp^2-coordinated boron samples, and at the very least show that all their materials they have made in the past did not contain any sp^2-coordinated boron. The lack of progress of this project should not be viewed by DOE as evidence one way or the other for sorbent materials or boron-doped sorbent materials. The project's lack of results cannot be used to gauge the success of future projects that use more appropriate approaches to investigate these types of materials.

Project # ST-044: Savannah River National Laboratory Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Adsorbent Storage

Bruce Hardy; Savannah River National Laboratory

Brief Summary of Project:

The Phase III objectives of the project are to (1) design, fabricate, test, and decommission the subscale prototype hydrogen storage systems for adsorbent storage materials; and (2) validate the detailed and system model predictions against the subscale prototype system to improve model accuracy and predictive capabilities.



Question 1: Approach to performing the work

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

- This year's approach of testing sub-scale adsorbent prototype systems is what is needed to successfully complete the Hydrogen Storage Engineering Center of Excellence (HSECoE) program. This testing will improve model accuracy and predictive capabilities, which has been the goal of this Center of Excellence.
- The design and fabrication of prototype systems with validation of modeling to contribute toward their predictive capabilities is well executed. The medium-scale testing before large-scale testing has proven valuable. Step-wise approaches to measure system properties have been advantageous in determining contributions to physical properties. Modeling and system properties are strongly integrated and well aligned across the center.
- There is a logical, step-by-step approach, involving designing and testing first with a 0.5 L vessel, then with a 2 L vessel, as well as testing with and without various media in HexCells.
- The project has a reasonable and straightforward approach, comprising development and application of gas transport models, testing and validation of a cryo-adsorbent system model, and final design and testing of the HexCell adsorbent heat exchanger. Understanding gas and heat transport is crucial to optimizing the HexCell and modular adsorbent tank insert (MATI) adsorption systems. The experimental effort is complemented by a well-focused modeling effort that should provide a powerful predictive capability.
- Some fundamental data can be accumulated to overcome the engineering barriers.
- A small-scale approach is the logical first-step test to test a new storage system.
- The principal investigator (PI) investigated two different thermal modelling approaches. More explanation could have been provided as to why the helical system was rejected (while the explanation has been given before, it should be communicated consistently for new reviewers).

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- There is good progress in changing from the 0.5 L vessel to the 2 L vessel. There is good progress in performing test results and comparison to simulations.
- Important and useful results were obtained on evaluating power distribution non-uniformities in resistively heated HexCell assemblies and understanding the effects of the adsorbent media and heat exchanger

assembly in homogenizing the thermal distribution. Initial results using metal organic framework (MOF)-5 in the HexCell assembly are promising. The construction of a MATI prototype test facility at Savannah River National Laboratory (SRNL) is a valuable addition to the project. It will allow the MATI assembly to be tested under conditions commensurate with actual fuel cell operation. The complementary modeling and experimental efforts are contributing to a solid understanding of gas flow phenomena and thermal transport in the two adsorbent media assemblies that have been adopted in the project.
- The charging and discharging experiments were well done, and the model works for replicating the experimental conditions.
- The project milestones are significant to achieving project goals, with many potential stumbling blocks along the way. This was highlighted quite well by monitoring the heater cartridge temperature response and determining the non-uniformity, but testing proved that it behaved quite well because of the thermal distribution of the MOF in the HexCell, matching model predictions quite well. This has led to significant progress toward the 2 L storage tank and test facility. The MATI test system is on track.
- Accomplishments in this reporting period appear largely focused on the 0.5 L vessel, with a plan arranged for the 2 L system. While substantial work was accomplished, the budget for this project is quite high, (approximately $1 million per year). A cost breakdown between experimental and modeling efforts would be interesting.
- The PI delivered two systems. This was a very complicated task with very sensitive materials and systems.
- The project is listed as 90% complete, but has not completed the most important tasks of actually testing these adsorbent storage systems, which have been under study since 2009.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- Strong and fruitful collaborations between SRNL and the other partners working on the adsorbent systems are readily apparent. They are contributing significantly to the success of the technical effort.
- As with all projects under the HSECoE, the collaboration and coordination between partners have been excellent.
- As part of the HSECoE, the PI is working with all of the appropriate industrial and academic partners.
- Close collaboration is needed and well-executed with partners in this project. Significant progress is made through the combinations of design, testing, and modeling in the different institutions, with added perspective coming from external collaborations.
- Contacts to all relevant partners and institutes are established.
- It continues to be difficult to estimate how much collaboration actually occurs between HSECoE participants. However, since there do not appear to have been problems that interrupted the test schedule, the collaboration must have been at least adequate.
- Roles are unclear.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The potential impact this project will have on the U.S. Department of Energy’s (DOE’s) research, development, and demonstration is rated excellent, because the plan testing of adsorbent storage systems under design for several years is needed to validate the modeling of these systems to access balance of plant (BOP) mass and volumes.
- The adsorbent system has emerged as the primary candidate for prototype development in the HSECoE. The SRNL effort forms the centerpiece of the modeling, testing, and prototype development effort. It is critical to the success of the project and is highly relevant to the overall goals of the DOE Hydrogen and Fuel Cells Program.
- Focus is currently on mostly adsorbent based systems. The results of this work are universally applicable to compressed natural gas systems and future sorbents.

- The system focus of the project is geared toward DOE goals and will have significant impact even in the advent of another storage material being used, as the design and modeling are all extensible. Several challenges and design improvements have been made, and the testing capabilities will provide the backbone for further development. The downside is that these will not be stand-alone tanks that will be useful as-is on a vehicle at the moment. Further integration with insulation, etc. will need to be considered in the end.
- Even in the case that it is obvious that this kind of storage system cannot fulfill some DOE goals, it makes sense to complete this project. Results of this project will help to set the focus on future projects.

Question 5: Proposed future work

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

- The proposed testing of these adsorbent storage systems will provide the data validating BOP models. The goal of the HSECoE is to determine BOP mass and volumes, so without these experiments, the HSECoE will have fallen short of its goal.
- Significant barriers to achieving the project's future goals are not apparent, though perhaps present. The work is logical, building on successful tank manufacture and test facilities to parameterize the tank performances.
- The PI should focus on evaluating effects of vibration on the system performance.
- Future work will capture MOF testing in both the HexCell and MATI designs. This will be the true measure of project success, as it will validate both the modeling and the system designs.
- Test and validation of the 2 L vessel make sense and is the logical next step. There should be an outlook or recommendation for the usage of other materials (not MOF-5) with better performances to be tested in that vessel. A follow-up project should be established to build up a full-sized system (MATI and HexCell). Upscaling to a full-sized system will reveal more issues with the realization of material-based storage systems.
- A clearly stated and detailed plan for future work is presented. The plan addresses important work in both the HexCell and MATI systems. A better understanding of the thermal contact resistance between the heat exchanger and the adsorption media may be needed to optimize system efficiency. It is unclear how "the loss of usable hydrogen" problem is being addressed.

Project strengths:

- This project is vital to the success of the HSECoE. It is dealing with important engineering issues in a straightforward and compelling way. The project is being conducted by a well-qualified, highly capable team, and it is well-coordinated with other technical efforts in the HSECoE.
- The project used available storage materials, and the PI was able to deliver working systems.
- Project strengths included integration across the HSECoE, smart choices in deciding the path forward, and step-wise monitoring of tank properties, etc.

Project weaknesses:

- An actual onboard tank design with integrated insulation, etc. would be desirable.
- The project does not satisfactorily address the key technical deficiencies illustrated in the "spider charts," most notably the loss of usable hydrogen. Approaches and plans to mitigate problems that are system-dependent (e.g., loss of usable hydrogen) should be considered.
- MOF-5 does not seem to be an ideal hydrogen-adsorbent. It is questionable if the data accumulated for MOF-5 will be of use for any useful hydrogen adsorbents.

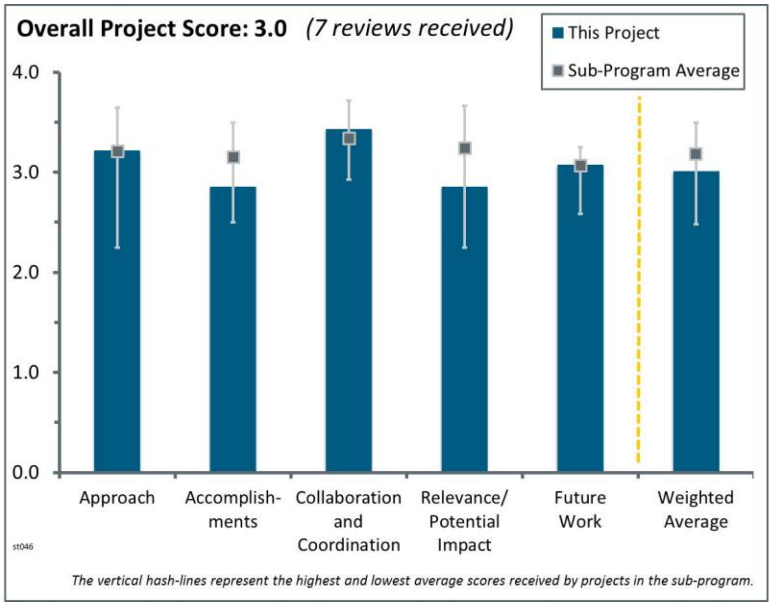
Recommendations for additions/deletions to project scope:

- An evaluation of packing density versus cooling channels for the HexCell system should be included in future work. The HexCell system selected was an off-the-shelf material for aeronautical applications. More work can be done to optimize the channel design for improved material packing and heat transfer.

- There needs to be consideration given as to how the HexCell system is loaded with MOF. There also needs to be attention given to keeping the MOF tightly packed against the heat exchange surfaces.
- There should be an increased focus on formulating approaches for dealing with the “white spaces” in spider charts; for example, loss of usable hydrogen.

Project # ST-046: Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage

Kevin Drost; Oregon State University



Brief Summary of Project:

The objectives of this project are to use the enhanced heat and mass transfer available from arrayed microchannel processing technology to: (1) reduce the size and weight of the hydrogen storage system, (2) improve the charging and discharging rate of the storage system, and (3) reduce the size and weight and increase the performance of thermal balance of plant (BOP) components. The project will use enhanced heat and mass transfer available from arrayed microchannel processing technology to design, fabricate, and test a modular adsorption task insert (MATI) prototype.

Question 1: Approach to performing the work

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

- The arrayed microchannel processing approach adopted in this project has emerged as one of the two principal approaches for adsorbent system prototype development in the Hydrogen Storage Engineering Center of Excellence (HSECoE). The approach is keenly focused on design, simulation, and experimental validation of MATI subsystems capable of facilitating efficient mass and thermal transport in a metal-organic framework (MOF)-based adsorbent prototype system. The combination of modeling, simulation, and acceptance testing of MATI subsystems optimized for densified adsorbent media is a crucial component in the overall HSECoE technical effort.
- The design and fabrication of prototype systems with validation of modeling to contribute toward their predictive capabilities is well executed. Step-wise approaches to measure system properties have been advantageous in determining contributions to physical properties. Modeling and system properties are strongly integrated and well aligned with HSECoE goals. Improvements have been significant from last year’s presentation in design and performance.
- Generally, the approach for the project is rated good, because it addresses system reduction in size and weight, charging and discharging rates, and determining BOP. Having Oregon State University (OSU) performing acceptance testing and Savannah River National Laboratory (SRNL) performing performance testing is also a good approach and an effective use of capabilities.
- The OSU effort is focused on a pivotal piece of the final feasibility testing and validation in the HSECoE’s signature proof-of-concept demonstration of an onboard hydrogen storage system. If successful, it will represent one of the first such demonstrations for a non-compressed hydrogen approach and will include cyclic charging and discharging of hydrogen into a tractable storage system for a fuel cell-powered vehicle. The MATI is a highly innovative (but also complex) structure requiring careful attention to all aspects of the fabrication/assembly. This approach is intriguing in principle, but also risky. Whether or not it will be “insensitive to mechanical failure of the medium” remains to be seen.
- Overall, the approach is well constructed and suitable for the work as defined. There needs to be a better explanation of how experimental system requirements and acceptance testing criteria relate to eventual onboard system requirements and criteria.
- As a partner in the HSECoE, OSU has been employing microchannel technology (MT) that enhances heat and mass transfer within components to reduce weight, volume, and cost of the storage systems. This

project does not directly influence the selection of composition of the storage materials themselves. The primary focus of OSU during Phase III of the HSECoE is upon adsorption hydrogen storage by compacted materials.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- Progress appears to nearly be on target, with fabrication of the subscale system nearly complete. Should testing of the 2 L scale prototype be completed this June, work will be on schedule.
- The goal is to demonstrate the feasibility and validate simulations of a 2 L MATI storage unit. To this end, there has been smart consideration of the cooling and charge/discharge functions within the prototype, a new cooling plate, and parallel modeling. The overcoming of difficulties in manufacturing/testing has been worthwhile, but full tests are still needed.
- The progress over the past year has been generally good, but this reviewer expected the project to be further along in terms of the MATI testing. While most of the key components of the MATI have been fabricated and tested to some extent, getting a MATI assembled, started up, and tested remains to be done in the coming year.
- Solid progress was made on meeting 2013 SMART goals for this project. Computational fluid dynamics modeling, fully instrumented MATI enclosures, and MOF pucks provided useful information to assess the design functionality and to guide and optimize on-going subsystem development. OSU investigators have worked closely with other HSECoE partners on optimizing the pressure vessel, cooling plates and headers, and media puck characteristics to ensure adequate subsystem performance. A more tightly focused effort on timely development and delivery of a MATI subsystem for acceptance testing at SRNL was an important research and development direction during this reporting period.
- During Phase III of the HSECoE effort, the OSU researchers have been working on the design and fabrication of a prototype MATI for experimental verification of its performance potential. The MATI could potentially facilitate heat transfers within the tank using compacted adsorbents, but its validation is still lacking. OSU has recently completed a detailed design review along with fabricating most of the MATI components for a prototype, as well as constructing a setup for acceptance testing of the prototype. However, assembly of the prototype seems to be at least a few weeks behind schedule, so meeting the Phase III goal of prototype delivery to SRNL for performance testing starting in August 2014 looks to be at risk.
- This year, the project was 82% complete from last year’s 72% completion. A 10% progress is rated satisfactory. The accomplishment this year was a complete pressure vessel design for the 2 L MATI storage system. The presentation showed a Computer-Aided Design (CAD) illustration of the MATI, but it only showed about eight MOF-5 puck layers. It depends somewhat on assumptions on the material properties, but a 2 L tank (10 cm diameter with 1.5 cm thick walls) should have more than 12 puck layers. If the storage system is to be tested in June 2014, then a drawing of the actual tank should have been in the presentation.
- It is not quantitatively clear how technical accomplishments such as improved heat transfer and temperature distribution contribute to improved system performance or durability, or how they ultimately improve metrics relative to DOE targets.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- Coordination and collaboration within the HSECoE has become a true strength of the Center. The OSU connection seems to be solidly in place. This is probably because OSU is tasked with supplying a critical piece of the final demonstration.
- The OSU effort was tightly coordinated with relevant work by other partners in the HSECoE. The collaborations with Ford and the University of Michigan (UM) (adsorbent puck development), Hexagon Lincoln (HL) (pressure vessel/header development), and SRNL (acceptance testing and code validation) are

particularly noteworthy. Those interactions have strongly enabled and supported the technical effort at OSU. They reinforce the relevance and merits of a “HSECoE model” for this kind of activity.

- Most identified collaborators appear to be actively involved in project planning and execution.
- There appears to be excellent collaboration between Ford and OSU on delivery of the MOF-5 samples. However, it was not clear who was responsible for making the MOF-5 pucks with enhanced conductivity.
- OSU interacted mostly with SRNL, HL, Ford, and UM regarding the development, fabrication, and testing of the MATI prototype. There do not appear to be any active relationships with the other HSECoE partners during Phase III.
- This project has to connect with several other groups in the HSECoE to be successful. It does this in several ways, relying on partners for MOF pucks, delivering prototype systems to SRNL for testing, and providing data for simulations. It is not possible to tell if there is also input from these partners regarding the system designs being developed, besides HL.
- The MATI subsystem is to be integrated with a vessel being fabricated by HL. Based on progress reported by HL, no issues are reported and integration of the MATI insert is the next scheduled step.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.9** for its relevance/potential impact.

- The high level of relevance of the OSU work to the goals of the HSECoE is unequivocal. In fact, the perceived success of the project a year from now will be based in large part on how well the MATI performs. The HSECoE has already done many good things in the form of (1) significant insight and knowledge relative to onboard hydrogen storage, and (2) analysis tools designed to probe and validate system performance. Failure to demonstrate a functional hydrogen storage system at some level would not bode well for a successful closure of the project.
- The system focus of the project is geared toward DOE goals and will have significant impact even in the advent of another storage material being used, as the design and modeling are all extensible. Several challenges and design improvements have been made, and the testing capabilities will provide the backbone for further development. The downside is that these will not be stand-alone tanks that will be useful as-is on a vehicle at the moment, and further integration with insulation, etc. will need to be considered in the end.
- The use of arrayed microchannel processing technology for adsorbent-based hydrogen storage prototype development is a key component of the technical effort in the HSECoE. By facilitating a reduction in storage system size and weight, improving sorption rates and thermal transport, and addressing key BOP barriers, the OSU project is closely aligned with DOE Hydrogen and Fuel Cells Program goals and DOE research, development, and demonstration objectives.
- Potentially, the MATI may reduce mass and volume of adsorption storage systems while enhancing thermal performance, providing the hydrogen permeation is sufficient within the compacts, the liquid nitrogen flow within the microchannels and discs provide efficient heat transfer into/out of the compacts, and the configuration of components within the storage vessels do not require extraneous volumes. Initial analyses indicate that higher costs due to fabrication and integration of these MATI components will make the overall storage system more expensive than using powder absorbents. OSU has yet to experimentally demonstrate the performance levels and robustness of MATI components during pressure/temperature cycling as well as their durability during extended operation. Fabrication of the internal structures using aluminum materials (necessary for reducing bed weight) and assembly of the MATI within light-weighted storage vessels have been incompletely addressed so far.
- The engineering challenge is to provide in situ cooling or heating within a vessel to make pressurized hydrogen storage through densified adsorbent media economically practical. The evaluation provided by the team indicates the MATI approach can provide more media per volume than other packing arrangements such as finned tubes, as well as meet functional goals necessary of the overall system to work. Alternatively, this allows more room for the hydrogen, given the presence of the thermal control apparatus. For this densified hydrogen storage concept to be successful, the MATI approach must not compromise overall system capabilities on the amount of hydrogen stored.

- It is not clear how the incorporation of improved heat transfer mechanisms "moves the ball" closer to DOE targets.
- The project was rated satisfactory in advancing progress in hydrogen adsorbent storage because of its good design of the 2 L MATI prototype reactor.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The future plans for work at OSU are as sharply focused as they could be. Its target in terms of a successful outcome seems to be well understood. A statement in the listing of Phase III MATI Functional Criteria caused some disappointment: "Provide data for model validation instead of meeting specific DOE goals." It seems to imply that either the DOE goals have become unimportant or that achievements will be so far from the goals that they are no longer embraced.
- During Phase III, resolution of fabrication issues and laboratory testing of the MATI prototypes are critical tasks that are mandatory to verify simulations of enhanced thermal performance. They also need to address issues and problems during component building and operating conditions (i.e., pressure/temperature cycling and fluid flow rates). Based upon the level of testing previously performed at OSU, it is unclear whether sufficient assessments of thermal performance and component robustness will be completed on new prototype MATI devices during the remaining duration of the project.
- The work has logical extensions to finish the assembly testing and validation. No significant risks are identified at this point in the project.
- The planned future work is appropriate for completing the approach as identified.
- The proposed future work is scored as good. The project addresses the remaining tasks of model validation and performing acceptance testing on the MATI hydrogen storage system.
- Future work entails assembly of a larger scale prototype, acceptance testing, and delivery to SRNL for performance tests. This work is followed by model validation and trial of "conduction enhanced pucks." This appears straight forward.
- The project is nearly complete. The future work is tightly focused on remaining project issues, especially completion of the MATI prototype and delivery to SRNL for acceptance testing. Although the plan for future work is reasonably complete and compelling, it is not entirely clear if there are any technical obstacles that could impede that effort. A succinct statement about remaining barriers (if any) to achieving final project goals would be helpful. The OSU investigators acknowledge that some thermal enhancement in the adsorbent puck may be needed to meet SMART goals. Although a potential solution (embedded aluminum pins) has been proposed, it is not clear whether that solution can be fully implemented and tested prior to the conclusion of the project.

Project strengths:

- The team at OSU has exceptional skill in the area of modular mass and heat transfer technology. Up to the present time, planning has been excellent and execution has been successful.
- This is an innovative project that comprises a solid technical approach implemented by experts in the field of microchannel array technology. It has significant relevance and importance to the overall technical effort in the HSECoE. The project benefits greatly from extensive collaborations and technical support from HSECoE partners.
- The goals and execution of the project are straightforward.
- OSU has unique experience with developing and fabricating MT devices for various purposes that suggest these assemblies may be suitable for those hydrogen storage components requiring improved heat and mass transport without sacrificing system weight and volume.
- Collaborations within the HSECoE are strong. The considerations in MATI design are detailed and provide an extensible test bed.

Project weaknesses:

- It has not yet demonstrated by laboratory testing at OSU whether the as-conceived MATI configuration will operate reliably under the variable pressure and temperature conditions that will be necessary for long life components in hydrogen storage systems. In particular, leaks between the different fluids could lead to very serious problems. While the project's models do predict high performance behavior from MATI under idealized configurations and operating conditions, robustness after fabrication and assembly of the components has not been sufficiently verified.
- There is some concern about the pace of work at OSU. It is unclear whether it has enough funding left to support the manpower needed to complete their remaining work in a timely manner.
- A comparison of cost, efficiency, and mass/thermal transport characteristics of the MATI subsystem with other competitive technologies is needed. A specific and candid description of technical risks and potential limitations/problems with the MATI approach to mass and thermal transport in an adsorbent-based hydrogen storage system would be helpful.
- There needs to be clear connections made between technical goals and accomplishments and progress toward achieving (or approaching) DOE targets.

Recommendations for additions/deletions to project scope:

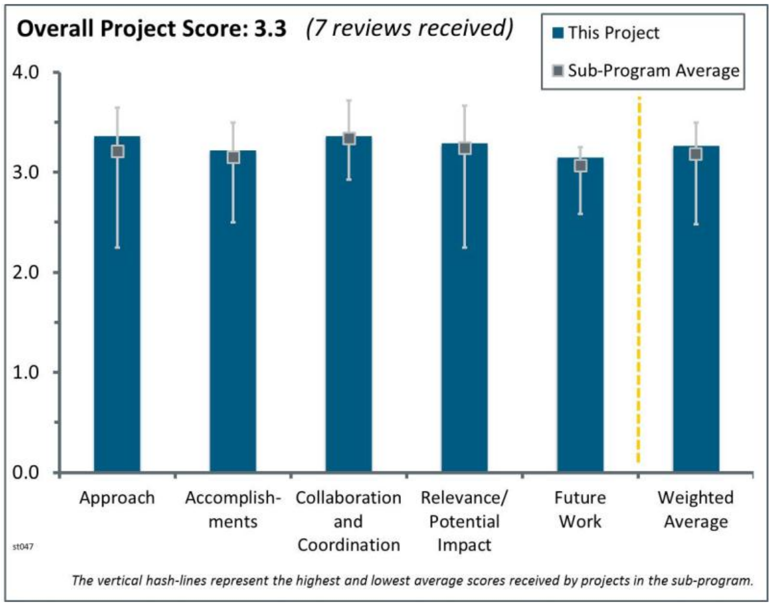
- The project is nearly complete, so significant additions to the project scope would be difficult to implement. However, a useful addition would be the development and testing of a method for enhancing the thermal conduction within the adsorbent media puck.
- OSU should confirm conceptual designs for the MATI devices via experiments. In particular, these demonstrations should show complete and reliable separations (i.e., no internal or external leaks of heat exchange fluids or pressurized hydrogen gas) during operation.
- The MATI is the most complex piece of the adsorbent bed test. It is recommended that the team stick with the plan of having several MATIs constructed in parallel for the validation tests. If one has a problem, it will be very beneficial to the progress of the project to have several backups. It is doubtful that one can put a MATI together overnight.

Project # ST-047: Development of Improved Composite Pressure Vessels for Hydrogen Storage

Norman Newhouse; Hexagon Lincoln

Brief Summary of Project:

The objectives of the project are to: (1) meet the U.S. Department of Energy (DOE) 2017 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 2017; and (3) work with Hydrogen Storage Engineering Center of Excellence (HSECoE) partners to identify pressure vessel characteristics and opportunities for performance improvement, in support of system options selected by HSECoE partners.



Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- Milestones are very specific and relate directly to the project. The approach keeps HexCell and modular adsorption task insert (MATI) tank designs as similar as possible.
- Hexagon Lincoln (HL) has performed good work in design and fabrication of the prototype pressure vessels. The finite element analysis performed to verify integrity represents best practices.
- The effort takes a systematic approach to storage development. More details regarding go/no-go requirements should have been provided.
- The approach undertaken is a bit different than other efforts. There is an attempt to build in flexibility in order to modify goals to accommodate changes or incomplete development in other coordinated research. This is realistic.
- The project built reusable tanks for testing the systems. The systems are probably over-built for automotive uses, but that is not the focus of this project.
- Cost and weight reduction are some of the most important criteria regarding the vessel development. Involvement of a vessel manufacturer is the best approach to gain results with a high confidence level. To start with a small-scale approach makes sense. It is good that not only performance, but also durability, safety, and operability are addressed. However, the strategy is not 100% clear. Design work of Type 1, 2, and 4 vessel designs are involved at different phases. It is unclear whether the goal is to develop a material-based or physical-based storage system.
- HL has a clearly defined role in the HSECoE program: to design, fabricate, and test tanks. Tanks are an essential part of the hydrogen storage approaches that remain under study, whether they are compressed gas- or adsorbent-based. HL's contribution is pivotal to the success of the planned adsorbent system demonstration.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The principal investigator (PI) built and delivered functioning systems to the HSECoE for incorporation into the systems.
- The project is on track and seemingly conducted in a very professional manner.
- Accomplishments of small-scale vessels include improvements in weight, volume, and cost. However, because there is no prediction to full-scale system, there is no correlation to DOE goals.
- HL has completed all the tasks assigned to it for Phase II. All the results reported in the presentation related to Type 1 tank performance were encouraging, which is critical to the success of Phase III of the project. The liner problem for Type 3 and Type 4 tanks remains to be solved (or so it seems). It is unclear how important this is in the context of planned system demonstrations for Phase III, or whether Phase III will include any tests of Type 3 or Type 4 tanks containing metal-organic framework (MOF)-5.
- HL has done a good job of meeting project timelines and milestones. The project is on target to meet all milestones. Down-selection to 2 L design and subsequent construction are on schedule. Weight reduction with 1-piece design is significant.
- There has been continued progress on separable Type 1 tank weight reduction. Work towards a number of Phase III milestones is still in progress.
- Progress continues in many areas, despite waiting on results from other coordinated projects.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- All relevant partners/institutes are involved.
- The PI is working with the HSECoE, which has the suitable academic, industrial, and government partners.
- The HSECoE structure of monthly/quarterly meetings appears to ensure close collaboration with performers.
- It is clear that HL is solidly connected to the HSECoE in terms of communications, coordination of design, fabrication, and testing details, as well as meeting delivery deadlines, e.g., for the Type 1 tanks.
- There has been good collaboration with other center members to establish design parameters and requirements.
- Work is well coordinated with HSECoE and other collaborators.
- This is a strong team that exhibits detailed coordination (e.g., communicating subcomponent dimensions for mating equipment).

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- There are several desirable impacts of this work:
  - Demonstration of improved capability with improved cost-effectiveness.
  - Demonstration of improved vessel subsystem capabilities (strength of carbon fibers at cryo temperatures, suitability of metallic liners, etc.).
  - Development of vessels designed as flexible “test beds” to help demonstrate the use of other technologies (e.g., MATI) that in conjunction will produce overall system gains.
- Low-pressure tanks for sorbent materials are required.
- Cost, weight, and volume reduction of a pressure vessel are very important, especially when a tank manufacturer is the project lead.
- HL’s contributions are critical to a successful outcome in Phase III of the overall HSECoE program. On the other hand, it was noted in slide 6 that the Type 1 tank is “designed to meet team needs for engineering

demonstration, but less responsive to overall DOE targets." Similar statements have appeared in several other HSECoE project presentations. It makes one wonder how far short of the original DOE targets the Phase III demonstrations will fall.
- The project is providing necessary tanks to HSECoE partners to facilitate other development efforts. It is not clear what the specific targets for the tank portion of a storage system are.
- The strategy of designing tanks for materials that are unlikely to ever be commercialized is highly questionable.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- A very good summary of future work activities was presented.
- The completion of tests with a 2 L vessel is good, as is the outlook for moving forward from upscale to a full-size system vessel.
- Future efforts should focus on methods to reduce cost and determine whether cheaper materials can be used.
- Future work is well-defined and will address several important development areas, including cryogenic Type 3 and Type 4 liners, external vacuum shells, and storage media installation into monolithic tanks.
- There is no issue with the proposed future work, as it directly pursues the goals of current work.
- Many of the Phase III tasks listed on slide 21 of the HL presentation seem ancillary to the remaining key outcomes that will ultimately define the overall success of the HSECoE program. For example, it does not appear that the results of these Phase III tasks will be incorporated in any storage system feasibility demonstration. At best, the results of HL's Phase III effort will provide some guidance for future technology development/demonstration beyond the HSECoE.
- Given the unlikelihood of current sorbent materials to ever see commercial use, the design and construction of additional tanks can hardly be justified. This is not a comment on the capabilities of the PI, but a programmatic issue.

Project strengths:

- The strength of project is the professional manner in which the activities are conducted on schedule, as well as achieving targeted results. Demonstration of a resin liner would be a novel and potentially significant achievement.
- HL's contributions to the HSECoE have been strong and steady. It is not the tanks that have limited the success in meeting DOE targets. Clearly, HL is a qualified, engaging tank manufacturer and a very good choice for the role it has played in the project as a whole. The PI has demonstrated great competence as the leader of the tank development effort. The work on the Type 1 tank for the adsorbent system tests looks to be exceptional.
- There has been solid design work and testing. There are strong analysis capabilities supporting design and construction.
- This project is providing needed and responsive support in the development of tanks for historically uncharacteristic operating conditions.
- The project directly pursues incremental improvements in vessel construction to improve performance. It does so with several different vessel constructions so that several potential paths of improvement are being simultaneously examined.

Project weaknesses:

- The only weakness (and it is simply a perceived weakness) is that HL's Phase III contribution may not bring much luster to the final list of HSECoE accomplishments.
- Other than the complexity of pursuing several different elements of vessel construction at once, no weakness is noted.

Recommendations for additions/deletions to project scope:

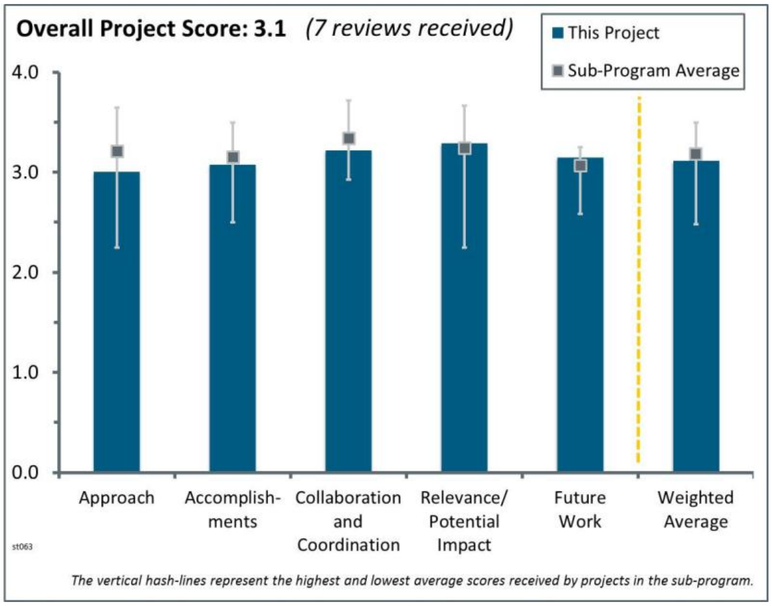
- The PI should provide more details or future study on designing liner robustness for cryogenic conditions. The PI should provide a list of suitable materials and operating parameters.
- "Resin liner" gas permeability needs to be explored.
- To counter the concern regarding the ultimate value of HL's Phase III results, DOE should consider going a bit further with the adsorbent system feasibility testing. If the Type 1 tank/MATI combination is a rousing success, there is no reason not to go one step further and do the full system demonstration using a Type 3 or Type 4 tank/MATI combination.
- It would appear that pre-formed storage media and monolithic tanks could be incompatible. Trade studies should be undertaken (not necessarily by this project) to evaluate the usefulness of monolithic designs with anticipated storage media configurations if completion of tank construction with media installed is not feasible.

Project # ST-063: Reversible Formation of Alane

Ragaiy Zidan; Savannah River National Laboratory

Brief Summary of Project:

The overall objective of the project is to develop a low-cost generation/regeneration process for alane (aluminum hydride, AlH_3), a promising hydrogen storage material. Key activities include characterizing the material's stability, thermodynamics, and kinetics, and evaluating its potential for fulfilling the U.S. Department of Energy (DOE) onboard hydrogen transportation and portable power performance targets.



Question 1: Approach to performing the work

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

- The Savannah River National Laboratory (SRNL) alane (AlH_3) project continues to make steady progress toward an electrochemically driven alane regeneration scheme. Its approach is logically driven by cost, and currently it is mainly directed toward niche applications, such as potential military applications. Here it appears that the cost of regeneration is either completely unimportant or of secondary concern, as the project appears to be concerned mainly with one-time use, disposing of the spent fuel canisters. The approach has added a small company involved in developing soldier-based fuel cell systems, where the cost of 'first fill' is still crucial, and so the project's continued interest in reducing cost of regeneration of alane makes sense for first fill for niche applications. As the path forward to reduce alane regeneration costs so that it could eventually be viable for onboard application, the SRNL approach is a good one. Although, in the end, it seems unlikely that the project will be able to meet DOE fuel cost targets for onboard/off-board regenerable systems.
- Electrochemical approaches for reversible formation of alane are interesting.
- The approach, an electrochemical generation of aluminum hydride, has been known for some time. It was attempted in the past (German patent DE 1141623 and Osipov, O. R.; Kessler, Yu. M. Elektrokhimiya (1971), 7(7), 923-7.) but never really implemented in larger-scale manufacturing. The main value of the current work is an attempt to design a "real life," scalable set of processes, which can be used to generate a high-capacity hydrogen storage material and regenerate the spent fuel.
- Off-board regeneration has many limitations. It appeared that the thermodynamic "costs" were going to limit the applicability of this approach for application to transportation beyond early market systems.
- Alane with 10 wt.% hydrogen is barely likely to make the 2017 DOE system target (when the entire storage system weight and volume is taken into account) and is highly unlikely to achieve any higher system targets, e.g., 7.5 wt.% hydrogen. It is no longer a candidate for demonstration in Phase III of the Hydrogen Storage Engineering Center of Excellence (HSECoE) program. Its contribution to overcoming barriers to the targets is not great. Therefore, in the context of achieving compelling Phase III accomplishments, further work on regeneration of alane seems unnecessary at this time.
- The electrochemical approach has great potential for efficiently regenerating alane. However, in the past, only very limited and general information was provided about process efficiency and process-limiting mechanisms. A more detailed examination of these issues and potential obstacles was a very positive addition to the approach employed during this review period. In this reporting period, there was a much sharper focus on identifying and implementing electrochemical methods to reduce alane regeneration costs. This is the critical issue for successful deployment of an alane-based system in either stationary or transportation applications.

- It looks like the electrochemical process touted by the principal investigator (PI) as the saver of the alane world is not working well after all. The team is back to using the traditional solid reaction method.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- Most targeted objectives have been accomplished. The progress is excellent.
- The science involved in this work on alane is actually quite good. The thermodynamic and electrochemical aspects are thoughtfully applied with modest success. Some barriers associated with regeneration have been overcome, but other barriers raised by the use of alane for vehicle applications seem insurmountable.
- Solid progress has been made in several areas, including more efficient $NaAlH_4$ regeneration using a new and less expensive catalyst, solid state alane production from $NaAlH_4$ via "dry" processing, understanding and quantifying costs and inefficiencies in electrochemical regeneration of alane, and use of alane-diethyl ether adducts for improved process yield. This work is providing increased confidence that a truly scalable electrochemical process for alane regeneration can be developed. A more detailed and understandable analysis of process-limiting steps and costs of individual process steps was an important (and welcome) addition.
- SRNL continues to exhibit good progress at every meeting, dealing with the multitude of problems it uncovers. One accomplishment has been to show that the kinetics of electrolyte recycle are quite slow, which may limit the overall efficiency and cost of the process. Nonetheless, this is an important observation, and the team is now focused on removing this barrier.
- Although positive results have been presented on a laboratory scale, the scalability of the electrochemical processes remains to be seen. The advantages of the electrochemical process over conventional chemical approaches may not materialize because of technical challenges associated with the need to run the electrolysis in a highly flammable and volatile organic solvent and the limited overall yield of alane.
- The PI has made steady, significant progress over the past few years. However, there appeared to be an overpotential limitation that the PI was unable to explain. The kinetic and thermodynamic barriers are significant, and a direct pathway to overcome these limitations was not clearly defined.
- The team accomplished relatively little in the past year. The electrochemical process did not look promising. The solid state reactions are not really different from what has been done before. The team demonstrated recycling of $NaAlH_4$ electrolyte with > 70% yield. The surface treatment is not any different from what was done by the Russian scientists.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- There has been close, appropriate collaboration with other institutions; partners are full participants and well-coordinated.
- It would be good to see collaboration with possibly the hydrogen storage group at the University of Hawaii or an International Energy Agency – Hydrogen Implementing Agreement member. A new external collaboration would give new insights and directions to approach the overpotential limitations mentioned above.
- A close collaboration with Ardica Technologies and SRI International (SRI) has been established. This is a positive and critical step to developing a scalable and cost-effective electrochemical process. A new DOE project, led by Ardica Technologies, will build directly on the work conducted in the SRNL effort.
- This project appears to be as plugged into the HSECoE as it needs to be at this time. It is not clear what the contributions of Ardica Technologies and SRI have been over the past year.
- This is a small project, and thus by its nature, collaboration may not be as extensive as in larger projects. It has recently added a small company partner, and that collaboration will likely assist the project in a higher fidelity cost model for its process.
- The extent of the collaboration is not very clear.

- The team is not really collaborating with other teams on the technical side. This is mostly a solo play at SRNL. The so-called collaboration is a Cooperative Research and Development Agreement to explore potential use of alane.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The project aligns well with the DOE Hydrogen and Fuel Cells Program (the Program) and DOE research, development, and demonstration (RD&D) objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- Development of an efficient and cost-effective method for regenerating alane would have significant positive impact on the overall direction and success of the Program. The electrochemical process being developed at SRNL and partner organizations is the leading candidate for regenerating alane at costs and quantities that are useful for the Program needs. The project is highly relevant and directly supports the overall DOE RD&D objectives.
- The project is relevant for early market applications.
- Of all the tractable hydrogen storage materials that have been studied as part of the work of the HSECoE, alane is certainly one of the best in terms of coming within reasonable range of the system targets. However, the spider chart for alane (notably missing from this particular presentation) still shows many substantial deficiencies, system gravimetric density being one of them.
- This small project may have an outsized impact on short-term DOE goals in the area of portable power and niche applications of hydrogen storage materials if the team can continue to make progress in improving efficiency and reducing cost.
- The advantage of alane to alanate is not clear. There is higher hydrogen capacity for alane, but since alanate is needed as a precursor, there is lower energy efficiency and one more step for regeneration.
- Alane looks attractive in terms of the high hydrogen content, but the cost of synthesis will be hard to overcome to make it practical. The electrochemical process was thought to be able to make a difference, but the results so far are nothing but disappointing.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- Future work plans are well defined within the scope of the current project.
- The proposed future work is a logical and reasonable extension of the current effort. It would be helpful if critical remaining technical issues could be stated and addressed more explicitly, i.e., the main obstacles that (may) limit scalability and efficiency and how they will be addressed. Maintaining clean electrode surfaces during the complex electrochemical process is critical for limiting over-potential in the reactor. Methods for mitigating electrode surface contamination should be addressed.
- The question here is about what the overall impact will be in regard to the feasibility of alane-based onboard hydrogen storage if this project is highly successful with their proposed Phase III research. In truth, more work on alane is hard to justify at the present stage of the HSECoE.
- The PI identified during the question and answer session that the kinetics of electrolyte recycle are slow (something around 50% conversion overnight, or some lengthy time). This may impact overall cost in a significantly negative way. It should become a focus of the project's future work to identify whether the kinetics impact capital costs significantly, and if so, determine ideas to accelerate the kinetics of electrolyte recycle. The fluid bed electrochemical reactor concept is intriguing and could help lower downstream separations costs as well as lend more of a continuous aspect to the overall process.
- The team is doing what is planned for the project, but it seems the final results will be far from anything practical.

Project strengths:

- The project has a strong team of materials scientists. Research results are independently confirmed (see German patent DE1141623 and Osipov, O. R.; Kessler, Yu. M. Elektrokhimiya (1971), 7(7), 923-7). There have been good collaborations.
- The progress the team has made has been good. The industrial partners' insight in batch processing of the samples is evident.
- The project involves smart people with good ideas doing high-quality science.
- Alane is a leading candidate for a storage medium that meets DOE targets. However, the problems associated with regeneration of the material have limited its use in practical applications. The SRNL project employs a novel electrochemical approach that may overcome the limitations and problems encountered in more conventional regeneration methods. Solid progress has been achieved in demonstrating the viability and scalability of the electrochemical regeneration approach. The results from this project have provided the basis for a new project led by Ardica Technologies to extend the electrochemical approach to produce large quantities of alane at greatly reduced costs.
- The project has a small team with potentially outsized impact, if the research and development continues to proceed successfully. SRNL has brought on a small company with a vested interest in its success, and this should help bring additional focus to efficiency and cost.
- The passionate PI is a project strength.

Project weaknesses:

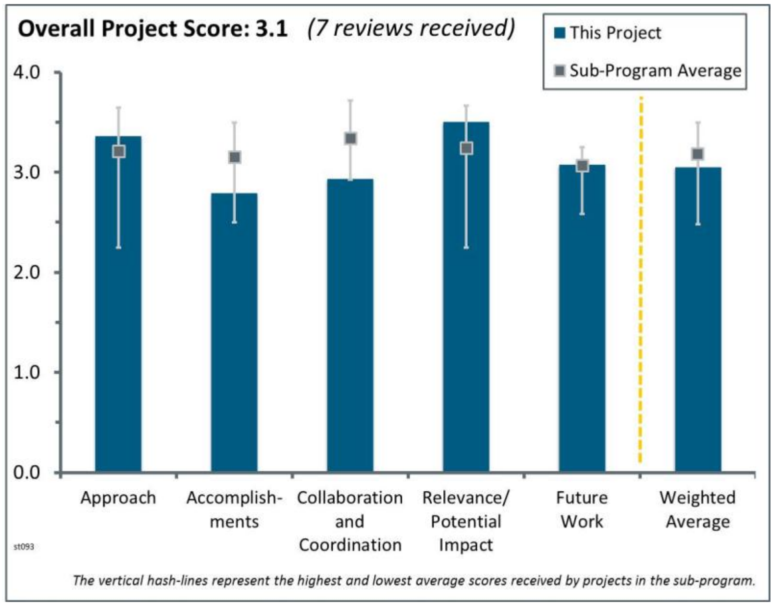
- With the addition of a small company partner, there will be no weaknesses.
- There are thermodynamic and kinetic barriers that will be very difficult to overcome.
- A more detailed description of efficiency-limiting steps (especially kinetics-limiting processes) is needed. Although conceptually straightforward, the electrochemical process comprises multiple steps, each of which has its own potential limitations. These should be addressed explicitly. Without that information, it is exceedingly difficult to assess the potential for achieving an efficient, scalable process.
- The advantage of alane to alanate needs to be clarified.
- The cost analysis does not seem to include the extensive processing cost (labor cost and capital expenditure). The loss of $NaAlH_4$ or $LiAlH_4$ also seems not to be included (only about 73% recovery).
- The high level of risk associated with scaling up the electrochemical process is the major drawback for the project. From the practical standpoint, if alane is really desirable in substantial quantities in the near future, it is recommended to back the current approach up by developing an alternative way of making alane in a safe and efficient manner.
- Working on a material that has little to no potential of meeting future onboard hydrogen storage targets that are at a level the automotive industry will consider attractive is the project's weakness.

Recommendations for additions/deletions to project scope:

- It would be interesting to know what Ardica Technologies and SRI are bringing to the table where this project is concerned.
- The inclusion of the Ardica Technologies/SRI collaboration into the project is an excellent addition that will provide expertise and experience needed to develop a scalable process.
- There should be more attention to the impact on kinetics on overall process costs.

Project # ST-093: Melt-Processable PAN Precursor for High-Strength, Low-Cost Carbon Fibers

Felix Paulauskas; Oak Ridge National Laboratory



Brief Summary of Project:

The objective of the project is to significantly reduce the manufacturing cost (>25%) of high-strength carbon fiber (CF) via: (1) development of alternative formulations for advanced precursors capable of being melt spun in high volumes; (2) enhancement of high-quality polyacrylonitrile (PAN) precursor melt spinning techniques for practical application; (3) development and demonstration of appropriate conventional and/or advanced CF conversion technologies; and (4) advancement of properties, scaling, and overall economics to meet high-pressure storage targets.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- This effort fits in very well with the other projects in CF. The learnings of this project can be extended to other programs. The goals of this specific project are well defined, and the project plan seems sound.
- Continuing the work from BASF to produce a precursor with a different method (melt-spinning) is a good way to reduce CF costs significantly.
- Focusing on the precursor chemistry before the filament processing appeared to be an effective approach. It is good to see that the project is moving toward the use of PAN-methyl acrylate (MA), which will allow for a better chance to achieving the targets. The approach should accelerate the cost model effort to understand the critical drivers in the processing that could affect the cost savings of the melt spun fiber.
- The approach of addressing costs of aerospace-grade CF by reducing the energy and cost for spinning a precursor is valid and should have an impact on the industry, specifically on storage technologies.
- Simplifying the preparation process of the CF has a direct impact on cost reduction.
- This project has effectively addressed the barriers encountered by previous work that was similar in nature and set a plan to overcome these obstacles. The approach is also aimed at creating a commercially viable solution to move this work forward. Switching PAN chemistry to use MA is an example of addressing barriers that have been discovered during this project.
- The comparison between the “melt” processing being investigated against the conventional “solution” processing makes it clear why melt processing is desirable to pursue. However, at the start of the presentation, it was made clear that while the use of PAN-vinyl acetate (VA) chemistry is feasible, the evaluation had to change to the use of PAN-MA because of material availability issues, a realization reported at the previous DOE Hydrogen and Fuel Cells Program Annual Merit Review. At this point, the impression given is that details of the approach involve a lot of trial and error work and the methodology cannot be presented because of Intellectual Property (IP) issues. Note that a subsequent presentation in this area was able to make clear the logic of testing through discussion of the test matrix and did not invoke IP issues. This did not seem like an adequate explanation by the principal investigator (PI).

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.8** for its accomplishments and progress.

- This project is meeting its goals. The main thing holding this effort back is resources (both equipment and human).
- The project appeared to take a significant step towards the feasibility of the melt-spun processing. It was useful to observe progress and results using PAN-MA.
- There have been good accomplishments up to now, but the delay of extruder delivery could cause a no-go decision at the next milestone. The target of >300 ksi is not shown in the presentation. Potential risks and future challenges should be documented.
- It looks like the project is starting to come together and produce solid results with new plasticizers developed, but still lacks consistent production of long segments that can be carbonized for mechanical testing. It will be interesting to see if the team can meet the third milestone with the extruder delays. The project is expected to really increase progress once the new extruder is in place.
- Much knowledge was gained last year on the barriers in using PAN-VA. The project responded well to this technical challenge, but in the process it needs to readdress the ability to achieve the project objectives. However, the team has remained focused on the project objectives while addressing the technical hurdles encountered.
- The status of progress was not clear because the PI stressed how complex and interdependent development is, and how coordination is critical in each area. Progress was reported as hampered by delayed receipt of equipment and a June milestone in jeopardy. No impact was indicated for fiscal year-end milestones. Accomplishments involving chemistry, spinning, and conversion were claimed.
- The feasibility that the melt-spun process could meet the costs target of the project has not been established.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The project has well-chosen partners.
- All relevant partner and institutes are involved.
- Collaborations between Oak Ridge National Laboratory (ORNL) and Virginia Tech (VT) seem to be increasing, which is positive. Reaching out to BASF is also a positive; however, it is not a U.S.-owned company. It would be nice to see a U.S.-owned company, such as Dow Chemicals or DuPont, become a partner.
- The project has good collaboration with VT but has discontinued effort with BASF, and there have been some delays due to the extruder. It is acknowledged that the project is searching for other partners. As the project develops the cost model, it would be helpful to have an industrial partner to confirm the manufacturing and feasibility assumptions. The project presentation indicated additional partners may compromise longer-term options, although a strong industrial partner may increase the probability of commercialization.
- Comments from previous reviews indicate additional collaboration may be desirable, but the presentation states that this may compromise longer-term options.
- The collaboration between VT and ORNL appears to function very well. However, the change in PI at VT may impact that collaboration and change this dynamic. The broadening of the collaborative team appears necessary at this point to optimize the PAN-based dope formulation from both a cost and performance standpoint. The project should consider adding a full industrial partner that can assist in and provide a steady supply of PAN-based dope.
- Collaboration with CF manufacturers (original equipment manufacturers [OEMs]) does not seem to be present.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- It is clear that despite the risk of being unable to accomplish goals, success would be a quantum leap for the manufacture of CF in reduction of complexity, reduction of cost, and reduction of environmental impact.
- This project can provide a significant reduction in high-quality CF costs and seems to be in very good alignment with the Hydrogen Storage sub-program goals.
- CF is the number one cost driver for pressure vessels. Reduction of more than 25% in manufacturing cost (as predicted) would be a huge step.
- If successful, this project could have significant impact on the cost of composite-wrapped pressure vessels by lowering the cost of CF. This will be very dependent on the CF industry adopting a new process.
- This project has high relevance because of the potential of advancing towards the DOE research, development, and demonstration cost objectives for pressure vessels. It would still be helpful to have a detailed cost model that confirms the cost savings to further highlight the opportunity of this project.
- If the feasibility could be demonstrated, high impact on cost reduction is expected.
- This project represents an opportunity to change the landscape of CF production. This is not only important to the DOE Hydrogen and Fuel Cells Program (Program, but may be more critical to Lightweight Materials and other CF-based programs. The higher strength CF needed by the Program may be too advanced for this technology, as this “game changing” approach may fall short of those strength targets; however, the results of this portion of the Program would still be considered extremely successful if properties adequate for lightweight applications are achieved.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The proposed future work is clearly outlined and has the appropriate steps toward developing the melt-spun fibers.
- Future efforts that target overcoming the technical challenges and in scaling up the process appear well planned. The feasibility of the project requires more urgent attention as to verify the economics of this approach and of changing to PAN-MA. The development of patentable IP requires a more definitive plan that includes how to add a PAN dope partner and protect the work accomplished to-date.
- Aside from current difficulties, planned future work appears to make sense.
- To produce a specific amount of CF tow is the logical next step. The PI should increase efforts to get a new extruder as soon as possible.
- The future work looks straightforward, but the precursor formulation work might be in jeopardy because of the passing of the PI at VT.

Project strengths:

- The excellent approach and use of previous work are project strengths. The approach as outlined is technically sound, and it represents an opportunity to change how CF is made and to impact its cost and use. This is a very important project and should be continued through scale-up.
- If successful, the process in development could change the industry.
- The project has an excellent team, and the team has learned a lot.
- The project strength is the potential impact on reducing CF cost for pressure vessels.

Project weaknesses:

- The project seems to be utilizing a trial and error effort towards optimizing the fiber. It would be helpful to have a better understanding of the systematic approached used to optimize the precursor and filament processing parameters.

- The project has not yet demonstrated cost reduction feasibility. The project also lacks collaboration with OEMs.
- Feasibility studies need to be continually readdressed as technical challenges are experienced and overcome. The data behind these feasibility studies, such as dope cost, processing throughputs, and quality impacts, should be made known to the program managers and project reviewers so that verification of project objectives can be addressed.
- It is not clear if there is some disorganization and if IP issues are compounding progress.

Recommendations for additions/deletions to project scope:

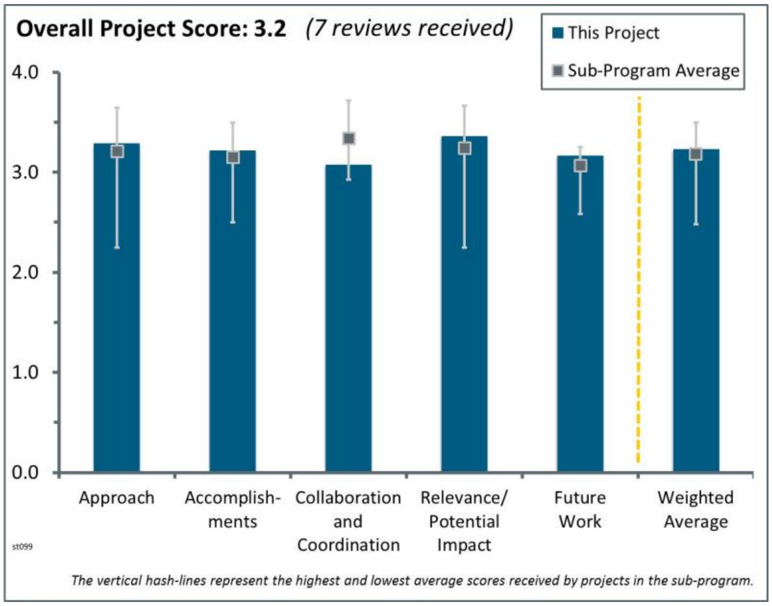
- Collaboration/regular communications with OEMs is recommended.
- The project should accelerate the cost model analysis and consideration of industrial partners. Also, the optimization of fiber parameters should be further developed and communicated as a key outcome of the project.
- This project’s fit within the Program should be reevaluated at Milestone 8. Achieving Milestone 8 but not Milestone 12 does not deem this project a failure, as this project represents a key CF development that will have an impact on the entire industry (even if the strength goals of Milestone 12 are not met).

Project # ST-099: Development of Low-Cost, High-Strength Commercial Textile Precursor (PAN-MA)

Dave Warren; Oak Ridge National Laboratory

Brief Summary of Project:

The objective of the project is to develop a low-cost precursor fiber that can be converted to low-cost carbon fiber (CF) with at least 650 ksi tensile strength. Fiber production cost modeling is also performed. For the precursor cost model (7,500 t/year line capacity), the precursor manufacturing will be evaluated at the level of two major process steps: (1) polymerization and (2) spinning. For the CF cost model (1,500 t/year line capacity), the CF manufacturing will be evaluated at the level of nine major steps.



Question 1: Approach to performing the work

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

- This is a good approach that addresses fundamental issues in CF. What is learned here can have implications in other related programs.
- This is a good overall approach to decrease the cost of the main cost driver, that is, the cost of CF. Starting with precursor selection, followed by process optimization, simulation, and final validation, is a comprehensive approach.
- The general approach of the project has been effective to optimize the fiber and exceed the objectives. The update on cost using the information from the Textile Research Journal was useful, but the independent cost model results are needed to confirm the opportunity of the textile fiber. Delivering fiber to a tank manufacture is an excellent addition to the approach.
- The approach of optimizing the processing of a textile grade precursor for yarn manufacturing has shown great promise in exceeding cost targets. An issue is that the SGL Group (SGL) now owns the precursor producer, but it might be converted in the United States. This will still create jobs, but ultimately will not contribute significantly to the Gross Domestic Product.
- The approach is well organized, and early coordination with FISIPE is well planned. The statistical analysis of the CF process optimization should be more evident in the work. The development of influencing factors and the rate of influence should be more evident and related to the down selection of the precursor formula. The effect of such key cost influences, such as CF conversion yield and A-quality spool yield, needs to be addressed.
- Exploring alternate synthesis of CF is necessary for reducing the tank cost.
- Despite the complexities of the CF fabrication process and elements of the fabrication process that are more art (trial and error)-based, the approach to cost reduction appears practically organized to achieve results.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The project has made excellent progress from last year by exceeding the targets and improving the tensile strength from 400 ksi to almost 600 ksi.

- Performance targets are being met or exceeded.
- The project is on a good track. Results from the past are showing a continuous improvement of tensile strength (ksi). In addition, it was mentioned during the presentation that the target of 650 ksi was reached and that the "roundness" issue was resolved. However, the presentation still shows "kidney"-like fiber shapes. The correlation between fiber shape and performance (strength) is not clear, that is, it is not clear whether the strength of a round fiber is higher than a "kidney"-shaped fiber.
- The project has shown steady improvement over time and steady efforts to overcome processing obstacles. The speed of property improvement could have increased once the precursor formula was identified.
- The effort appears on track and is exceeding planned technical goals.
- The modulus was exceeded (which was not that difficult), but the tensile strength target was exceeded by over 30%, which is a major accomplishment with textile fiber. Further optimization should demonstrate a textile fiber with tensile strength of over 650 ksi.
- It would be useful to investigate the potential of the preparation process on the properties of the CF.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- There are well-chosen partners that have the resources to expand and commercialize this work.
- The addition of a tank manufacture to evaluate the finished fiber for a tank application is an excellent improvement in the collaboration to gain confidence for potential commercialization. It would still be helpful to identify other partners or collaborations that could ensure the technical results further improve the industry besides just SGL.
- All relevant partners are involved. However, the principal investigator (PI) should clarify the CF requirement with Hexagon Lincoln (HL). Because HL was selected to validate the CF performance, the PI has to make sure that the provided CF will meet HL specifications to ensure that the validation tests provide appropriate results.
- The collaborations appear to have worked well within the framework developed by the partners. The ability of Oak Ridge National Laboratory (ORNL) to provide information back to the precursor producer, even when ORNL could not directly influence the corrective actions of the precursor, appears to have worked well.
- The project is limited in the collaborations because of the proprietary nature of the work. SGL-FISIPE will define the sharing.
- Collaboration with tank manufacturers would be helpful for this project to ensure that requirements for tank materials are met.
- Given the nature of the industry, the collaboration is international in scope. There is an issue of U.S. tax dollars helping an industry that is global in scope, as well as partners who have headquarters in other countries. However, it seems there is little alternative at present. It could be argued that the target of this research is the automotive industry and, since it is international in scope, aiding any party will benefit everyone.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- CF is the main cost driver of a compressed hydrogen storage system. Significant cost reduction was shown based on the simulation results.
- Direct reduction of the cost of major factors in the production of composite overwrapped pressure vessels (COPVs) is relevant. The presenter did a good job of explaining how the various factors were being addressed despite the confidentiality of some aspects of the work.
- The project has high relevance because it relates to the key cost driver (precursor) for CF that is the main cost element in hydrogen tanks. The cost pie chart in the presentation should be updated based on the current cost analysis by Strategic Analysis.

- This project could have a direct near-term impact on the cost of COPVs, as these precursors could be adopted right away by CF processors.
- The project could have a strong impact on reducing the cost of the high-pressure tank.
- This project has shown the capability to reduce precursor cost significantly (25%). Successful accomplishment of the cost savings level will clearly have an impact on the CF industry and ultimately the overall DOE Hydrogen and Fuel Cells Program (the Program). However, the ability to commercialize appears to be overstated, as the strategy of the precursor supplier and whether this precursor will becomes available to the industry remain unknown. A definitive path to commercialization is required by the precursor producer.

Question 5: Proposed future work

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

- The approach presented for proposed future work is straightforward and includes the following:
    - Completing fine-tuning the conversion protocol
    - Addressing scale-up
    - Completing the cost model
    - Delivering sample fibers for fabrication into a vessel
    - Testing and comparing to existing fibers
- The future work appears to be appropriate for overcoming the barriers and completing this project. The key items would be the cost modeling, achieving the >650 ksi tensile strength, and the fiber demonstration with the tank manufacture.
- Refinement of conversion protocol should be completed. Transferring the process from pilot precursor line to industrial fiber line and delivering fiber to a tank manufacturer are reasonable and important next steps to finalizing this project.
- The extent of refinement, partnering, and delivering 20 kg of CF to a tank manufacturer will directly demonstrate the impact of the project. However, it is possible that the surface treatment and sizing will need to be optimized prior to filament winding a tank.
- The optimization of the preparation process and its impact on the cost is missing and needs to be addressed.
- The future work aligns itself with refining the cost model and with continuing to approach the CF properties. Both of these tasks are necessary and appropriate; however, more effort may be required within these tasks. The cost model needs to include any yield loss and impact on A-quality spool yield. The impact of yield and product quality needs to be addressed in the CF optimization work, as only mechanical properties are targeted. Product quality and yield will be affected by each change in the CF process, and the future work needs to address this in more detail. The unknown factor is whether the quality of the precursor can achieve the variability targets, and if the precursor process could be modified to enhance its ability to maximize oxidation stretch. Consequently, the future work represents the right approach, but needs to include more in-depth effort and analysis to be considered fully successful.

Project strengths:

- The project strengths include good cooperation between FISIPE and ORNL and relevance to the overall goals of the Program. The steady improvement is also representative of a solid approach and an effective team.
- The project strength is the focus to reducing CF cost with a known “textile” grade polyacrylonitrile (PAN) precursor. The project has made significant steps towards meeting the ultimate goal for the CF strength.
- The strength is that the approach is practical in scope and addresses readily made improvements.

Project weaknesses:

- Unfortunately, SGL acquired FISIPE, so the precursor is now linked with SGL. This may have the effect of reducing flexibility in the supply chain—this remains to be seen.
- The delay in the cost model and evaluation of CF is a weakness that should be corrected prior to the completion of the project. Additional explanations for the optimization parameters would also be useful.

- The future work requires more detailed focus on the use of the textile precursor in a full-scale operation. For instance, questions concerning overall yield and optimized ability to stretch during oxidation need to be fully addressed. In addition, the commercialization plan needs to be identified. Efforts should be taken to ensure that this technology, once proven to meet all targets, is available to the industry.
- Perhaps more could be achieved if understanding extended to the "trial and error" portions of the research.

Recommendations for additions/deletions to project scope:

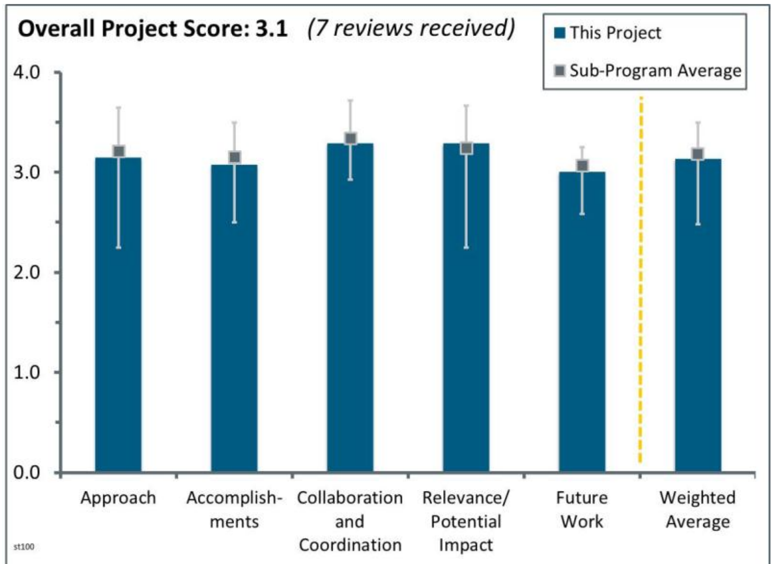
- The outcome of the project should include a transfer of knowledge and/or methods that can be used to optimized textile grade PAN precursors in order to benefit other CF manufacturers and avoid only helping a single supplier (SGL). The optimization parameters and effects in the CF processing should also be documented in detail.
- Product quality standards and cost impacts due to product quality need to be added to the cost models. The influencing factors of the precursor, such as filament shape, filament diameter variability, tenacity, etc., need to be identified so that a specification can be produced. Composite data in flat panels should be included, as pressure vessels are very complex structures and may not reflect such key aspects as fiber bonding and shear, flex, and compressive properties.

Project # ST-100: Ongoing Analysis of Hydrogen Storage System Costs

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:

The overall objectives of the project are to: (1) perform a process-based cost analysis of current and future hydrogen storage technologies, (2) gauge and guide U.S. Department of Energy (DOE) research and development (R&D) efforts, and (3) validate cost analysis methodology so there is confidence when methods are applied to novel systems.



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Question 1: Approach to performing the work

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

- It is a good strategy to focus on highest-cost balance of plant (BOP) items first (piping/fittings 26%, pressure regulator 12%, fuel tank controller 9%, and integrated in-tank valve 13%). The "Design for Manufacture and Assembly" (DFMA) method appears to be a very powerful approach to costing components. The limits are unclear, as is when the DFMA approach is and is not feasible. The team appears to be accommodating and providing timely analyses.
- To use compressed natural gas (CNG) BOP to make cost estimates for hydrogen BOP and the vessel is a good approach. It is a good idea to use DFMA for cost analysis.
- The principal investigator (PI) used systems from the DOE Hydrogen Storage Engineering Center of Excellence (HSECoE). While these systems are still in the prototype phase, they are the only publicly available models.
- For the current review period, Strategic Analysis (SA) has been primarily assessing costs for compressed gas hydrogen storage systems, with the major focus on the impact of the configurations and production volumes on the BOP components. Most attention has been on the numerous fittings in the baseline storage system configuration, as well as on the large ticket price components, such as the integrated valve and pressure regulator. The team's analysis methods are based upon scaling of the manufacturing processes and making comparisons to current production levels for both hydrogen and natural gas storage tanks. While this approach is reasonable and does permit some validation of the team's methodology, projected cost reductions at production rates exceeding 100,000 units/year probably do not fully account for safety inspections and verification of such high pressure (e.g., circa 700-bar), and it is likely that regulations will be imposed by various government agencies. Furthermore, significant reductions in BOP costs are probably only achievable with much more highly integrated and multifunctional components with demonstrable reliabilities.
- It seems like the team is drilling down and further understanding the impacts of BOP on the tank costs. The approach to cost analysis is standard DFMA methodology, with validation through quotation and modeling.
- The approach is okay; however, it would be useful for the BOP diagram to be carefully re-examined.
- The approach of this project is a process-based cost analysis to guide DOE R&D efforts. Previously, the project evaluated 350-bar and 700-bar tanks using DFMA. This year focused on the DFMA for the BOP components, especially the top cost components. In addition, the approach included a helpful comparison of CNG tanks and balance of components.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The PI did a good job costing out all the components and providing a scaling factor for sales volumes and system size.
- There has been significant effort on and significant results from BOP cost analysis. The project goal of guiding DOE R&D efforts was not as transparent. It is suggested that future cost optimization may be realized by component integration. It should be determined whether the project will provide recommendations as to which components should and should not be integrated.
- Validation of the DMFA analysis tool using CNG tanks was an important result.
- SA has completed significant revisions and updates to its past detailed assessments of costs for compressed gas tank BOP components that provide refined predictions during large-scale production. It has considered some variations and made good efforts to vet its results with several sources, including component manufacturers, commercial suppliers of hydrogen, and natural gas vendors, as well as some automobile original equipment manufacturers (OEMs). While its assessments may be adequate for parts manufacturing and assembly, this reviewer is concerned that SA has underestimated the degree of quality and safety inspections that will be necessary for delivery of large quantities of components to the downstream users. Furthermore, the high-pressure tanks will require very close tolerances and interface surfaces for the fittings, valves, etc. to minimize leakages upon integration into the vessel plumbing for long-term robust operation. SA does not seem to have addressed directly how this is done during high-volume production rates. SA has done only initial assessments on costs for lower-temperature gas storage and for updating the carbon fiber-resins compositions and configurations.
- “Life-cycle-assessment” is listed as a barrier in this project. However, it is not clear how this barrier was addressed. Some components have a high variant with regard to cost, e.g., fittings. This was well-understood and documented. The 2014 presentation is dealing with these high variants by additional investigations and increasing the level of technical details.
- The BOP understanding and refinement is needed to drive the efforts of the project. The DFMA analysis of the integrated valve does suggest routes for cost improvements. Validation through the use of CNG tank costs is a good approach since they are currently manufactured in significant volumes.
- The project was very focused on the dominant cost items in the BOP, which were the fittings/piping, integrated valve, and regulator. Additional details were added to the schematic to evaluate the fittings. The fittings assessment was based on price quotes and the DFMA analysis of Parker fitting, which had results similar to electric vehicle metal fitting. The analysis included error bars with sensitivity of assumptions. The integrated valves were estimated at $130 for CNG to $2,000 for hydrogen. DFMA aligned well with CNG but was lower for hydrogen than price quotes and slightly higher at high volume. The regulator was not analyzed with DFMA but had updated quotes. The result of the BOP analysis was similar but had higher confidence in results. The CNG comparison was a helpful and appropriate approach to gain confidence in the cost model. It was difficult to confirm the validation based on slide 18 due to different feedback from suppliers in comparison to the DFMA.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The PI worked closely with the HSECoE and Argonne National Laboratory (ANL) to pull together the necessary costs.
- There has been close, timely, and appropriate collaboration with other institutions; partners are full participants and well-coordinated. The project team includes ANL and the National Renewable Energy Laboratory (NREL) to provide complementary analysis.
- SA appears to work closely with its project partners ANL and NREL as well as with various outside sources, such as component and storage tank manufacturers.
- The project has a high collaboration with ANL in regards to system analysis and has made efforts to verify results using both other DOE projects (e.g., Pacific Northwest National Laboratory [PNNL]) and supplier

quotes. The role of NREL is not evident. The project has done an excellent job in collaborating with industry companies beyond its formal partners.
- Collaboration with ANL is good. The team seems to have a good relationship with tank vendors, which is a requirement for this project.
- While collaboration with ANL is visible, it would useful to show how the collaboration is conducted with OEMs, i.e., automotive and component suppliers, to ensure that the estimates provided represent a real-world scenario.
- At this level of design details, there should be more cooperation with component manufacturers.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The project has high relevance to guide the DOE portfolio and establish cost baselines for hydrogen storage technologies. It is important to have an independent cost assessment from this project to serve as a public reference showing options for storing hydrogen on a vehicle.
- The project provides great support to several DOE research, development, and demonstration programs.
- This project is currently assessing key issues associated with the overall costs of compressed gas storage tanks, especially related to BOP components. Methods for reducing these costs are certainly necessary for greater acceptance and usage of these vessels in vehicles. However, knowing and understanding these cost drivers does not necessarily lead to innovative engineering solutions. Much more highly integrated BOP subsystems, along with significantly reduced costs for the wall materials of the tanks, will also be necessary.
- An understanding of the cost of these systems is required to compare the materials-based approach to conventional 700-bar technology. The analysis provided a surprising cost savings as compared to 700 bar (due mainly to the pressure reduction of the system). The PI is encouraged to continue to integrate new discoveries and progress into the analysis as they become available (as the project has always done).
- Decreasing costs for BOP are necessary, but not as important as decreasing the costs for the vessel, which is the main cost driver (BOP ~30% and vessel ~63%).
- This project gives the Hydrogen Storage sub-program (the sub-program) a gauge to guide the development of technologies.

Question 5: Proposed future work

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

- The future tasks proposed by SA are reasonable extensions of its current efforts and are worth continuing. It should place greater emphasis on reducing the number of components and fittings via higher integration, as well as incorporating the least expensive structural materials (e.g., fibers and resins) and most efficient designs. SA should also continue to explore options for reducing costs of the higher-priced BOP components. There is little value at this time in looking into refining cost models for either onboard or off-board properties of the various solid storage media, because no candidates capable of meeting the DOE performance targets have been identified. On the other hand, extending the cost analyses and scale-up methodology to compressed hydrogen gas storage vessels now being used rather extensively in forklifts and similar vehicles would seem to be a useful means to validate its predictive capabilities.
- The future work seems to be effectively scoped to progress the cost analysis, which included the BOP integration and alternative materials along with the analysis of the PNNL cold gas system.
- Future work should include an analysis on the following:
    - The kind of break in period required
    - Manufacturing facility clean room requirements
    - System validation testing
    - Ability to withstand noise, vibration, and harshness (NVH)

- The new areas for analysis make sense, e.g., cold storage; however, it is not as clear how examining lower cost components in the BOP will make a great difference in cost analysis. For cost reduction strategies, it is not clear whether the project will provide recommendations for which BOP components should and should not be integrated. This could be the most valuable contribution. It is also unclear whether the project could do this with a life cycle analysis of individual components. It may not be advantageous to integrate components with a shorter life cycle with components of a greater life cycle, assuming replacing an integrated component would be more expensive than individual components. Cost analysis for other projects at greater maturity, e.g., Small Business Innovation Research projects, could be useful for benchmarking vehicular cost targets.
- Further integration of components and examination of lesser BOP should be reviewed with OEMs. At this level of detail, the design could be very dependent on the final vehicle application. It is not clear whether there are alternatives for stainless steel as the main material for the integrated valve housing. If yes, it would be good to know the potential risk of a material combination (galvanic corrosion).
- Proposed future efforts applying true DFMA to BOP components for reduction in BOP could be beneficial. Validation of PNNL pressure/volume improvements should be accomplished.
- Moving forward, it would be useful to have a representative BOP diagram. Caution is needed when considering other material of construction because of hydrogen’s embrittlement properties.

Project strengths:

- The project strength is the independent assessment of system cost and associated cost drivers. Another strength is the disciplined DMFA approach and full disclosure of assumptions and results (which is often not provided by others that perform cost analyses).
- The project was well coordinated with the HSECoE and modelling from ANL.
- The project team was very responsive to DOE and U.S. Drive Hydrogen Storage Technical Team suggestions and directions.
- SA has applied a systematic analysis methodology to evaluate manufacturing processes and cost analysis. It has recently revised its assessments with further refinements of the models and selection of input variables. SA revealed the sensitivity of predicting costs for large-volume manufacturing of compressed gas storage vessels.
- Project strengths included DFMA modelling and validation.

Project weaknesses:

- The project does not have significant weaknesses, but it could develop further recommendations to reduce costs based on the cost drivers.
- The systems available for modelling are based on the current prototype systems. These systems’ primary goal is to demonstrate operation and good system density. These systems are likely not amenable to high-volume manufacturing. More input from industry will be required in the future in order to scale up these processes.
- Any current cost assessments being performed by SA are hampered by two situations: (1) lack of accurate and comprehensive manufacturing and pricing levels being supplied by BOP and tank suppliers, since they usually consider this information proprietary; and (2) current low production levels (i.e., at most hundreds or low thousands) of either CNG or hydrogen high-pressure storage tanks, which greatly increase the risk of unreliable extrapolations to the desired 100,000s production levels. Consequently, the SA-projected cost levels will have larger uncertainty than indicated in its presentation.
- Collaboration with OEMs is not visible. The estimate may not represent a real-life scenario.

Recommendations for additions/deletions to project scope:

- The PI showed nice agreement between the DMFA-estimated cost for integrated valves and fittings, as well as CNG vessel prices, demonstrating and benchmarking the utility of the DMFA approach. The PI also mentioned using DMFA to find new places to reduce cost. This can be a great advantage to the sub-program goals and should be pursued.

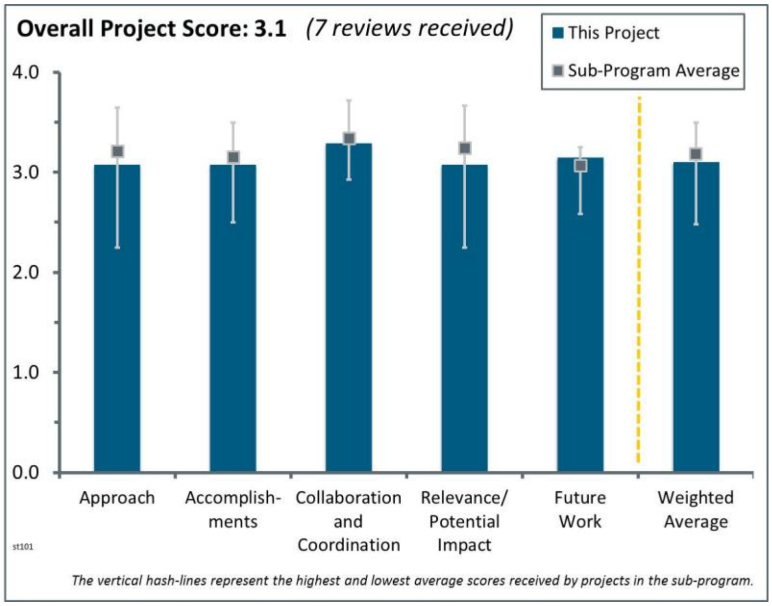
- It would be useful to have a sensitivity analysis based on potential cost reduction opportunities (e.g., integration of components or utilizing alternative materials) or hypothetical requirement changes such as burst or permeation modifications.
- Next time, more time should be spent on describing the loading procedures of the materials into the systems, i.e., determining how robust the process is, what room conditions are required, etc., if a large clean room is accounted for in the costs, or if glove box operations can be upgraded for high-volume manufacturing.
- SA should look much more closely into the impacts of higher inspection demands and verification of the safety aspects and robustness of the BOP components at maximum operating/acceptance pressures. It does not seem reasonable that costs for pressure and leak testing can be greatly reduced during large volume production rates compared to smaller manufacturing lots. A similar issue exists with pressure/leak testing of the final assembled storage vessels. Extending SA assessments to the status of costs for hydrogen storage tanks in forklifts and similar applications could be a good test for its assessment schemes.
- Sensitivity analyses would be useful, especially if it is not possible to access information from BOP charts.

Project # ST-101: Enhanced Materials and Design Parameters for Reducing the Cost of Hydrogen Storage Tanks

Kevin Simmons; Pacific Northwest National Laboratory

Brief Summary of Project:

The objective of this project is to develop a feasible pathway, through cold gas enhanced operating conditions, to achieve at least an additional 20% cost reduction for compressed hydrogen storage tanks above the 15% (13.5 kg composite, 9.6 carbon fiber [CF]) accomplished in fiscal year 2013 through resin modification and fiber placement. This will be accomplished through at least an 18.7 kg reduction in the required amount of composite (13.3 kg reduction in required CF) and demonstrated through detailed cost modeling of specific low-cost thermal insulating approaches.



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Question 1: Approach to performing the work

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

- The project team has done a good job of addressing obstacles and creating a path forward to achieve the target savings. The development of cold gas storage represents the step change needed to achieve the targeted savings. The impacts of the resin modification on the actual winding process including pot life, gel time, viscosity, etc. are being properly addressed. With the focus on cold gas storage, the other efforts remain in focus, providing a solid, full-system approach.
- The approach is addressing project goals.
- Minimizing the amount of CF used in order to reduce cost is an interesting approach.
- This Pacific Northwest National Laboratory (PNNL)-led project seeks to lower the costs during high-volume manufacturing of Type IV tanks for hydrogen storage at 700-bar through: (1) changing the composition of the CF-resin composites and wrapping the fibers more efficiently, and (2) reducing quantities of fiber/resin material via decreasing operating pressure and temperature to ~500-bar and ~200 K, respectively. The project's modeling assessments suggested the first approach could decrease costs by probably no more than ~10%. The second option might reach a net 30% cost reduction, but there would be penalties, such as needing insulation for retaining dormancy during storage, as well as uncertainties in mechanical and structural properties of the tank polymeric liner, resins, and fibers at lower temperatures. This project apparently does not consider the off-board impacts to the infrastructure that arise with cryogenic filling of the tanks or probable modifications to the balance of plant components.
- It seems much of the composite property work has been done with epoxy resin (the additive put into vinyl ester [VE] resin). The VE/CF interface is known to be poor. It seems some assumptions are made that the property translations will be the same even when the resin is changed from epoxy to VE.
- CF is the main cost driver for a 700-bar hydrogen storage system. Therefore, it is very important to improve the design of the pressure vessel and to compare it with a baseline. Because a 700-bar system cannot reach the U.S. Department of Energy (DOE) goals, it is a good approach to include a cold gas tank in the cost analysis. A cost breakdown of the manufacturing costs (TIAX estimated $290) should be shown to review the assumptions.
- The approach of the project is to model projections, prove the concepts empirically, and then build tanks and demonstrate as a system. So far most of the savings have only been demonstrated through computations, but some of the empirical studies in composite properties have validated the modeling.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- This project has advanced significantly over the year, with the presentation much improved from last year's. Efforts to address gas dormancy versus cryogenic storage are a notable example of the advances within this project and show the understanding of the team in pursuing the cold gas approach. The path toward the DOE goals is on pace, with all relevant and key topics showing continued progress.
- Further cost reductions are shown. The project is on a very good track.
- Technology advances sought are shown to meet cost objectives. Progress is on track with goals.
- The project has apparently completed its initial phase of developing and modeling a pathway for lowering costs of Type IV tanks through improving fiber/resin composition and adapting a lower pressure and a 200 K storage concept. The team has performed some testing of material properties and behavior of resins with nano-phase additives, but apparently is just starting to build and evaluate prototype vessels for validation of their models and predictions. It was not apparent from the team's presentation whether there are actually any attractive candidates with the appropriate properties that have been found to-date. The properties of these resins at cryogenic temperature were also unclear, and there did not seem to be any plans to evaluate them. There are issues with the number of test tanks that have yet to be built and evaluated, as indicated in the table on slide 20.
- There are many parts to this project. It seems most of the individual components are progressing and meeting goals, but there are many assumptions being made on how they will all come together.
- Most of the impactful accomplishments have been computationally based, but the team has done a great job in demonstrating properties of matrix modifications, nanofillers, and catalysts for curing. The team did demonstrate a burst of a baseline tank, but the real meat of the project will come in the third year, when all improvements will be combined and demonstrated empirically.
- It would have been beneficial to calculate the cost reduction based on the real processing function of the material and processes used.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- All relevant partners/institutes are involved.
- All team members—PNNL, AOC, LLC (AOC)., Toray Carbon Fibers America (Toray), and Hexagon Lincoln (HL)—seem to be communicating often and appropriately. The team also communicated well with Strategic Analysis, Inc. and Argonne National Laboratory (ANL) to baseline the composite modeling efforts.
- Collaboration with original equipment manufacturers (OEMs) is visible.
- The collaboration between partners is following a systematic approach where the level of involvement adjusts depending on the priorities. AOC's efforts with the resin have been a key element of the project during the year, whereas HL's efforts will greatly increase over the next phase. Toray's efforts are not evident, as the surface treatment and sizing efforts expected from Toray were not made visible during the presentation. Toray's supply of fiber and data is obvious, but its surface-related topics require more discussion.
- No apparent issues on collaboration were noted.
- While most of the effort seems to be at PNNL and HL, it does appear that the other partners in the project are also reasonably well involved. It is less clear whether interactions with outside organizations have been occurring. In particular, it is unclear whether the project is directly communicating with ANL and Lawrence Livermore National Laboratory concerning issues with cryogenic temperatures on materials and dormancy, which are likely to be problem areas.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The models show significant cost reduction that will meet the goals.
- Costs are one of the main barriers for a hydrogen tank system. This project is very important because it shows a pathway to reducing the costs by 37%, compared to a baseline model.
- This project is very relevant to the near-term deployment of fuel cell electric vehicles. These advancements could be adopted and implemented quickly by a tank manufacturer such as HL.
- The project is relevant to reducing the costs of the tanks related to CF.
- This project is now showing very good relevance with the accomplishments associated with cold gas storage and use. This was not necessarily evident last year. This project is now well focused on achieving the goals and making a significant impact on the DOE goals for hydrogen storage.
- The project estimated cost reduction opportunities of 48% before insulating costs. When the projected insulating cost margin is included, the projected cost saving for a composite tank is estimated to be 37%, which is significant.
- According to presentation slide 10, there would be only modest improvements toward the mass and volume targets (along ~10% cost reduction) of 700-bar Type IV tanks from this project. Only the much more risky low-temperature storage option would lead to the significant enhancements alluded to in this project. More radical materials or designs are probably necessary to reach the DOE goals.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- It is very good and important to validate the estimated cost savings; therefore, the proposed future activities support the overall goals in a very good way.
- The future work of combining the individual empirical and modeled improvements and validating them with an actual tank system will be critical to the success of this project.
- The future work is well planned and properly addresses the necessary steps. The pressure vessel characterization requires additional insight other than burst testing. The final composite is very complex and requires additional thought as to proper characterization, including critical use analysis such as cycle and drop testing where shear, impact, and compressive properties are influential, as well as thermal effects.
- There are no issues with proposed future work.
- It would be useful to optimize the materials and processing function of the properties and costs saving.
- It would be great if the project can complete all of the tasks outlined in slide 28. However, it does not seem probable that prototype and full-scale tanks will be fabricated and tested, given the current status shown in slide 19. There appear to be both unresolved materials and test facilities issues to be dealt with.

Project strengths:

- The strengths of this project lie in the leadership and direction provided by PNNL. The potential impact this project could have on the DOE goals can be viewed as a strength as well.
- Probably the best aspect of this project is PNNL forming a team with a vehicle OEM, a tank manufacturer, and materials (i.e., fiber and resin) producers to address issues with improving the design and construction of Type IV storage vessels in order to decrease costs. This has allowed specialized resources and expertise to be directed toward a common purpose. The fabricating and testing of prototype tanks is also an excellent aspect.
- The project has a skilled team and appropriate collaboration with stakeholders.

Project weaknesses:

- The 2013 presentation shows some open milestones. The completion of these milestones should be documented in the 2014 presentation.
- It seems that there is somewhat too much dependence on cost estimates, which are not based on the optimized materials and processes created within the scope of this project. It would be beneficial if costs reductions were measured based on the optimum processes found.
- The pressure vessel characterization could use some further review to better understand the influence of hybrid fiber designs, resin matrix usage, and temperature on properties beyond burst testing.
- The only viable pathway for this project to reach its 30+% cost savings goal is to implement a reduced pressure and cold gas operation, as only minor improvements in mass, volume, and costs targets will occur at ambient temperature. However, the robustness of the resin, liner, and fibers being considered are either unknown or highly uncertain at cryogenic temperatures and probably will not be sufficiently evaluated by the team. There also seem to be issues with the fabrication and testing of prototype vessels (see slide 19), with just a little over a year remaining in the project. It does not appear that any full-size tanks will be made and evaluated. The project seems to underestimate the impacts on both performance and cost targets of providing sufficient thermal insulation to the tank for acceptable storage dormancy and robustness.

Recommendations for additions/deletions to project scope:

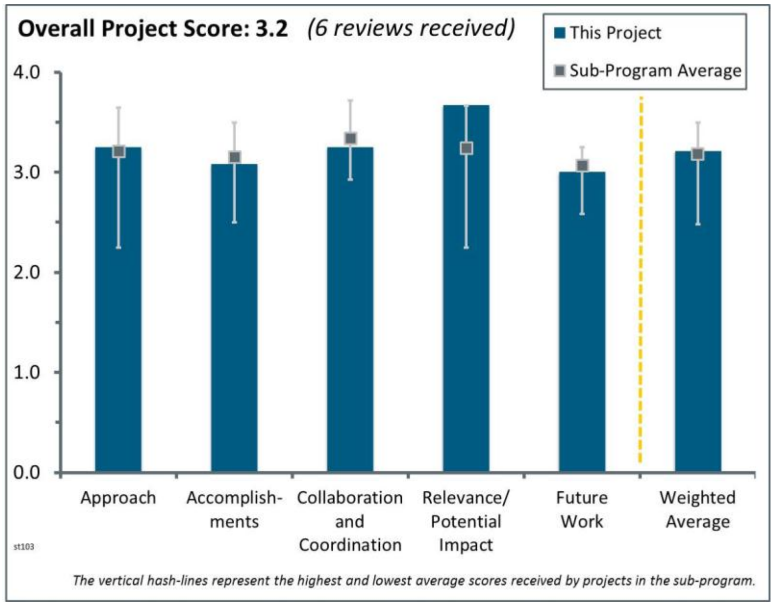
- It is suggested to include costs estimates based on the optimized resin material and related processes.
- The scope is well planned and continues to address key aspects of the project. Added focus on fiber surface chemistry influences using the proposed resin system should be added and demonstrated during the next year.
- There are no suggestions for deletions to the project; however, more screening of the cryogenic properties of fiber/resin materials would be valuable in order to see whether any of the proposed candidates do actually have appropriate behavior to be used for cold gas storage. The project team should also look more carefully into the requirements and impacts of improving thermal isolation and insulation materials for the cold gas storage option.

Project # ST-103: Hydrogen Storage in Metal-Organic Frameworks

Jeffrey Long; Lawrence Berkeley National Laboratory

Brief Summary of Project:

The objectives of the project are to: (1) research and develop onboard systems that allow for a driving range greater than 300 miles, (2) seek materials that offer the potential for meeting the U.S. Department of Energy's (DOE's) targets of reversible uptake, and (3) synthesize new metal-organic frameworks (MOFs) capable of achieving the -15 to -20 kJ/mol adsorption enthalpy required to be used as hydrogen storage materials operating under 100 bar at ambient temperatures.



Question 1: Approach to performing the work

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

- The team's approach is excellent in terms of exploring higher enthalpy binding sites of MOFs. The expertise in synthesis of MOFs, the first-principles calculations of the structure and binding energy, and the neutron characterization is an excellent mix. MOFs are an attractive class of compounds to be explored for hydrogen storage.
- Unsaturated metal centers with multiple bound hydrogen molecules is one of the few ways that any materials are going to be able to meet DOE hydrogen storage targets for light-duty vehicles.
- The approach for the project appears to be effective in progressing the exploration and characterization of MOF materials. The high-pressure measurements have significant error bars, which should be reduced prior to utilizing them as a conclusive result of the project, and/or the error bar should be shown as a function of pressure. The approach seems to be focused on improving gravimetric density without improvement in volumetric density, which is an issue in achieving the ultimate goal of a driving range greater than 300 miles.
- The approach is generally interesting. However, it is based on some high-risk assumptions, namely, that Mg-based MOFs can contain unsaturated/open metal sites capable of binding substantial amounts of hydrogen, which may not work out.
- It would be beneficial to see more detail on the specifics of materials synthesis and characterization. The principal investigator (PI) mentioned several times the stability of the "new" MOF materials; specific experiments on how those were evaluated would be helpful.
- Achieving multiple hydrogen-metal bonds is a laudable goal, but likely very difficult. The computational component could be improved if it were to operate in a more predictive fashion on a wider range of compositions. Fewer than 10 compositions were examined for their hydrogen binding energies. It is not clear that computation is truly guiding synthesis efforts. Thus far, the neutron diffraction experiments are providing information (e.g., one hydrogen molecule per metal site) that can be discerned from the uptake curve and crystal structure alone. It is unclear what new insight is being derived from this activity.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The team is making steady progress toward the project goals and deliverables.

- Authors have demonstrated, through structured studies, a preferential absorption near the cobalt site in the MOF. However, the hydrogen storage capacity of such systems is rather limited.
- The PI has made significant efforts and progress. It appears that this work may be more of a DOE Basic Energy Sciences (BES)/National Science Foundation future effort than an applied DOE Office of Energy Efficiency and Renewable Energy effort. It appears there is some confusion as to whether the gravimetric milestones of the project are hydrogen total or excess, which should be clarified. Also, specifics of how volumetric capacity is calculated should be outlined in the additional slides.
- The project made progress in evaluating various MOFs for the potential increased binding enthalpy. The synthesis of the M_2(m-dobdc) was a useful accomplishment as a potential cost reduction to the MOF-74 linker (although the cost benefit was not quantified). The Ni_2(m-dobdc) resulted in a significant increase in the isosteric heat of adsorption, which is a step in the right direction. The neutron powder diffraction provided good insight regarding the polarization of the hydrogen molecule. The project, through the computational analysis, provided the influence of the initial adsorbed hydrogen on the other binding hydrogen energies based on the open metal site concept.
- In general, the project has shown good progress, and it is great to see the project team reporting room temperature results as its metrics. While total adsorption on slide 14 is interesting to see, the more important metrics are excess adsorption and/or the amount of adsorbed hydrogen at the system level. The total absorption has too many misleading components to be very usable for evaluating the progress of materials development. A simple calculation indicates that the present approach to put one metal atom on an organic linker in the MOF structure will at best produce materials with less than 1 wt.% of additional hydrogen storage capacity because of the metal centers. This can only be changed by substantially increasing the number of metal centers, perhaps even by more than an order of magnitude. A simple calculation shows that if there are only four carbon atoms used to support one Mg atom, then the hydrogen storage capacity at best, with four hydrogen molecules adsorbed per Mg, is around 10 wt.%. If there is one Mg atom per 40 or more atoms, then even if with four hydrogen molecules per Mg atom, the additional hydrogen storage capacity will be less than 1 wt.%. The use of a single temperature isotherm to calculate the isosteric heats of adsorption should be minimized, and the use of two or more temperatures should be instituted as routine.
- Thus far, there is no indication that achieving adsorption of multiple hydrogen molecules per metal site is possible. Given the large budget allocated to this project, disappointingly few new compounds were synthesized/tested during the past year.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- There has been close, appropriate collaboration with other institutions; partners are full participants and well-coordinated.
- There has been strong collaboration across the team members: Lawrence Berkeley National Laboratory, National Institute of Standards Technology, and General Motors.
- The project has a good team with appropriate interactions and collaborations.
- It is not apparent based on the oral presentation and the presentation slides whether collaboration has been very effective.
- Capacity levels should be verified through an independent laboratory.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- The project, should it produced the material desired, would be very well aligned with the DOE Hydrogen and Fuel Cells Program research, development, and demonstration (RD&D) objectives. It would have the potential to substantially advance progress toward DOE RD&D goals and objectives.

- The project is very relevant to the DOE hydrogen storage targets. The potential impact is high if the project is successful. A systematic exploration of new MOFs for hydrogen storage is one area that DOE should be supporting, and this team seems to be the best in this area.
- If the project continues to develop materials with increasingly higher hydrogen capacities, then these materials would have a significant impact on future materials design and implementation for both transportation and stationary applications.
- The objectives of the project are well aligned with the DOE RD&D goals.
- The project approach is one of the only ways to meet DOE targets. The main issue is whether the appropriate metal sites can be made and the solution contaminants adequately removed.

Question 5: Proposed future work

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

- The future work is well aligned with the project deliverables, including finding the right ligands for metal insertion (especially Mg), characterization of the MOFs and their interactions with hydrogen, and high-pressure adsorption measurements, as well as first-principles predictions of optimal metalation conditions and the best metal cations for optimal metal-hydrogen interactions.
- The future work proposed is well designed. However, it does not guarantee that final goals will be met. It is still not obvious that a high-capacity Mg-based MOF, containing stable unsaturated coordination sites on Mg, can be successfully synthesized.
- The future work is clearly defined in order to progress each task. It is uncertain if the defined future work will make significant steps towards the multiple binding site concept in order to achieve the projected adsorption isotherm on slide 5.
- Task 4 (high-pressure isotherms) seems unnecessary given the stated goal of <100-bar operating pressure. Redirecting funds devoted to this task elsewhere is suggested.
- In addition to improving processing and finding appropriate ways to form metal sites, the project needs to focus on increasing the density of unsaturated metal sites by an order of magnitude or more.

Project strengths:

- The team has excellent expertise on synthesis, modeling, and characterization. The focus on charge-balancing ligands as well as high-valence metal cations is a promising direction. The team is paying close attention to the stability of the MOFs while considering potentially higher binding energy and thus higher desorption temperatures. The Co_2(dodc) has already reached 2.5 total wt.% hydrogen at 298 K and 140 bar. New MOFs are expected to achieve even higher hydrogen storage capacities.
- The project has a strong research team, well-thought-out collaborations, and good fundamental science.
- The PI’s insight and advanced knowledge of framework materials is evident. The approach and extensive characterization of the materials set leaves little doubt as to the team’s performance.
- The project is well-aligned with DOE goals and has a knowledgeable team.
- The project has all the right capabilities to perform the work, as well as an appropriate approach and focus. The synthetic components of the project are very appropriate, and the understanding of the very difficult challenges appears to be good.
- The strength of the project is the focus on achieving the ultimate goal of sorbents, which is to increase the adsorption enthalpy at room temperature.

Project weaknesses:

- There has been limited progress during the past year.
- The project clearly requires a material that can bind hydrogen to multiple binding sites per metal. The project includes theoretical projections of the multiple binding site concept, but the path to demonstrate the concept and protect the binding site needs to be further developed.
- There is limited applicability of the project’s results for real hydrogen storage systems. This is a high risk project more appropriate for BES. Practical implementation aspects of the expected results are left out. For

instance, even if they were possible to make on a laboratory scale, Mg-based MOFs with free coordination sites may be not scalable or may be impossible to handle on a commercial scale.

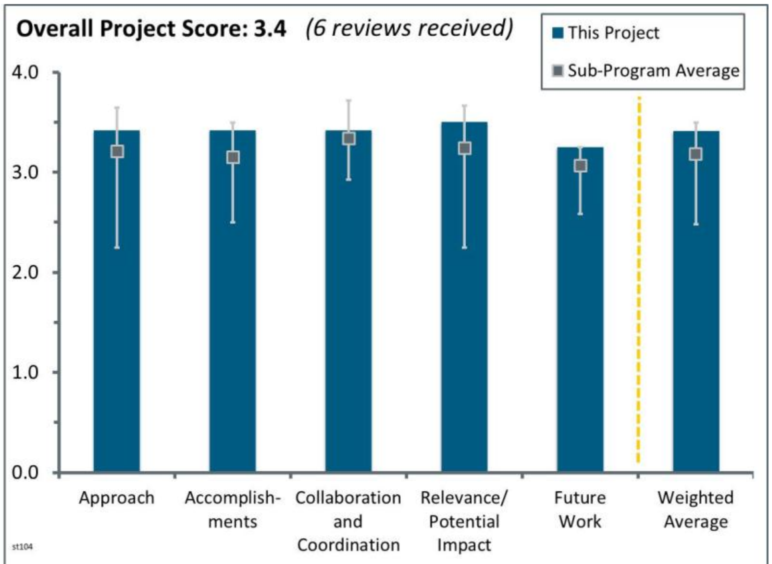
- There are two main issues:
    - It will be challenging to develop appropriate metal centers where the chemical contaminants can be adequately removed. This may require a fundamental change in the way the materials are made and the way the contaminants are removed.
    - To meet DOE targets, even if every metal atom reversibly adsorbed four hydrogen molecules, only a few (i.e., four or five) carbon atoms per metal site would be allowed. In the relatively large MOF structures being discussed, it appeared that there will be dozens of atoms associated with the support for every single metal atom. Therefore, the materials presently being discussed will only have, at absolute best, a 1 wt.% or 2 wt.% hydrogen storage system capacity at room temperature.
- It seems that first-principles calculations are not providing enough up-front guidance to experimental exploration. The success of the project depends on the right metallation and activation of the charge-balancing ligands during synthesis. The ability to synthesize the predicted MOF structures/compounds may be a more significant challenge than the team has envisioned, and may be physically impossible.

Recommendations for additions/deletions to project scope:

- No additions or deletions are suggested at the current stage of the project.
- Clarify the capacity numbers; please report in excess, not total. Clarify how volumetric capacities are calculated.
- Delete Task 4.
- There is a need to focus on increasing metal site density and access. The use of a single temperature isotherm to calculate the isosteric heats of adsorption should be minimized, and the use of two or more temperatures should be instituted as routine.
- The project should either reduce the measurement errors or eliminate the high-pressure adsorption tasks. Additional priority should be placed on developing materials with improved volumetric density. The project should include some preliminary material cost analysis because a complex material may improve the binding energy, but it will not be feasible if the material cost is too high.

Project # ST-104: Novel C-B-N-Containing Hydrogen Storage Materials

Shih-Yuan Liu; Boston College



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Brief Summary of Project:

The objective of the project is to develop novel chemical hydrogen storage materials that have the potential to enable non-automotive applications and meet the 2017 U.S. Department of Energy (DOE) targets for vehicular applications with focus on three classes of materials: (1) liquid phase, (2) potential reversible, and (3) high capacity.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- In a primarily materials discovery activity, this team continues to follow an excellent approach to seek out new materials. Along the way, the team quickly assessed key properties and continuously down-selected to the most promising materials. As the team has learned more about the chemistry of carbon, boron, and nitrogen (CBN) heterocycles, it has continued to be able to adjust its synthetic approach to explore new concepts in the reactivity or physical properties of these CBN heterocyclic compounds. As in previous years, this approach incorporating computation, experiment, and some practical assessments of performance continues to answer crucial questions that enable better understanding of novel chemical hydrogen storage materials. The project represents a well-coordinated effort directed at potentially reversible systems in the novel class of CBN heterocyclic compounds. The team demonstrates an active down-select mentality to rapidly assess materials, cease work on less promising compounds, and focus on the most promising candidates. This approach provides valuable insight to DOE and the current and future materials discovery efforts. It is very valuable to make a complete and accurate assessment of such new materials concepts. DOE should stay the course while this very competent team continues to make sure the research and development (R&D) is well performed, and the R&D team completes a reasonably comprehensive survey of the possibilities that these novel compounds may provide.
- Effective approaches from both experimental and theoretical sides have been presented for overcoming barriers toward reversible hydrogen storage materials with high hydrogen capacity. "Liquid phase" with "heterocyclic CBN compound" is a correct direction, and the methodology adopted in this project is adequate.
- The team has an excellent combination of expertise on synthesis, computer simulation, and characterization. It is doubtful that other teams could do better than this team. The team explored several types of compounds, but Mother Nature has not been too kind in revealing any one that would be a home-run success for hydrogen storage. It is suggested that the team focus on exploring new compounds instead of spending too much effort on existing compounds that are unlikely to meet the targets. It is critical to find compounds or mixtures that will stay liquid before and after desorption. The team is encouraged to venture outside the composition space that was specified in the original proposal.
- The project combines the expertise of participating partners to develop novel hydrogen storage materials that have potential to meet the 2017 DOE target. The approach is adequate to achieve the project objectives.
- It is an interesting idea to modify ammonia borane (AB) by including it into an organic ring system, which would be more stable and low-melting. However, every carbon atom added reduces the overall capacity.

Also, the formation of an aromatic system during dehydrogenation makes the reverse hydrogenation reaction more difficult.

- There are only two compounds addressed in this project (compounds H and J) that have any chance at all of meeting the 2017 DOE system gravimetric target. To do so, compound H requires reversibly removing all 10 hydrogens in the structure without destroying the rest of the structure along the way. The possibility of doing this is expressed in a way that leaves little hope of success. Considering where the Hydrogen Storage Engineering Center of Excellence (HSECoE) is today and where it is headed in the coming year, it seems hard to justify continued effort on work of this type.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The team made steady progress toward the overall project goal. It developed fuel blends of compound B + AB, which has a hydrogen storage capacity of 6 wt.%.
- The synthetic capability to gain entry into these difficult to prepare CBN heterocycles continues to impress, as does the ability to characterize the molecular products/outcomes of dehydrogenation reactions. These products themselves are new and novel. A rather large library of CBN heterocycles has now been prepared and assessed, with a focus on the most promising compounds. With this larger number of compounds available and the assessments that have been carried out to date, a valuable number of lessons have been learned, and it is a nice accomplishment to summarize them for the community as the team did in its presentation. Exploring blends of compounds to achieve a liquid “fuel” and “spent fuel” is another valuable accomplishment, and it highlights a potentially useful pathway to providing liquid fuels. Some additional attention to regeneration of these mixtures may be in order. Identifying a heterogeneous catalyst that acts on both the boron-nitrogen (BN) fragment and the carbon-carbon (CC) backbone of a CBN heterocycle is a nice and necessary accomplishment, in that it provides proof of concept that hydrogen may be released from the entire molecular backbone, not just the BN unit.
- A rich group of liquid-phase heterocyclic CBN compounds has been carefully investigated, along with combinations with some other BN hydrides. The properties of several of them have been made clear. The results have shown high potential toward the goal of reversible hydrogen materials with high hydrogen capacities.
- The project has very interesting fundamental results. Researchers may also want to look into phase diagrams of different mixtures, which could help to create liquid or low-melting materials.
- The chemistry is interesting and the go/no-go decision making is reasonable. It seems a waste of effort to study materials that have low hydrogen content. Any material with less than 10 wt.% hydrogen is probably going to be useless in terms of meeting DOE onboard hydrogen storage targets. The argument that something important might be learned is hard to appreciate at a time when the HSECoE is in the final stages of feasibility demonstrations.
- The team did an excellent job in performing, testing, and narrowing down the compounds for further study. Unfortunately, what is left behind is not as attractive to a real hydrogen storage system.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- There has been good collaboration; partners participate and are well-coordinated.
- The team maintains quarterly conference calls, complemented by more frequent and necessary two-way exchange of information. The team also collaborates with researchers/institutions outside the team.
- The roles for each member of the project team and the outside collaborators are clearly spelled out. The connection/interface with the HSECoE is mentioned.
- As in the past, there is a record of excellent collaboration among the team members. This is perhaps best exemplified by the fact that the (excellent) presentation was given by the Pacific Northwest National Laboratory partner, as the PI was unavailable. The communication within the small project is clearly top notch.

- The project has a well-balanced team with experts from both experimental/theoretical fields and university/laboratory/industry. The team has shown efficient performances to overcome difficult barriers with effective cooperation. It has become clear that reversibility might be achieved by the design of such heterocyclic CBN molecules and efforts made in this project have shown hopeful enhancement of hydrogen capacity.
- Team collaboration seems very effective.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The project is critical to the Hydrogen Storage sub-program and has potential to significantly advance progress toward DOE research, development, and demonstration goals and objectives.
- The project tested liquid fuel blends and potential reversible materials while also exploring high-capacity liquid materials. It is highly relevant to achieving the DOE hydrogen storage targets.
- This project continues to provide valuable information regarding the viability of molecular systems as hydrogen carriers for hydrogen storage applications. The range of thermodynamics, kinetics, and other physicochemical properties that are spanned by these types of compounds is thought provoking. This approach, while it may appear to be "too complex" to some, provides DOE with valuable insight into what might be possible with continued research into these molecular systems that have such a wide range of hydrogen release properties and regenerability.
- A completely new and correct direction with very high potential for future breakthrough. More efforts by more groups on a longer time scale will definitely increase the possibility of reaching the DOE targets.
- The work is directly related to DOE hydrogen storage targets.
- Without convincing evidence that there is a CBN compound out there with real potential to deliver >10 wt.% hydrogen reversibly, it is hard to get excited about the work going on in this project. If it were purely a science program, the science would be considered interesting by most. But if the goal is to identify a material the HSECoE can really use to meet its desired targets, there does not seem to be hope for success in a timely manner.

Question 5: Proposed future work

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

- There are good future work plans. Phase diagram studies may help to answer the question on slide 25, "Can compound J be a liquid at room temperature?" and similar ones.
- The project lists CBN cyclohexanes, room temperature solid materials, in the future work. These compounds can be synthesized and will be characterized. These efforts are in line with the project scope.
- Future work should focus on compounds H and J. Working in the molten state with compound J seems like the only real chance for success. Diluting compound H with a liquid carrier will of course lower the effective reversible hydrogen content. Work on compound G is not valuable for vehicular applications.
- While the regeneration of some of these compounds with lithium aluminum hydride ($LiAlH_4$) is chemically facile, it is not likely to be practical, as the recycle of $LiAlH_4$ is energetically problematic. As was pointed out during the question and answer session, were one to use $LiAlH_4$, one might as well just put it onboard and hydrolyze it rather than go to the effort of preparing CBNs, etc. Thus, the future work should acknowledge that for non-reversible systems regeneration must proceed through plausible reaction pathways that can achieve the DOE off-board efficiency metrics.
- The proposed future work is adequate.
- The proposed future work is very reasonable, considering the short remaining duration of the project.

Project strengths:

- This is an excellent team that integrates superb synthesis expertise as well as computational expertise and characterization. Several new compounds were synthesized and characterized. These compounds are not easy to synthesize or characterize. The team did a superb job in delivering the promised results. The understanding obtained from the synthesis and characterization will be very useful for future explorations.
- The project is novel and innovative with a high potential to lead to breakthrough.
- The project has a strong research team and yielded interesting fundamental results.
- The project uses the expertise of the participants to address the project objectives and DOE targets. The project combines the strengths of each participating institutions.
- The project has scientists who are knowledgeable about the relevant chemistry. The project's down-selected materials are well considered based on the results to date.
- The project has an excellent, productive, and experienced team working on a high-risk, potentially high-reward project. The project continues to impress with its synthetic prowess, computational integration, and good knowledge on what the key barriers are to a chemical hydrogen storage (CHS) material, and how those interact with potential CHS engineered systems (which is not the focus of the project).

Project weaknesses:

- Although this is a DOE Office of Energy Efficiency and Renewable Energy (EERE) project and physical understanding of the materials behavior is not at the top of the agenda, understanding the mechanism is critical in improving overall efficiency. For example, the mechanistic understanding would be truly useful to understand the kinetic measurement and improved system performance.
- There is a lack of chemical industry partner(s). The goal to "develop novel chemical hydrogen storage materials that… meet the 2017 DOE targets for vehicular applications with focus on three classes of materials" may be very difficult to achieve with the studied materials.
- The project team is working on a class of materials that others have investigated with little success.
- There is limited time left. A successive project is needed.
- Though it is unclear whether the team has looked into the composition space broadly enough, continued work in this space will likely be unfruitful; instead, new compounds/composition space should be explored (even though such an approach may not fit into the existing deliverables).

Recommendations for additions/deletions to project scope:

- Focus on compounds H and J in the coming year.
- With the little time remaining, the team may wish to focus on those materials that are directly able to be regenerated onboard with hydrogen. Even a low capacity of a few weight percent that is reversible may provide an entry into low pressure, portable power applications.

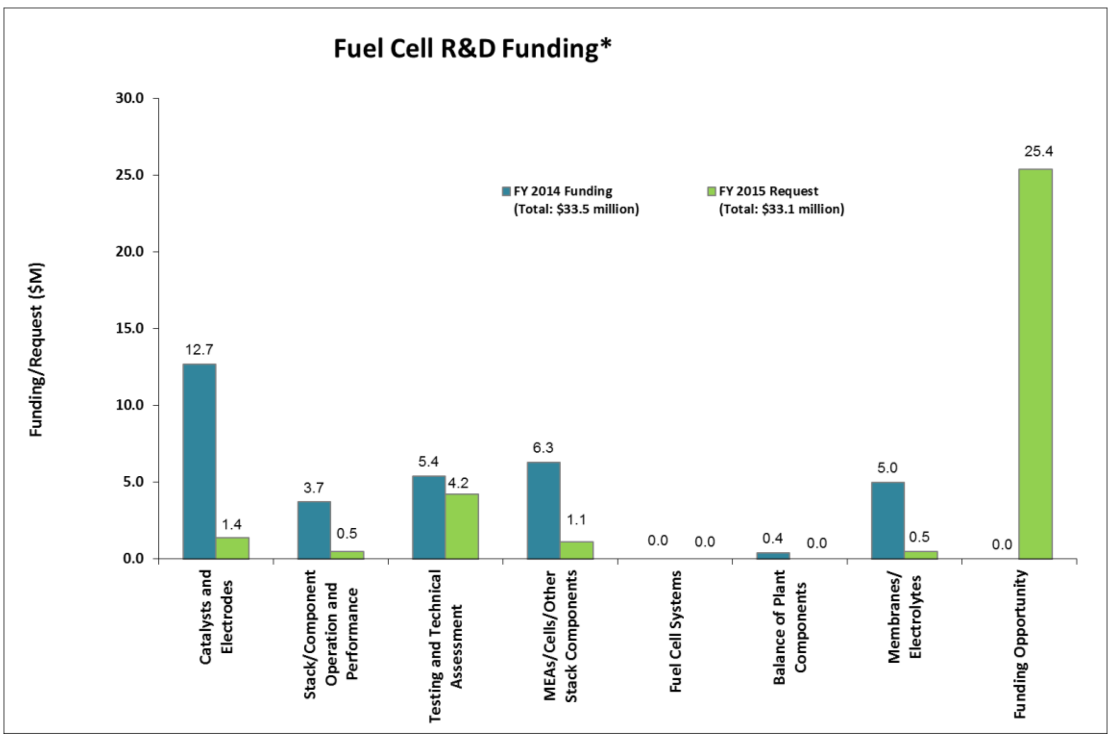
2014 — Fuel Cells
Summary of Annual Merit Review of the Fuel Cells Sub-Program

Summary of Reviewer Comments on the Fuel Cells Sub-Program:

Reviewers felt that there was a good balance between near-, mid-, and long-term research and development (R&D) in the Fuel Cells sub-program. Reviewers agreed that cost and durability are the major technical challenges and praised efforts taken by the Fuel Cells sub-program to address these issues. A strength of the sub-program, as noted by the reviewers, is its well-structured, focused, and well-managed projects. Reviewers also stated that the project teams are strong and feature excellent collaboration between academia, industry, and the national laboratories. Input from the community—for example, via the U.S. Council for Automotive Research—was also identified as a strength in focusing R&D on the most relevant challenges. Reviewers noted that many of the projects in the current portfolio are ending and expressed optimism that the next funding opportunity announcement will address current gaps in the portfolio. Specifically, reviewers identified understanding degradation mechanisms, developing new analytic tools and increasing access to existing tools, and accelerating integration of recently developed materials into cells and stacks as areas that need to be addressed. Some reviewers expressed a desire to see more effort to develop a domestic supply base for critical components and achieve better transfer of technology from the laboratories to industry.

Fuel Cells Funding:

The sub-program received $33.5 million in fiscal year (FY) 2014. The request for FY 2015 is $33.1 million. The focus of the sub-program is on reducing fuel cell costs and improving durability. Efforts include approaches that will achieve increased activity and utilization of platinum group metal (PGM) and PGM-alloy catalysts, as well as non-PGM catalysts for long-term applications; ion exchange membranes with enhanced performance and stability at reduced cost; improved integration of catalysts and membranes into membrane electrode assemblies (MEAs); and a better understanding of degradation mechanisms and mass transport. Two new projects were funded in FY 2013 and initiated in FY 2014, and one new project was funded in FY 2014 from the most recent solicitation.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area and the relative merit and applicability of projects competitively selected through planned funding opportunity announcements.

Majority of Reviewer Comments and Recommendations:

At this year's review, 42 projects funded by the Fuel Cells sub-program were presented, and 27 were reviewed. Projects were reviewed by between 7 and 12 reviewers, with a median of 10 experts reviewing each project. Reviewer scores for these projects ranged from 2.6 to 3.6, with an average score of 3.2. This year's highest score of 3.6 and average score of 3.2 were similar to last year's highest and average scores of 3.6 and 3.1, respectively. The lowest score of 2.6 for all projects reviewed in 2014 was a modest improvement over 2012's low score of 2.4.

Catalysts and Electrodes: The scores for the nine catalyst projects ranged from 2.6 to 3.6, with an average of 3.1. Reviewers praised the highest-rated project for making good progress toward meeting U.S. Department of Energy (DOE) targets, and for applying a solid understanding of surface science to catalyst development. For the lowest-scoring project, development of durable catalyst supports, reviewers felt that the results for the selected metal oxide supports do not yet meet the performance requirements for use in fuel cells, although the reviewers liked the approach.

Stacks and Component Operation and Performance: Four projects were reviewed, receiving scores between 2.9 and 3.4, with an average score of 3.2. Reviewers praised the highest-rated project's approach to studying durability for its strong combination of characterization, diagnostics, and modeling. Reviewers noted that, although the approach is thorough, its focus is limited and would benefit from an expanded material set and a deeper parametric study. Reviewers felt that the lowest-scoring project's approach was generally good and addressed the known durability issues in polymer electrolyte membrane (PEM) fuel cells; however, they expressed concern about an overlap with efforts being pursued by automotive and fuel cell stack original equipment manufacturers (OEMs) and a lack of automotive OEM collaboration.

Testing and Technical Assessment: Seven projects were reviewed and received scores between 3.1 and 3.6, with an average score of 3.3. According to reviewers, the analysis and testing projects take a good approach and produce highly relevant analyses for the fuel cell community. In some cases, reviewers questioned assumptions being made and recommended interaction with OEMs to refine assumptions where possible.

MEAs, Cells, and Other Stack Components: Three projects were reviewed in this area, receiving scores of 3.0, 3.1, and 3.2. Reviewers considered the projects to have well-coordinated teams that achieved good technical progress. Reviewers noted that current approaches should be aimed at addressing operational robustness in practical applications for thin-film catalysts.

Fuel Cell Systems: The one project reviewed this year received a score of 3.3. According to reviewers, good technical progress has been made toward simplifying the system design and reducing part count and cost. Some reviewers expressed concern that there was not a business case for hydrogen from renewable biofuel. They also noted that durability still needs to be demonstrated.

Balance-of-Plant Components: One project, development of a compressor/expander unit, was reviewed. It received a score of 3.1. Reviewers regarded the focus of this project as very important for successfully launching fuel cell electric vehicles. While reviewers characterized the upstream (i.e., sub-system and system-level) partnerships as strong, they suggested that the project could benefit from a motor/controller partner, noting that these components seem to be the primary barriers to meeting the cost target. Reviewers lauded the project for its progress in modeling, improvement of designs, and hardware testing, but they saw overall progress as relatively slow.

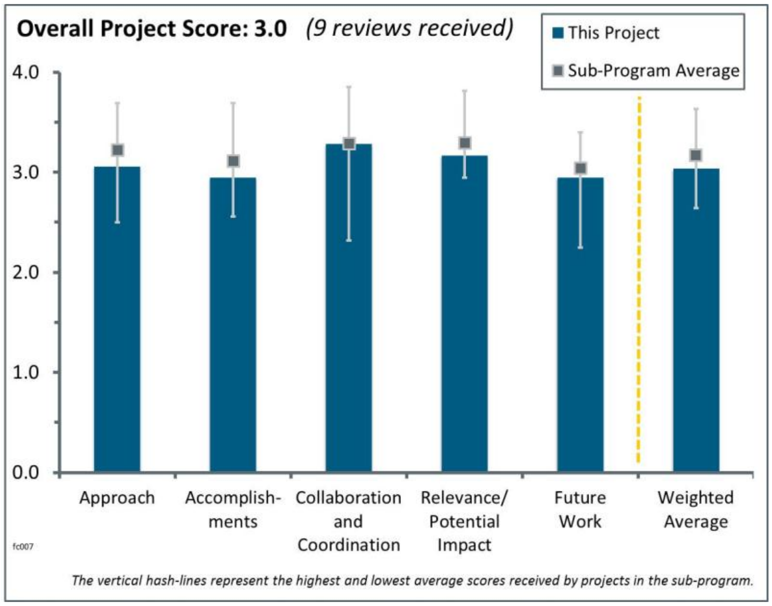
Membranes/Electrolytes: The two membrane projects reviewed received scores of 2.9 and 3.2. Reviewers indicated that the highest-rated project has made good progress toward meeting DOE targets, using sound approaches to combine high conductivity, low swelling, and mechanical stability. Reviewers expressed concern about the scalability and manufacturing cost for both projects.

Project # FC-007: Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes

Bryan Pivovar; National Renewable Energy Laboratory

Brief Summary of Project:

In the current review period, this project pursued synthesis of novel extended thin-film electrocatalyst structures (ETFECSs) and incorporated ETFECSs in membrane electrode assemblies (MEAs) to meet the U.S. Department of Energy (DOE) targets for fuel cell cost, performance, and durability. The approach includes developing durable, high mass activity, extended-surface Pt catalysts and optimizing MEA performance/durability for these materials.



Question 1: Approach to performing the work

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

- The project focuses on the key barriers of activity and durability. During the last year of the project, there has been much better balance between synthesis, characterization, and making electrode assemblies. The team has focused on the galvanically displaced samples and put more effort into MEA testing. The initially unique approach to incorporate extended thin films into thick electrode architectures has shifted to less controlled systems. The team has acknowledged that the initial goals were too difficult to achieve and that, if obtained, the materials would have lower electrochemical surface area (ECSA). Even though this is disappointing, the achieved high activities of less controlled systems deserve high marks.
- This has been an ambitious project that aims to make a step-change in electrocatalyst design from current commercial materials. The project aims to directly address the DOE targets relating to electrocatalyst activity and stability. Arguably, the intent to take materials from inception to developed MEAs was overly ambitious, but this has proven to be a very solid project that tackles some very difficult issues.
- The project is directly addressing the barriers of catalyst performance, durability, and cost. The technical approach is novel in intentionally generating high-activity-alloy extended-surface catalysts rather than alloy nanoparticles, and it appears to be well executed and focused.
- The approach is focused on meeting the DOE targets to decrease Pt cost by decreasing its loading and increasing durability.
- This approach is based on existing systems that show very high activities and durability properties that should be able to overcome the barriers for automotive applications.
- The approach of this project is unique and tackles one of the key issues concerning catalytic activity and durability for the electrodes. The idea is innovative, and the impact would be significant if successful.
- This project has evolved over the years. Initially, there was great hope that by studying galvanic displacement on extended-surface substrates, nice continuous layers of Pt would be deposited. Having found difficulty in getting this to actually happen, a shift in focus to less crystallographically perfect Co and Ni core wires was understandably made, and the focus now has been to get these into operational cells before the program ends. The team should be congratulated on actually making devices, but it was done in an ad hoc manner. As a result, not much fundamental information about these materials was learned regarding what strategies to use to design next-generation catalysts other than to keep the surface area up and make them as active as possible. It would have been useful to have more information about the microstructure of the very high-activity catalysts. Their surface area is too high to be a transition metal alloy; for example, Pt/Ni catalysts usually have surface areas around 40–50 m^2/g. Unresolved is whether

the high activity is due to something intrinsic in the material or whether there is better accessibility of reactants. It would also have been useful, as an approach, to focus on the microstructure of how the catalyst is dispersed in the MEA catalyst layer; the microscopy resources of this program seem to be underutilized in this regard.

- The approach certainly has its merits, because it focuses on increasing available active sites on the catalyst. However, it is not clear how this would help reduce degradation, which is currently the main problem with catalysts. The degradation of Pt-based catalysts is well known, as are the reasons. The project aimed at increasing the utilization of available catalysts; however, it was not clear on how to solve the dissolution problem at high potentials and subsequent deposition at low potentials, leading to agglomeration of the particles and, consequently, performance loss. The data shown with the potential cycling test do show degradation within limits; however, degradation tests have to be more complete, because voltage cycling only does not provide certainty that the catalyst will be durable in an MEA under automotive conditions (e.g., including start-up/shutdown cycles).
- The approach to develop new catalyst materials is excellent. However, the schedule seems too tight regarding achieving performance/oxygen reduction reaction (ORR) targets in an MEA because there are a lot of barriers in the catalyst layer fabrication process. If the team keeps the schedule and target, it would be better to consider simultaneously what the critical challenges are for the selected materials to be an electrode and a scenario of how to overcome them.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- Remarkable progress has been made; materials have been developed that achieve four times the DOE 2020 mass activity target.
- The project team is to be commended for the depth with which it has looked to understand the materials that are being synthesized and to rationalize the activity and durability observed. This is apparent in the study of the role of the oxide within the Ni variant. It is, however, less than clear why such differences are seen in the Ni and Co systems. Although exceptionally high mass activities have been reported for some materials in the project, as determined from the summary "MEA and RDE data" slide, only a small number of the catalysts taken forward for MEA testing were seen to have met this target in rotating disk electrode (RDE) measurements. As is acknowledged, significant losses in moving to the MEA are observed; the reasons for this should be understood before spending extensive time on high current density operation. Good developments to higher-surface-area electrocatalysts have been made; this is considered integral to enabling tolerance to (even low levels of) the impurities required in real-world operation and, when MEA optimization is tried in earnest, good high current density performance.
- The observed increase in catalyst activity is certainly a merit of this project. It does show a potential route to increase beginning-of-life activity. The study on the oxide layer does provide good insights into the effect of oxidation of the catalyst. However, this was not the best area on which to focus the resources of last year. The effects of oxide layers on catalysts are relatively well known in the literature. They do provide protection for the nonoxidized part of the catalyst, but they are temporary layers that get dissolved again during low potentials. Over time, the nonoxidized part of the catalyst will be oxidized. The efforts would be better redirected if there were studies on how to prevent dissolved Pt particles from becoming detached from the ionomer and support as they are reduced under low potentials.
- The project team is commended on making excellent progress on developing high-activity catalysts, as measured by RDE. One critical point is that progress toward developing high-activity MEA electrodes is not nearly as advanced and remains a key barrier. In MEA, mass activities are lower than Pt/HSAC. The challenges of developing fuel cell electrodes with novel electrocatalysts are severe, and it appears this has not been as strong of a project focus as warranted. It is unclear if the team has a clear pathway toward resolving this issue. Assuming the electrode development issue can be resolved, another MEA integration challenge that must be addressed is the minimization of transition metal leaching in MEA, which negatively impacts rated power performance. Electrocatalyst durability of the highest-activity next-generation Pt nanowires (NGPtNW) against potential cycling must be improved. While the final mass activity is high, electrode loadings in MEA are determined to meet a minimum performance level at end of

life, and many of the higher-activity materials lost substantial fractions of their initial activity. Assuming the electrode development issue can be resolved, it is highly recommended that durability is evaluated in MEA form.
- The investigators made great progress in achieving high surface areas and specific activity catalysts and demonstrated high durability. They performed MEA optimization and achieved reasonable performance. The role of high surface area carbon (HSC) in the best-performing MEA is not clear. Its use is in contradiction with the initial carbon-free approach. The problem with metal leaching is the biggest obstacle at this point.
- Very good progress has been made toward increasing extended-surface catalysts' surface area, specific activities, and mass activities. Next-generation Pt nanowires look very promising based on RDE activities and fuel cell durability testing. It is still unclear if it is real to make both durable and highly active Pt-coated Ni nanowires. Ni nanowires are not stable when partially coated with Pt, and they are not very durable while fully coated with Pt.
- The rate of progress seems to be increasing as the project members have gained experience with these kinds of materials. It is disappointing that it has taken so long for them to translate "RDE expectations" into MEA performances. This is true, given the high levels of expertise of the personnel involved. This should imply that more understanding of electrode formation is needed, with less focus on screening lots of different ETFECS particle types and materials. Until the investigators understand how to make effective electrode structures that capture the advantages of the high-aspect-ratio thin-film ETFECS particles for ORR, they may not be able to correctly evaluate the latter. They should benchmark their MEA performances and durability results against the 3M-reported nanostructured thin film (NSTF) results rather than Pt/C, because the 3M results will likely have much better membrane integration and reveal whether they need to improve basic activity or electrode construction.
- It is commendable that the team made some highly active catalysts and translated them to MEAs, although their fuel cell performance was a bit disappointing. The team, however, did not learn much about the mechanism of high activity, nor the role of oxides.
- The team is continuously making progress, demonstrating that the 2020 targets can be achieved. However, it is still not entirely clear whether the issues associated with the oxide layer formed can be successfully addressed. This has been one of the concerns, and yet it remains uncertain.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The team is very strong covering synthesis; extensive characterization; testing; and finally, with addition of General Motors, consultation on fabrications of MEAs, which was the weakest point.
- Under this topic, the interaction with the partners seemed to have worked well. It was impressive that so many partners were involved in this project and that they managed to contribute in relevant ways to the successful areas of the project.
- This project features a well-coordinated effort between the National Renewable Energy Laboratory (NREL), Oak Ridge National Laboratory, Colorado School of Mines, and the University of Delaware.
- The researchers have a good mix of academics, original equipment manufacturers, and experienced suppliers.
- The project features very appropriate collaboration with partners, and each role is well defined.
- The wealth of ex situ analysis data clearly shows the roles of some of the other project partners, which are uniting to provide a cohesive study of this new material set. While the industrial partners will clearly not disclose too much information, more in-depth commentary on the feasibility of material scale-up would be helpful.
- Based on the publications, it looked like most of the work was performed at NREL and the input of the subcontractors was sub-par. The only publication with Yan (Delaware), for instance, was on Pt-coated copper nanowire in base, so it is only tangentially related to the project. In addition, the team did not address what it is doing with regard to the oxide layer.
- Collaborators have the appropriate skills to aid in the project mission. This year, it is not perfectly clear exactly which accomplishments could be attributed to a particular collaborator.

- The team's efforts are generally well orchestrated, but the role of individual team members is somewhat unclear.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- The project is well aligned with Hydrogen and Fuel Cells Program objectives. The potential impact can be compromised by durability issues related to leaching of transition metals in an acidic polymer electrolyte membrane fuel cell environment.
- The project has advanced development of durable and active low-Pt-content catalysts. The accomplishments of achieving very high surface areas and high specific activities are advancing the overall progress in this development. There was incremental improvement in MEA fabrication and performance. The biggest problem is contamination with transition metal to improve durability.
- This project has a good shot at developing alternative catalyst approaches to the 3M NSTF technology that still relies on extended-surface-area catalyst concepts. Mass activities as measured in RDEs are promising, so the key areas that should receive focus include limitations of electrode fabrication with the team's catalyst approaches and whether they are amenable to high-volume manufacturability.
- The electrocatalytic process is one of the critical controlling factors for the performance of fuel cells. The project aims to develop a metal catalyst with unique structure that may improve catalytic activity as well as durability.
- The catalyst development portion of the project appears to be very successful and impactful toward generating high-activity ORR catalysts, a key commercialization barrier. If the high-activity catalysts can be integrated into fuel cell electrodes with similar activity, improved durability, and minimum base metal leaching, the technology could be revolutionary.
- The project could have a very big impact on further Pt reduction in the future. It achieved four times the mass activity by RDE compared to the DOE 2020 target.
- The main argument for relevance is scalability and the ease of using galvanic displacement. However, like any chemical reaction synthesis, it is not clear that scaling is easy. Indeed, it would have been useful to see how well a simple scale-up would do. It appears that the chemical vapor deposition (CVD)-grown nanowire templates are relatively "lower cost," especially compared to directly sputtering, or reducing similarly priced salts. Also, these materials have such a large amount of transition metal left that it is not obvious whether they can ever be removed. Passivating them behind or in an oxide will only last for so long, especially if the cell is cycled so that the cathode sees transient-reducing conditions.
- This project sought to directly address the DOE's key electrocatalyst targets and provide some real insights into some alternative ORR materials. As an extremely challenging project, it has not always achieved success, but this is exactly the type of fundamental materials project that should be supported. As the project concludes, it appears that its potential impact is one of learning and understanding, rather than of developing materials that will find a direct route to scale-up and application. This is in no way to diminish the outcomes of the project.
- The project addresses one of the key barriers, catalyst activity, which is relevant to the reduction of total Pt loading and further cost reduction. This has been obtained before with similar approaches, such as 3M NSTF, which does have performance problems at lower operating temperatures. It was not clear whether performance at wet conditions was also checked by the investigators here. Overall, this project is relevant in identifying new manufacturing methods to create structures that increase Pt activity, but this would be relevant in a future where basic durability issues are solved first.

Question 5: Proposed future work

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

- The proposed future work is logical based on the current status. Optimization of fuel cell performance and evaluation of in-MEA durability are key and should be the primary focus of the project remainder.

- Although it is clear that very significant MEA optimization work is required to understand whether these catalysts are capable of giving the required operating performance, it is less than clear whether the project team and remaining project time will enable much meaningful progress to be made in this area. However, it is recommended that time and energy be spent on trying to understand why such a considerable drop-off in performance is observed in the move from RDE to MEA testing. Questions on material stability also remain. Owing to the use of transition metals capable of existing in both the metallic and oxide states, and also capable of leaching, it is suggested that durability studies utilizing oxygen, rather than nitrogen, would be prudent.
- The proposed future work states “Novel synthesis is focused on durability concerns of high performance transition metal containing materials.” This is the right direction; however, there has been no mention on what the approach would be to solve this problem. The transition metal materials used in the project so far (Co and Ni) have proved to have similar problems as Pt, with dissolution at high potentials. It is not clear how the synthesis will change this.
- The proposed future work is focused on overcoming the durability and stability issues identified last year. Revealing the role of graphitized carbon nanofibers in the structure of RDE films and fuel cell catalyst layers should be one of the priorities for the future work. While using an unsupported catalyst was one of the benefits of the proposed approach, incorporating carbon into the structure of the catalyst layer brings back problems with carbon corrosion and thickening the catalyst layer, among other issues, especially considering the high wt.% of carbon fibers used in RDE films, as reported in the 2014 American Chemical Society catalysis paper.
- The future work is correctly focused on the optimization of fuel cell performance, whereby good electrode fabrication will be the goal. But its downside is that it is still limited to making inks and dispersions. The researchers need to think outside the box and understand how to take full advantage of the high aspect ratio of the ETFECS particles and ways of packing them so that they realize the full mass activity potential. Studies of the MEA electrode losses to separate the mass transfer, ohmic, and kinetic losses are very good and should be done in-depth with dozens and dozens of MEAs using one type of catalyst and multiple controlled and varied fabrication/process variables. When the researchers progressively benchmark their best of class, they can do periodic durability testing.
- The future work is addressing all the relevant issues and challenges. However, with only a few months left, only limited progress can be made toward addressing the role and durability of transition metals. It is not exactly clear how MEAs will be optimized. The role of HSC must be addressed.
- Given the limited time remaining on this project, it makes sense to try to mitigate the effect of residual transition metals on performance.
- The proposed work identifies the remaining issues and suggests plans to address them. However, the proposed tasks are not entirely new, and it is not entirely clear whether the current issues can be effectively tackled by the work proposed.
- So far the progress is excellent in terms of material development. If the scope includes MEA performance, more detailed strategy and plans are needed.

Project strengths:

- The project features a strong team that has demonstrated an excellent capability to screen ETFECS electrocatalysts and achieve very high activity. The team has invested a lot into fundamental understanding of extended-surface area Pt catalysts, employing significant analytical characterization. The team has made significant progress toward fabricating electrode structures.
- The project combines advanced microscopic characterization techniques with well-established RDE and fuel cell characterization. Combined with synthetic intuition, it helps to make fast progress toward synthesis of catalysts with desired activities.
- A strength of the project is the galvanic displacement method that yielded higher beginning-of-life-activity catalysts. The down-selected synthesis method does have advantages such as a lower temperature for the synthesis, compared to alternatives.
- The team has developed novel approaches to generate catalysts with extraordinarily high activities in RDE. The project direction toward catalyst development has been logical and the team has proceeded with appropriate focus.

- Strengths of the project include the principal investigator (PI), collaborators, and experience of the group as a whole.
- The ideas of the project are unique and innovative. Progress is continuously being made, although it may not be fast enough.
- Strengths of the project include the potential impact of Pt reduction, measurement database and reproducibility, and management by the PI.
- This project has featured a very sound project scope and capable project partners, and it has been well coordinated.

Project weaknesses:

- The objective to make thick electrocatalyst layers to try and overcome some of the known water management issues with thin-film catalysts has been clear from the outset. While the reintroduction of carbons into the layer serves to achieve this, some of the known stability issues again come to the forefront. Clearly, the degradation of the electrocatalyst and the support medium are now separated, but carbon corrosion will lead to the same mass transport losses as occur in conventional layers. There is little discussion on the details of the materials that are selected to fulfill this role.
- The routes toward optimization of MEAs are not clearly defined. The use of HSC in a “carbon-free” approach is not expected.
- The project lacks a well-defined approach to increasing the durability of Pt-Ni nanowires. Using oxide films to prevent leaching of the transition metal does not seem like a promising solution because of (a) potential problems with conductivity and (b) the solubility of nickel oxide in an acidic environment.
- A weakness of this project is that the researchers are trying to force high-aspect-ratio, NSTF-type particles to behave in an electrode like a dispersed Pt/C catalyst.
- The progress on optimization of the catalytic activity is not very fast. The issues associated with oxide formation and its role are not yet addressed, and the plans to tackle these issues are not entirely clear.
- The MEA preparation strategy is an area of weakness.
- A research focus on the oxide layer is not key to solving durability problems.
- Fuel cell electrode performance is poor, and insufficient focus has been expended in this effort.

Recommendations for additions/deletions to project scope:

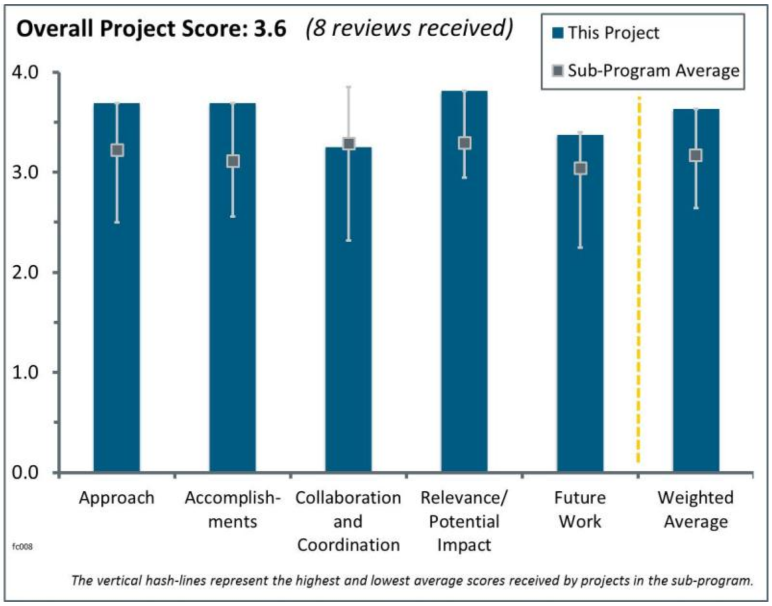
- The researchers should delete the oxide layer investigation. Performance of transition metal has been extensively researched by academia and industry. Failure modes, such as leaching and temporary sulphonic group exchange in the ionomer, have been extensively studied. The researchers should include start-up/shutdown durability testing (as based on relevant accelerated stress tests [ASTs]), and other automotive-based ASTs.
- This project should focus on material development. (The reviewer understands that 0.44 A/mg Pt as MEA is a stretch target.) For MEA optimization, more time is needed to solve a lot of further challenges. That could be included among the activities for the next DOE project (funding opportunity announcement).
- The researchers should delete some of the new catalyst particle synthesis work and replace it with studies to understand how to make optimized electrodes with the best catalyst particles they have now. Until this is done, they will not realize the maximum potential of any of their catalyst particle approaches.
- The effect of morphology on performance and leaching of transition metals is of great importance and must be studied.
- More significant progress should be made to address the remaining issues regarding the durability of the catalyst, and the results should be demonstrated.
- It would be beneficial if this project had a modest amount of additional time to specifically focus on electrode development to determine if there is a path forward.
- Not applicable—this project is scheduled to end in September 2014.

Project # FC-008: Nanosegregated Cathode Catalysts with Ultra-Low Pt Loading

Vojislav Stamenkovic; Argonne National Laboratory

Brief Summary of Project:

The main focus of this project is the development of highly efficient and durable multimetallic Pt-alloy nanosegregated catalysts for the oxygen reduction reaction (ORR) with ultra-low Pt content. Argonne National Laboratory (ANL) will establish a methodology capable of determining nanosegregated Pt-skin surfaces for different classes of electrocatalysts, as well as establish protocols for scaling up production of catalyst materials.



Question 1: Approach to performing the work

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

- The synthetic results, characterization effort, and achievements in specific activity and mass activity are excellent. The contributions into the fundamental structure-performance of catalyst structures are of great importance to the whole fuel cell community.
- The project has a strong focus on achieving targets well above the stated DOE technical targets, with the goal of achieving thin film Pt_3Ni activity. The systematic approach is focused on developing a knowledge base on the atomic structure relationships with catalyst activity and durability. The focus on catalyst targets, without diluting efforts of membrane electrode assembly (MEA) integration, is appropriate for catalyst work at this level.
- The Stamenkovic/Markovic group is truly an asset to fuel cell development. The group's research approach is effective, as shown by the advances it has made in exceeding DOE fuel cell technical targets. The project team is strong and well led.
- The materials-by-design approach to synthesize, understand, and develop the multimetallic nanoparticles in different structures is excellent, and the results prove this.
- The team has made significant progress toward the DOE goal. The experimental approach is reasonable and the results of the data analysis look convincing.
- Prior to this project, thin films of Pt_3Ni(111) were found to have 90 times greater specific activity than Pt supported on carbon. This project has been an ongoing attempt to translate this finding into a practical nanoparticle catalyst that would be suitable for high-volume production. The project has expanded its scope in a productive manner. Rather than fixate on obtaining a high-activity PtNi nanoparticle, the team has also explored alternative morphologies (Pt alloy nanowires and mesostructured catalysts), the use of gold inner layers for stabilization, and PtNi nanoframes. The team has attempted in the past to scale up high-activity materials into quantities sufficient for cell testing; the same approach will be taken with the nanoframe catalysts.
- The researchers' approach is to try to get the Pt_3Ni(111) surface into nanoparticulate catalysts using whatever options they find. In truth, the approach has been somewhat serendipitous and a bit ad hoc—last year they focused on annealed nanostructured thin film (NSTF), and this year they focused on the nanoframes. The results have been impressive but limited to laboratory scale. A recent focus on stability has been commendable, and the addition of ionic liquids into their materials is very interesting. One particularly good aspect of their approach has been careful microscopy to really understand the microstructure of the catalysts they are making.
- Focus should be on the crystalline phase of the surface to enhance the specific activity.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.7** for its accomplishments and progress.

- The project has made outstanding progress toward DOE goals. All technical targets have been met or exceeded. During the last stage of the project, high stability (exceeding that of Pt/C catalysts) was achieved.
- The project team has made strong progress and exceeded the DOE fuel cell goals for catalyst mass and specific activity. The team has met its performance goals with several Pt-alloy nanocatalysts and has shown good progress with its core-shell materials. The work in Pt-Ni nanoframes is very interesting. The Pt-alloy nanocatalysts have shown durability and are ready to be evaluated in MEAs.
- This project tackles one of the key issues that should be addressed to develop an electrode catalyst containing less Pt based on synthesis of a new type of macrostructure. The concept of the nanoframed catalyst is innovative, and the results of the activity test look promising.
- ANL has demonstrated outstanding accomplishments and progress. The focused approach is paying dividends with progressive improvements in catalyst activity through creation of new structures. In addition, excellent stabilization of particles has been achieved through the use of an Au interlayer. The nanoframe structures with the ionic liquid are an exciting development and provide a breakthrough in catalyst activity.
- This project features several outstanding outcomes, including the nanoframe catalyst.
- While cell testing is still awaited, this project has delivered the most important result for the entire Fuel Cell sub-program section of the 2014 DOE Hydrogen and Fuel Cells Program Annual Merit Review: a manufacturable nanoparticle catalyst that demonstrates 35-times activity improvement over Pt/C based on rotating disk measurements. No change in activity was found for nanoframe catalysts after 10,000 0.6–1.0 V cycles. Again, fuel cell testing is needed, as well as 30,000 cycles, but this is a good start. A small amount of Au was shown to retain mass activity of the PtNi nanoparticle after 10,000 0.6–1.1 V cycles. This may prove to be important to address eventual failure modes in PtNi catalysts. The project features an excellent use of characterization to confirm the existence of the Pt nanosegregation at the surface of the nanoparticles.
- Accomplishments in RDE are superlative, but not having MEA performance is discouraging. Mesostructured thin film (MSTF) has shown some promise in MEA, but many of the designs/catalysts systems are not tested in MEA, or at least not reported. Efforts should be made to realize the great RDE activities in MEA—that would be a breakthrough. A 30-times activity improvement with nanoframe over Pt/C is excellent, but the big question is how much one can expect in MEA. Even a 3–5-times improvement would be great in MEA. At the end, these catalysts need to work in MEAs. Excellent durability has been demonstrated under 0.6–1 V cycling, which is very promising.
- The development of new, highly active catalysts is impressive, and the characterization and modeling milestones will certainly be met. Less obvious is the progress toward the goals of integration into MEAs, much less scale up. These issues were marked as 70% and 55% complete, but the basis for these numbers is unclear.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The project features excellent collaboration between the partners. This is very evident from the work accomplished, publications, and patents.
- There has been good collaboration with other national laboratories (e.g., the microscopic analysis with Oak Ridge National Laboratory [ORNL]) and industry (3M).
- The Stamenkovic/Markovic group has excellent interaction with other institutions and has led the project team well.
- Collaborators are listed to be ORNL (microscopy), the University of Pittsburgh (theoretical modeling), 3M (MEA testing), General Motors (GM) (technology transfer), and Brown (synthesis). Judging from the slides presented on just the past year's work, it is difficult to see where significant collaboration existed. It appears that ORNL contributed to the microscopy, particularly in terms of identifying composition versus

position over an individual nanoparticle. Theoretical modeling and MEA testing did not appear to play a significant role in the past year's achievements, so there is not much mention of the University of Pittsburgh or 3M. Given the degree of accomplishment in the past year, however, the investigators may not have needed these collaborations at this stage in the project. However, MEA testing should happen soon now that RDE testing has yielded some outstanding results. It is difficult to say whether Brown University played a role in the past year's work. For future reporting, the project may wish to adopt the convention of another ANL project and place logos to show which collaborators worked on which tasks.

- The microscopy collaboration with ORNL has been excellent, as has been the interactions with 3M. The interactions with the Brown University and University of Pittsburgh groups have been less obvious over the past year, and the nanoframe work was done with the University of California, Berkeley, which was not recognized as a collaborator.
- Individual team efforts are well orchestrated to maximize the synergy among the collaborators. However, the role of GM seems to be less clear.
- The project team features suitable collaboration, with industry, universities, and national laboratories represented. Better identification of the relative contributions of each team member would be useful.
- The collaborators' roles are well defined on slide 23 but not very transparent from the overall report.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- Cost analyses over the past 3–4 years have shown that the projected cost of a fuel cell system has hit a plateau in the range of about \$53–\$56/kW. DOE must fund efforts to reduce costs significantly. The most direct way to do this is to improve catalyst activity, which should result in an improvement of the polarization curve that drives down the area of the stack, resulting in less Pt, fewer membranes, fewer gas diffusion layers, and fewer plates. ANL has discovered at least two catalyst systems that significantly increase oxygen reduction activity. The project has explored catalysts with materials that are commonly used for manufacturing. Nickel nitrate is commonly used in industry and is handled safely. Reaction time to make the solid polyhedral nanoparticle is only three minutes. Lowering the cost of balance-of-plant components and some stack components often does little but lower the cost of an individual component. A project like this, which vastly improves catalyst activity, can lower the cost of the entire stack and also improve performance under lower pressure, which could reduce balance-of-plant component cost. The results of the project could be used to improve the entire system.
- The project has focused on developing different design platforms of multimetallic nanoscale catalysts that are highly relevant to advancing the commercialization of fuel cells. The nanosegregated Pt alloys demonstrated high performance, durability, and scalability potential. The nanoframe structures demonstrate exceptional specific activity and mass activity, but their scalability is under question.
- This project is very relevant to the issues of cost, performance, and durability being addressed by the Fuel Cell Technologies Office. This project has the potential to surpass DOE's research and development goals under realistic MEA conditions. Charts show that it has already surpassed DOE's target, although in RDE.
- The team has really made excellent progress on delivering a vision of new catalyst materials for oxygen reduction. New paths forward with regard to the synthesis of new materials and incorporation of the ionic liquid speak to exciting new directions.
- The use of Pt as a catalyst has been a major issue in developing fuel cells. This project aims to develop a catalyst with a novel structure that enables less use of Pt. The team members shed light on a basic understanding of ORR, a key process for electrocatalytic reactions.
- The project is extremely well aligned with the DOE Hydrogen and Fuel Cells Program (the Program) goals and with the need for step-change improvements to Pt catalysts. The dual approach of high durability and very-high-activity catalysts provides a real path to low-cost fuel cells.
- Enhancement of ORR activity is one of the most important areas on which to focus.
- The research being led by the Stamenkovic/Markovic group is critical for meeting the Program's goals.

Question 5: Proposed future work

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

- Future work points toward MEA testing of the catalyst in a fuel cell, which is needed in order to understand what the catalyst does in a cell environment and to begin gaining some understanding whether special processes (ink, application, pressing, etc.) are needed throughout the process of integrating the catalyst. MEA testing is also needed to understand the robustness to temperature, humidity, and transient operation. Most importantly, it is also needed to understand durability. It may also be interesting to follow up on further cell testing of the MSTF materials reported last year. A polarization curve was shown last year with MSTF that could possibly be improved if efforts were taken to address high current performance. Cell testing of catalysts with Au inner layers (especially accelerated stress tests) would also be useful to understand if the same trends from rotating disc experiments exist in a fuel cell.
- Considering that the project is ending, the future work is very relevant, particularly the MEA testing of nanoframes and scaling up of the synthesis approach.
- The future work is very well outlined with a focus on evaluation in MEA and scaling up to produce larger quantities.
- The Tafel slope is an important factor to evaluate the cathode catalyst.
- The future work is appropriate. The approach to date to focus on the catalyst structure and development of catalyst design knowledge for high activity and durability in a Pt-thrifted catalyst is appropriate. The project is now at the stage where increased focus on exploration of synthesis scale-up and preliminary MEA testing should be conducted.
- Per the “Future Work” section of the progress report, the team is focused on continuing catalysts optimization and scale-up. The materials seem to have reached a maturity where incorporation into MEA is required to assess DOE goals.
- The future plan focuses on a mechanistic understanding of the ORR reactions using the new nanoframed catalyst as well as the core-shell structured catalyst. On the other hand, the applicability of such a catalyst seems unclear.
- Incorporation of these catalyst materials into an MEA should be a priority for this team during the next year.

Project strengths:

- The efforts to improve catalyst activity are the most relevant work to the DOE fuel cell portfolio. The approach to develop nanoparticles that take advantage of high-PtNi specific activity through nanosegregation of Pt at the surface is a natural progression from prior breakthroughs and has sufficient scientific foundation. While the project is derived from prior work, it has also addressed challenges with immense creativity (e.g., nanoframes, MSTFs). The project has some of the very best RDE scientists involved in the measurements. In general, the project makes use of an all-star team of electrochemists.
- The project team has maintained a disciplined and detailed approach to develop structure-function relationships for high-activity, durable catalysts to maximize the use of noble metals. Strengths include in-depth theoretical understanding, catalyst synthesis, and material and electrochemical characterization.
- The Stamenkovic/Markovic group has worked hard to make this project successful. The catalysts development approach has been executed very well. The coordination with other team members is also a strength. The Pt-alloy catalyst technology is very mature and the durability seems excellent. The group seems to have progressed very well toward exceeding the DOE technical targets.
- ANL demonstrated outstanding synthetic capabilities. The contribution into structure-property relationships of different types of structures is of great importance. Different classes of materials demonstrate activity and durability superior to Pt/C.
- Strengths include the ANL team, ORNL, and collaborators; the material-by-design approach; the characterization techniques; and the excellent and accurate RDE evaluations.
- Strengths of the project are its focus on a pseudo-bulk and surface crystal orientation of catalyst concept to enhance ORR activity, and its effective collaboration with high-resolution microscopic analysis.
- The project features a strong, chemistry-based, innovative design of the catalyst, which enhances the catalytic activity for the ORR, leading to less use of Pt.

- This project features very-high-performing catalysts and novel directions for catalyst design.

Project weaknesses:

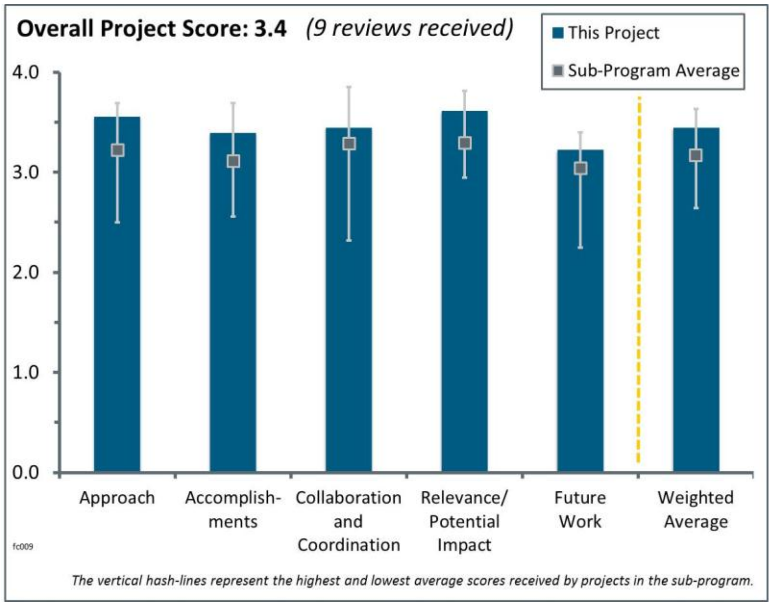
- Greater emphasis could be made on fuel cell testing versus RDE testing, especially now in the later stages of the project. The 3M and GM collaborations need to work better. The 3M MSTF polarization from last year may have sold the catalyst short, given the high activity. There should be some way to capture the activity of the catalyst in situ and have polarization curves that avoid major mass transport losses from as low as 1.0 A/cm^2. In general, the project needs to make better use of the non-ORNL collaborations. The best work appears to come from within ANL, but other partners could contribute more.
- There is almost no MEA evaluation, and there is too much focus on RDE data. MEA performance and durability are different and many things can be learned from MEA testing that can help in designing a stable and high-performing electrocatalyst. In addition, the researchers are synthesizing too many designs/structures instead of down-selecting one and putting efforts into making it work in MEA.
- As with many catalyst development tasks, a stronger focus on MEA development is needed. If it is difficult to transition the catalysts to MEA operation, the ultimate DOE goal of commercializing fuel cell technology may not be met.
- The optimization of MEAs of Pt alloys is not sufficient. The scalability and cost of synthesis of nanoframes is the biggest barrier at this point.
- The application of such a catalyst to fuel cell systems is still in question because of several issues, including the cost for the synthesis and scalability.
- The project features an ad hoc approach to materials discovery. The full team involvement is not clear.

Recommendations for additions/deletions to project scope:

- The work is excellent. The only recommendation is to focus on the MEA development. The reviewer is looking forward to seeing the fuel cell/MEA performance with these novel catalysts.
- Instead of developing any more new structures, the team should focus on how to realize the great activities in MEA with the current designs. It may not have the same improvement as in RDE, but any improvement in MEA will help the research to progress further.
- Promising outcomes of cathode catalyst materials can be seen. High performance of mass activity at MEA (rather than RDE) is expected, particularly for the nanoframe concept. Significant progress on the MSTF concept—which was presented in 2013—was expected because the manufacturing process is promising (already demonstrated by NSTF at 3M).
- Officially, the project only has three months left. Emphasis should be on fuel cell testing with the nanoframe catalyst, and in that, there is a lot to do. The team could add fuel cell testing on the Au inner layer catalysts, as well as another try with MSTF.
- The scalability and manufacturing of the catalyst with performance data should be provided.

Project # FC-009: Contiguous Pt Monolayer O_2 Reduction Electrocatalysts on High-Stability, Low-Cost Supports

Radoslav Adzic; Brookhaven National Laboratory



Brief Summary of Project:

The overall objectives of this project are to: (1) synthesize a high-performance Pt monolayer (ML) on stable, inexpensive metal or alloy nanostructure fuel cell electrocatalysts for the oxygen reduction reaction (ORR), and (2) increase the activity and stability of the Pt monolayer shell and the stability of the supporting cores, while reducing noble metal content. Project objectives for the current review period include: (1) scale up of syntheses of three catalysts, (2) obtaining perfect Pt ML deposition and achieving 100% utilization of Pt, (3) developing new methods for increasing the stability of core-shell nanoparticles, and (4) delivering a 300 cm^2 membrane electrode assembly (MEA) for testing at General Motors (GM).

Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- This project's core-shell approach has proven to be one of the most fruitful pathways toward lowering platinum group metal (PGM) costs in polymer electrolyte membrane fuel cells (PEMFCs). The continued efforts toward the use of non-noble-metal cores, now through nitridation of base metals, gives a pathway to significant further cost reductions and should be emphasized in future work. Brookhaven National Laboratory (BNL) should continue on the path to move away from reliance on rotating disk electrode (RDE) testing toward more demonstrations in MEAs. Work with currently inexpensive, but scarce, PGMs such as Re should be limited, as any substantial use of such metals in mass production would drive their prices up, potentially above that of Pt. Rhenium does seem to have an advantage in that it is generally extracted from different rocks than those that yield Pt and other PGMs. Efforts toward more manufacturable methods of making catalysts should be continued (e.g., reactive spray and scale-up of electrodeposition on powders). Direct electrodeposition on gas diffusion media is unlikely to be practical in mass production.
- A core-shell approach with a perfect Pt ML on cheap cores is a very good approach. It has the potential (also demonstrated already) to reduce Pt loadings. Technology transfer to industry would also address the scale-up issues.
- The Adzic team has been a leader in developing the core-shell approach to fuel cell electrocatalysts. It is very relevant that the team is able to reproduce these samples on a large enough scale to be tested in multiple laboratories.
- The approach of Pt ML catalysts is tackled in this project with great focus. The synthesis strategy is clear and has led to many new materials being developed. BNL has a keen awareness of the technical difficulties in scaling up the process, but it has devoted the proper amount of time and attention to this issue, working with industry and other collaborators.
- BNL uses innovative technology to develop low-PGM catalysts. Previous results on Pt-Pd cores have been very encouraging in the laboratory and have been scaled up for commercial evaluation. The new approaches are mostly electrodeposition based; however, plasma processes are now being developed to reduce cost.

- The methodology was invented at BNL and has since been verified at many places. BNL continues to innovate in this area and is making good scientific progress. The stability of the core materials is still a major concern, but some approaches have been laid out to address it. More evaluation in an MEA is required to differentiate good approaches from bad ones. That being said, the project has a more practical approach than most projects—showing transferrable technology from RDE to MEA and successful technology transfer to a catalyst supplier.
- The approach is focused on U.S. Department of Energy (DOE) targets to lower noble metal content and reduce the cost of the cathode catalysts in PEMFCs.
- The focus is on a novel concept of core-shell to realize a pseudo-bulk catalyst concept for both nanowire and nanoparticle.
- This year, BNL appears to have made good progress toward the development of low-cost, high-stability Ni4N cores while largely maintaining high specific activities, which has resulted in a good step forward for the project. Too much time was spent on catalyst development with Pd-based cores. While it was very important to use such materials in the concept demonstration phase, development work with PGM cores should be de-emphasized because of cost considerations. Evaluation of performance and durability at the MEA scale still appears to be rather limited. While the newly developed materials have very good activity and durability in RDE, MEA demonstration is critical and needs to be a larger focus now that non-PGM cores are available.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- Excellent progress has been made toward synthesis of highly active catalysts with low total PGM content. New, more stable cores—versus those of pure transition metals—were identified and synthesized. Much faster progress has been achieved in RDE testing versus fuel cells.
- Excellent progress with MEA evaluation was made by industry partner GM. High activities and very good durability have been demonstrated with the 0.030 mg Pt/cm^2 loading cathode. Air performance is poor, but suggestions were made that can improve it. It would be nice if the team showed some Pt/C benchmark data with similar Pt loading on the same plot to get perspective on how much improvement there is due to the core-shells.
- BNL is commended for reporting both transmission electron microscopy (TEM) and x-ray diffraction (XRD) data of its samples. Few researchers in the field do this, because the XRD is an average measurement; they cannot pick and choose areas of the TEM samples that support their conclusions. BNL investigated a number of bimetallic systems and worked with fuel cell developers to obtain fuel cell testing data. The nitride core work is also very interesting.
- BNL has delivered outstanding outcomes. Progress from last year is not as good as from previous years. It is good to evaluate ORR activity at the MEA level as well as in RDE.
- Good progress has been made, considering the project mostly ended. It was encouraging to see the application of Ru-Pt catalyst in an electrolyzer's hydrogen electrode. MEA activity measurement is often done at high oxygen partial pressure, which skews the activity higher. The principal investigator (PI) should comply with the DOE-recommended protocols.
- The project appears to have made a substantial step forward with the Pt ML on Ni_4N core system in terms of cost reduction.
- BNL continues to make new advances. The accomplishments over the past year are noteworthy because they include new concepts for generating active fuel cell catalysts.
- The team has primarily focused on stability and has been able to demonstrate impressive stability of its materials by taking advantage of interesting nanoscale effects. Significant increases in mass activity seen in RDE are being translated into MEAs.
- Several novel catalysts demonstrated total PGM mass activities comparable to more conventional Pt alloys—some using metals currently cheaper than Pt. BNL continues to demonstrate improved control over the composition and structure of particles at the atomic level. BNL demonstrated improved uniformity of the particles. There has been no further work on the promising hollow Pt particles discussed in past years. While the activity of those per mass Pt was not as high as those for the systems that have received follow-up, they were still promising in activity and are more likely to give adequate durability than those containing less-corrosion-resistant PGMs, such as Pd.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- There is excellent collaboration under this project. There is a strong team and strong interaction with industry. MEAs are tested by industry partner GM.
- A number of partners are actively participating in this project. It is good to see the contributions from theory and the testing of the materials at GM and other industrial partners.
- The interactions with industry (i.e., MEA and scale-up) and with the University of Wisconsin (UW) (i.e., theory and calculations) seem fruitful.
- Work is well coordinated with scientific and commercialization partners. The leveraging of DOE Office of Basic Energy Sciences funds and research activities for studying fundamentals is also well coordinated.
- This project is an excellent model of technology transfer to the industry through licensing.
- BNL actively seeks feedback from original equipment manufacturers (OEMs) and makes efforts to improve. This project is one of the most practical projects and shows great potential to contribute to the marketplace. However, it appears that most synthesis and characterization work was done at BNL. Collaboration with others is limited to testing. More collaboration in other areas will benefit this very promising approach.
- The project features very good collaboration between BNL, Michigan Technological University, and University of Wisconsin. More extensive collaboration with industrial partners would be helpful.
- Collaborators made a good start on MEA testing; such activity should be emphasized in the future. If possible, an update on development progress at the licensees of the patents from this project (e.g., N.E. Chem Cat Co.) should be given at DOE reviews. Reactive spray deposition is an interesting approach, but it is so different from the methods used at BNL that the relevance to the core of the project is unclear.
- The role of most collaborators appears rather limited. It is unclear what contributions, if any, were made by Johnson-Matthey Fuel Cells Inc. and Toyota, for example. The project would benefit greatly from having a dedicated development effort to optimize fuel cell electrodes.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- This project has led the development of core-shell catalysts that constitute one of the most promising pathways to the reduction of Pt usage in PEMFCs. Reducing Pt costs is one of the critical goals needed to enable use of fuel cells in mass-produced applications. By providing high-PGM mass activities in catalysts with high specific surface areas, core-shell catalysts provide a pathway toward addressing the problems in high-current-density operation in air that have typically been seen for low-loaded catalysts.
- Considering the high activities of low-PGM catalysts toward ORR, the project has the potential to significantly advance DOE goals toward commercialization of PEMFCs.
- This project has made a significant impact in realizing DOE research and development goals, and industries are interested in the technology.
- The work to develop stable, high-activity, and low-cost electrocatalysts is of key importance to the DOE Hydrogen and Fuel Cells Program.
- The work is extremely relevant to meeting the DOE goals of reducing catalyst loading in fuel cells.
- Enhancement of ORR activity is one of the most important technical focuses to fill the gap.
- Novel catalysts are essential for achieving DOE goals; however, it is unclear whether the added costs of adding the other metal components are fully considered. A full cradle-to-grave assessment of the costs and the environmental impact in terms of waste products produced during synthesis should also be considered as the methods continue to be perfected. Large-scale industrial manufacturing may be precluded due to some of the more expensive rare earth elements being considered.
- Although the team has shown that its catalysts are active and durable, at least in research settings, the catalysts still seem very complicated to make. As such, how much impact the team’s methods will have

remains to be seen. That said, the team is trying creative variants to the problem, including electrodeposition and reactive spray deposition, and it may overcome this issue.

Question 5: Proposed future work

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

- The future work plan makes sense for the continued optimization of the low-loading PGM core-shell catalysts.
- The MEA and stack testing at GM is a very promising step.
- The future work is well balanced. Durability tests need to be complemented by tests under "start-stop" conditions (i.e., between 1 and 1.5 V).
- The plan for the proposed work shows new concepts such as the onion ring structures, which may be interesting candidates. The economics of producing such structures need to be considered, both from a materials perspective and in terms of the possible waste products being generated during synthesis.
- The team is guided by good modeling efforts. One of the things BNLwould like to do is incorporate Y into its materials. This may be quite challenging, and technical barriers were not discussed.
- The team should continue the trend toward MEA testing and away from total reliance on RDE. MEA testing should include rigorous durability testing, as even metals as noble as Pd can cause major problems in fuel cells subjected to such tests. If nitrided base-metal cores show any real promise in RDE tests, MEA testing plans should include such catalysts. The effort to be expended on the promise of PtY and PtSc alloys (based on theory that neglected the severe problem of oxidation of Y and Sc) should be limited. Nonaqueous solvents and possibly thermospray are more promising approaches than any aqueous processing method, but the chances of such alloys being stable in fuel cell applications are very low. Straight vacuum deposition would seem to be a better approach if these materials need to be studied. If DOE funds are to be spent on these systems, it would seem to make the most sense for DOE to pay 3M to grow some PtY and/or PtSc nanostructured thin film at the 50 cm^2 scale.
- The project is ending. MEA testing should be included for all new catalyst work as a means to evaluate stability. As shown by many, catalysts with acceptable activity and stability in RDE often do not show the same properties in MEAs. More effort on materials that are valued by suppliers and OEMs should be made.

Project strengths:

- The PI has a vision of how active catalysts should be designed. This helps to synthesize highly active catalysts in a timely manner. In addition, the strong theoretical component provides fundamental insights on how the catalysts work.
- Strengths include the very strong team that made lots of MEA evaluations under O_2 and air, the strong collaboration with an industry partner, and the efforts to move forward with stack testing.
- The project features a novel concept of core-shells to enhance ORR mass activity significantly. It also features an excellent technology transfer model with industry partners.
- Strengths include the strong technical capability, a good record on practical invention, and healthy motivation toward commercialization.
- This project is a leading-edge electrocatalyst development effort with strong modeling, and ex situ and in situ characterization capabilities. An active effort to reduce the PGM content of cores is critically important.
- This is a very-well-managed project that continues to produce excellent results.
- The approach is multidisciplinary and is performed by extremely knowledgeable researchers.
- The project features impressive control of the deposition of multilayer nanoparticle catalyst systems. In addition, it features very impressive initial kinetic activities expressed per gram of Pt. The initial kinetic activities per gram of total noble metal or per unit metal cost at projected mass-production volumes are less impressive, but still encouraging. There is good coordination between synthesis and characterization.

Project weaknesses:

- One weakness is the continued reliance on RDE testing of ORR activity and, even more of a concern, for durability information.
- Direct electrodeposition onto gas diffusion media is unlikely to be feasible in mass production. While advanced catalysts on support powders can be dropped into fuel cell projects, catalysts electrodeposited onto gas diffusion layers would likely require extensive reengineering of fuel cell systems to give robust performance over such operating conditions as cold start-up and load transients.
- The PI is encouraged to utilize the U.S. DRIVE Partnership's Fuel Cell Technical Team durability accelerated stress tests and polarization curve protocols to allow for direct comparison to other projects. Too much focus continues to be placed on work with solid precious metal cores.
- It would be nice to see the high stability of the core in the case of the nitride-stabilized Pt-M core-shell catalyst in MEA. A benchmark against commercial Pt/C or PtCo/C with similar Pt loading was missing.
- Weaknesses include the inconsistency of MEA activity measurements and the high dependency on the use of PGM for a sufficiently stable core.
- MEA tests are needed to demonstrate the broader applicability of this work.
- The Pt ML is not very durable, as has been shown before.

Recommendations for additions/deletions to project scope:

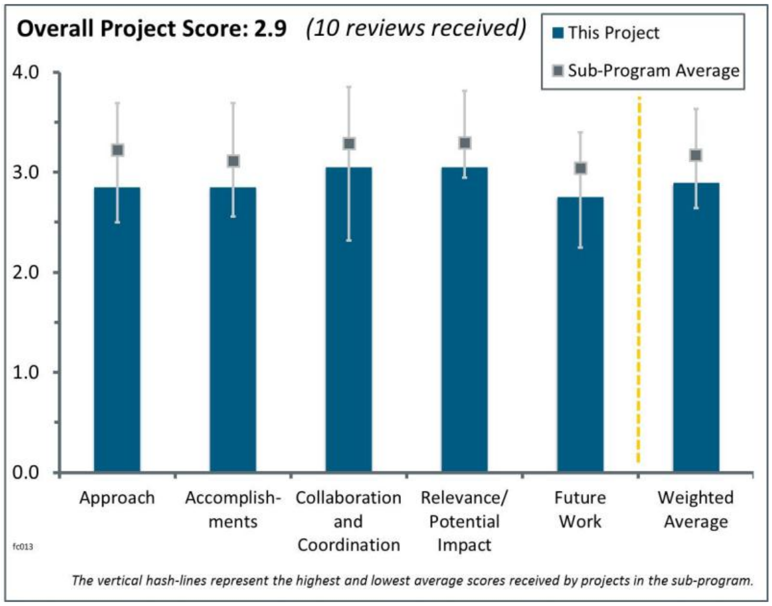
- Pure transition metal cores should be excluded from the scope; it has been shown multiple times that they leach. The issue of stability of ML Pt or mixed ML film in fuel cell tests needs to be addressed. It was reported by the Argonne National Laboratory team at last year's DOE Hydrogen and Fuel Cells Program Annual Merit Review meeting that three MLs of Pt are necessary to keep the shell stable in fuel cell tests.
- BNL should comply with DOE-recommended protocols. The team needs more understanding of the fabrication cost with the electrodeposition approach.
- Dedicated effort to integrate electrocatalyst into high-activity electrodes is a critical next step to validate the performance and durability observed in RDE.
- BNL should consider leaving PtY and PtSc to folks experienced with vacuum deposition. It should also work with OEMs to optimize core-shell catalysts for high-current-density performance in H_2/air.
- Improvement in air performance would be good.
- The hollow core-shell concept was expected to see progress.
- Catalyst costs need to be evaluated.

Project # FC-013: Durability Improvements through Degradation Mechanism Studies

Rod Borup; Los Alamos National Laboratory

Brief Summary of Project:

The objective of this project is to improve fuel cell durability without compromising component cost or performance. Los Alamos National Laboratory (LANL) will identify and quantify degradation mechanisms through characterization of the impact of component interactions, operating conditions, and electrode structure on durability and performance. Methods to mitigate degradation of components are investigated, and degradation mechanisms are used to design new materials and develop operating strategies. Models are developed relating components and operation to fuel cell durability.



Question 1: Approach to performing the work

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

- The project features a strong combination of characterization, diagnostics, and modeling, which is beginning to extrapolate the impacts of degradation on lifetime.
- The approach is generally good and addresses the known issues of durability in polymer electrolyte membrane fuel cells (PEMFCs). In some areas such as the electrochemical impedance spectroscopy (EIS) plots in slide 21 and 22, it is not clear if data are presumably taken under O_2 or air. Data taken under N_2 would provide the change in the catalyst layer resistance that is critical in estimating the change in catalyst layer thickness. The particle size distribution change (growth) with durability as a function of the type of carbon black has been well established for the last decade, and it is not clear what is new in that area from the presentation. Ce has been shown to be effective in recent studies by automotive companies when used in small quantities, and it does not leach out as badly as presented in this work. Mitigation has been addressed in this work, which is good, but most of the mitigation technologies for shutdown/start-up (SD/SU) have been patented by the automotive companies and are easily available. These mitigation technologies are being implemented in the next generation of fuel cell electric vehicles. It is not clear why these patents are being ignored and not studied in detail. The automotive companies are ahead of the national laboratories in this area, and it would be a good approach to study the mitigation techniques that actually work and bypass the need for durable supports. Perhaps a different group or laboratory should be funded to summarize these mitigation techniques.
- The approach is mostly a post-mortem approach—trying to simulate fuel cell operating conditions or obtain as much material from industry as possible and then figure out what went wrong. While this is a defensible approach, it is always a few years dated; materials change and often what researchers are looking at is no longer relevant. Cathode carbon degradation is a clear example; the original equipment manufacturers (OEMs) have suggested to decrease emphasis here because they have mitigation strategies with which they are content. Where the national laboratories can be most useful is chasing down mechanistic studies; for example, the investigators found that Ce doping ends up in the cathode and anode and leaves the membrane electrode assembly (MEA) altogether. There are questions about why it leaves, what counter ion it left with, and why it segregates in the catalyst layers. The investigators can only guess; a very simple experiment using a Pt black electrode would determine if it was catalyst affinity or carbon affinity that was

sequestering it there. If carbon; there are questions about whether a carboxylic functional group is leading to the capture, and whether carboxylic acids have a higher affinity than sulfonic acids. These are all simple experiments that could aid the community, regardless of the membrane/catalyst system being investigated.

- Overall, this project is attempting to address key commercialization barriers due to insufficient durability of PEMFC materials. The project is well focused in terms of studying relevant degradation modes. However, this reviewer questions the relevance of exhaustive studies of carbon corrosion; Pt particle growth; and electrode thinning on outdated carbon-supported, pure-Pt nanoparticle catalysts, because these topics have been studied in depth for many years and it is clear they cannot achieve the performance or durability targets. Significant utility can be obtained from all this work if it results in the development of an overall degradation model that is predictive of performance as a function of time and degradation extent. While modeling is listed in the approach slide, little modeling appears to have been done, and few, if any, correlations of the component degradation extent to performance loss have been made. The results appear to be largely one-off tests without replicates. Sample-to-sample variability effects could be large. For example, the very unexpected result on slide 11, where the H_2 crossover increased substantially with *dry* SD/SU, should be repeated before any conclusions are drawn.
- The team is composed of respected groups from national laboratories and industries. The multidirectional approaches taken for the completion of all tasks are adequate. All the technical barriers have been addressed appropriately. However, the team has used MEAs composed only of Nafion® ionomer (in membrane and catalyst layers). The team should plan to sample MEAs that use different types of perfluorinated (e.g., 3M, Solvay) ionomers to broadly understand MEA degradation.
- The team is doing a limited set of durability tests combined with thorough, advanced post-mortem analysis, but it is not doing deep enough parametric studies (employing design of experiments) and diagnostics to provide the fundamental mechanistic learning that will help developers come up with effective degradation mitigation strategies. The researchers seem to run isolated sets of tests and then diagnose what kind of degradation is present, rather than actively address the critical degradation phenomena that are limiting (at least for light-duty automotive) fuel cell commercialization.
- This project has a huge scope; part of the approach should include sensitivity studies to organize degradation mechanisms in terms of expected mV lost at the end of life, and resources should be prioritized. Currently, it seems that resources are prioritized around available characterization methods.
- The project employs a combined modeling and experimental approach and coordinates activities with other durability projects.
- This project features a good approach based on extensive characterization and modeling, and it facilitates the development of mitigation strategies.
- The major problem with the approach in this project is that it overlaps with activities being pursued within automotive and fuel cell stack OEMs. Unlike LANL, the OEMs have access to state-of-the-art materials through non-disclosure arrangements and joint development agreements. Despite identification of an automotive durability target, there is no automotive OEM associated with the project. The different tasks shown involve Ce migration, SD/SU degradation, OCV degradation, and carbon corrosion. These phenomena are all well studied by OEMs. The best approach for the project is to discover experimentally new failure modes that have not been observed before. However, without state-of-the-art material sets and without OEM cell designs, the effort turns into guesswork. This is especially true when transient operating conditions (e.g., SD/SU) are also missing. The protocol for membrane degradation under SD/SU needs to be clearly reported. For a complete analysis of carbon corrosion, statistics for carbon loss, thickness loss, electrochemical surface area (ECSA) loss, and performance loss should all be rigorously compared. This should be done for both absolute numbers and percentages.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- The team has made great progress in defining MEA degradation mechanisms and, to some extent, providing mitigation conditions. The modeling of carbon corrosion and loss during SD/SU and expansion of the model for membrane durability is well done. The team has done a good job in identifying membrane and catalyst degradation mechanisms separately and offering mitigation strategies that are not at the expense of performance or additional cost. The team also has done a good job in identifying the Ce migration from the membrane to catalyst layers. The team should consider conducting the studies with MEAs with a Ce-containing membrane (such as Nafion®-XL) and Ce-containing Nafion® ionomers in the electrode layers to understand if the migration behavior of Ce occurs when there is no Ce concentration gradient between the membrane and catalyst layers.

- The presentation shows comprehensive studies on MEA degradation and mitigation with mostly qualitative analysis on cause and effect. The results of Ce migration from membrane to catalyst layers is interesting and disturbing, which warrants a quantitative study by modeling.
- The team has identified some new mechanisms and effects, as well as screened several mitigation strategies.
- The accomplishments are moderate, based on the approach adopted. Based on what is already known in the literature and in industry, new advancements in techniques or new insights are a bit scarce. Slide 11 does not have complete information on the plots, as is the case with several other slides. It is unclear if the ECSAs after the durability tests were measured at room temperature with 100% relative humidity (RH). The HUPD peaks look smeared, indicating that the conditions for the measurement of the ECSA were suboptimal. Low N_2 flow and 100% RH provide the best HUPD areas. ECSA values are not reported in units of m^2/g, so the base ECSA value is not known from the figures.
- Nearly all of the work presented for this year is on carbon corrosion. Very good work was done on the carbon corrosion modeling and tornado plot, which are very good and helpful; however, it is not clear how much of a problem carbon corrosion appears to be and this could be too large of an effort on a questionable problem. Only superficial work was done on Ce, where the team is capable of doing some very strong work; it has all of the capabilities.
- Good progress has been made toward evaluating the effects of carbon corrosion and load cycling accelerated stress tests (ASTs) on electrode and electrocatalyst structure, but it is questionable how useful this work is because the degradation of these materials has already been so well documented. Characterization of Ce movement in MEA and modeling of pinhole growth in polymer electrolyte membranes (PEMs) at Lawrence Berkeley National Laboratory (LBNL) are useful.
- The team has presented several examples of not-well-understood degradation results, which could potentially lead to misleading conclusions. For example, the start/stop testing results compared at dry and wet conditions could lead viewers to believe that anode purge during shutdown leads to accelerated membrane degradation. Because these results conflict with expectations, they warrant repeating with added diagnostics before dissemination. It was also unclear where SiO_2 particles came from in the OCV tests and why they were in the MEA in the first place. The work on decreasing cathode Pt weight loading compared to diluent carbon showed some interesting results, but without a mechanistic understanding, it does not drive the development of improved electrode designs. The catalyst layer porosity loss during the drive cycle was particularly interesting. This is one area where the results could drive developers to seek solutions as well as help in the development of performance degradation models, assuming the porosity change can be quantified and modeled.
- Results involving membrane degradation under SD/SU with dry conditions are somewhat confusing. It is unclear why membrane degradation would “level off.” This is rarely observed. Observations of Si-O particles may be particular to the membrane being used. It is interesting to see the cathode ionomer skin missing, but ionomer degradation on the cathode side has been reported before. Quick decrease in thickness and porosity due to carbon corrosion for Vulcan or high-surface-area carbon has been observed. Slower decrease in thickness loss for graphitized carbon has also been observed. Some interesting trends were shown: preference of Ce in catalyst layers, quicker Pt particle growth for graphitized carbons (and then no more), ionomer removal over channel regions during OCV testing, and inclusion of residence time in the SD/SU model (could be used as a way to translate to different cell designs).
- It is unclear where the Ce goes, whether it is found in the fuel cell outlet water, and what the mechanism is for its loss. Presumably, a better understanding might lead to mitigation strategies. The effect of the cool stack in comparison to an anode purge (slide 11) is unclear. There is a large amount of material generated by this team. It would be quite helpful to clearly map the material between the milestones (slide 7) and the material presented (proper labeling, etc.). This extends to other developments that may not be captured by the milestones.

- The accomplishments toward Ce transport and electrode cracks do not go beyond current literature and patent disclosures. These issues will certainly be resolved by manufacturing engineers in a commercial product. It is unclear how a milestone can be "Complete, continuing."

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The team has good collaboration with different national laboratories and industries for this project. All of the different institutions have appropriate technical collaborations and interaction on the project.
- The content in the presentation shows excellent team efforts toward achieving the project goals.
- Collaboration partners are well aligned with project objectives.
- Collaborations with other organizations are generally good. The modeling work from Argonne National Laboratory (ANL) on performance and hydrogen crossover has too many critical electrochemical parameters that are variable and used as fitting parameters. For all models, the value of the exchange current density, Tafel slope, and ORR reaction order should be clearly stated and compared to the literature to verify accuracy.
- Collaboration, especially with the other national laboratories and universities, is a strength of this project. One major recommendation is that the researchers should solicit more input from OEMs besides Ballard (especially automotive) to help ensure that they focus on the most critical problems and avoid studying durability issues that have already been solved by industry.
- The project has many team members providing input that seems well aligned; the effort is coordinated and managed well, but it is not entirely clear how much true collaboration is occurring among the partners.
- The project is desperately in need of an automotive OEM collaborator. While it is true that Ballard has past automotive experience and has resumed some automotive work with Volkswagen, Ballard would not be expected to have the same perspective as an automaker that has had a consistent and continuous effort from the stack level to the vehicle level over the past decade. The addition of this collaboration is necessary so the investigators will avoid overlapping work and focus on the aspects of stack failure modes that are in need of more fundamental study. Collaborations with other national laboratories appear to deliver the needed microscopy results, as well as other characterization. Collaboration with the University of Nancy has yielded a segmented half-cell potential technique that was noted at last year's review for delivering results that confirmed the hypothesis about SD/SU degradation mechanisms.
- This project poorly integrates input from the key stakeholders. This list of collaborators seems to be heavily weighted toward national laboratories. If a group attempting to make a profit on a fuel cell system indicates that an area of research is of little value, serious considerations should be given.
- It seems like the researchers are fighting yesterday's battles. Close work with an OEM would greatly aid this effort.
- There are extensive collaborations with varied institutions. LANL leverages a similar European-based project, partly leads the activities of the durability working group, and conducts joint accelerated stress tests with a project tasked to develop accelerated stress test protocols (FC-016).

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The project is relevant to the objectives of the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. The activities are aligned with DOE's goals. This project is clearly focused on defining MEA degradation mechanisms through experimentation and modeling, and thereby on developing mitigation strategies. Due to the difference in membrane and catalyst composition and quality from various vendors, it is important to understand the similarities and differences in degradation behaviors under fuel cell conditions.
- Improved understanding of component-level effects and mechanisms affecting reliability is valuable. The team needs connections to OEMs or system integrators that have enough long-term data to guide the focus;

carbon corrosion has been studied many times. A rigorous failure mode and effects analysis (FMEA) based on real long-term data is needed to ensure relevance.

- The project aims to explore degradation mechanisms, find mitigation methods, and develop durability models at the component level, including component interfaces and component interactions. Its outcome would help develop durable fuel cells with better knowledge in materials selection and cell design.
- Durable unit fuel cells are essential for successful commercialization.
- Durability is one of the two barriers to the commercialization of fuel cells—both automotive and stationary. This project is relevant to meeting targets. Despite an approach that overlaps with what stack or automotive OEMs already do, this project does show the potential for digging deep into understanding the fundamentals of degradation mechanisms. With the right collaborations; the right cell testing fixtures; the right material sets; and, more importantly, a focus on answering deeper questions, this project could yield the answers that OEMs need. For example, OEMs already understand some of the basics about carbon corrosion, which includes an understanding of how carbon loss, thickness loss, electrochemical area loss, and performance loss are related, and how they are related for different carbons and for different Pt weight percentages. It would be more interesting for this project to understand how the ionomer is affected under SD/SU in terms of a chemical degradation mechanism, or what reactions are involved. LANL has a wealth of chemical characterization tools at its disposal; these techniques should be used so that LANL is not left just relating cell performance and diagnostics to observations from microscopy and energy-dispersive X-ray techniques.
- Many of the phenomena observed in this project are known by the OEMs (e.g., lower Pt weight loading proves more effective than diluent) or conflict with at least some OEMs' observations (e.g., gas diffusion layer degradation, Ce wash out). Effective SD/SU mitigation control strategies are also well known among many OEMs. It would be more relevant if LANL could develop degradation models that OEMs can use to project the degradation of best-in-class materials as a function of operating strategy.
- The use of a drive cycle is definitely a good idea and of practical value. It is unclear how useful these results are to the automotive fuel cell industry; the trends are well known but for different catalysts and different systems. Issues such as SD/SU are specific to the flow fields used; flow rates; and many other system factors such as the RH of the purge gas, compressor cooling, etc.
- The history of PEMFCs has been one of moving targets, changing materials, and changing assumptions toward operating conditions and balance of plant. If a project is going to be funded for five years, it has to be given the flexibility to move with these changes. Working toward targets established six years ago in a proposal limits the usefulness of this talented group.
- Several components of this work are not considered to be very important by the U.S. DRIVE Partnership's Fuel Cell Tech Team/OEMs/suppliers.

Question 5: Proposed future work

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

- Focusing on the catalyst-layer morphology effect and carbon/ionomer/catalyst interaction (particularly for low-Pt-loaded MEAs) is consistent with the need to reduce cost for fuel cell vehicle commercialization.
- Of all the suggested future work, the effect of the Nafion® ionomer on catalyst durability is of the highest importance and value.
- The future work described is aligned with the proposed work of the project. The team should consider working with Ce-containing electrodes and a Nafion®-XL membrane to better understand the Ce migration behavior. In addition, LANL should expand the use of MEAs to non-Nafion®-type ionomers.
- The following should be removed: conventional carbon corrosion studies (OEM overlap); SD/SU (too system- and cell design-dependent, unless done with OEM); Ce additive work (insofar as it overlaps with what General Motors [GM] has already published); and membrane durability work based on membranes of a generation prior to ca. 2010/2011. The following should be kept: study of catalyst-layer cracks and their effect on membranes, and all catalyst/ionomer interaction and ionomer mapping work (with a focus on developing tools for doing so, and on fundamental understanding of Pt/ionomer or C/ionomer interactions). Pt alloy work should only proceed if state-of-the-art alloys are to be used. Conventional Pt alloys from even 3–4 years ago are considerably different from what has been developed. There should be collaboration with either the GM dealloyed PtNi project or the ANL PtNi nanoframe project. There may also be other supplier

materials that have very recently been developed. These comments apply as well to the section regarding "Fuel cell catalyst widening materials models." Mitigation work needs to follow the development of a mechanistic understanding of failure modes. For example, understanding how to mitigate ionomer poisoning of Pt would follow understanding whether and to what extent ionomer poisons Pt.
- A few topics proposed for future work include defining the effect of individual degradation mechanisms on durability. Post-mortem characterization of long-term—on the order of 1,000 hours or more—test fixtures should be done to set priorities. The team should move on to more applications. It should design mitigation strategies, design accelerated tests, or extrapolate lifetimes more effectively.
- More work needs to be done on Ce, which appears to be in all future PEM systems. We do not have a very good idea of what affects reaction rates, what are competing reaction rates with ionomer degradation, how much is needed, how it competes with Fe, how the Ce:Fe ratio affects degradation, the temperature dependence of the kinetic rates, how Ce leaves the membrane, why it moves to the catalysts layers, etc. The proposed future work is very ambitious for a project that is winding down.
- With the exception of "Discern carbon/Nafion®/catalyst interactions and structure on durability," which should become a primary focus of this project, and "Expand our previously developed models on Pt dissolution," most of the proposed future work will not be valuable to OEMs. Membrane degradation work is only valuable if done on the best available robust materials (beyond Nafion® XL). OEMs know they must eliminate electrode cracks to prevent membrane degradation. OEMs and PEM suppliers have done extensive work on optimizing the form of Ce to prevent degradation and minimize performance loss, and they have developed their own effective start-stop strategies. The team should bring back the plate work because it is known that there is a strong correlation between plate corrosion and membrane degradation. It would be interesting to determine which is the initiator—Fe from the plate initiating PEM degradation or F- from the PEM causing plate corrosion.
- Larger focus should be placed on incorporation of state-of-the art materials (e.g., core-shell and dealloyed Pt alloy cathodes at target loadings), rather than on Pt on carbon, which is clearly unable to meet performance, cost, and durability targets. The team should deemphasize catalyst-layer morphology studies unless results are to be incorporated into a predictive model.
- It is difficult to discern an overall direction(s) and where efforts should concentrate. It is unclear whether the team regularly obtains feedback from OEMs, including for stationary applications.
- The team should keep the focus on the reaction surface.

Project strengths:

- The team is well organized and has the best technical people for conducting the durability study. The team is composed of respectable research organizations with adequate expertise. Overall, the team is equipped with the knowledge base, resources, and industry/academia/national laboratory mix required for the success of this project.
- Collaborations with national laboratories yield characterization results. Investigators have a deep background on fuel cells (>20 years). Despite the overlap with OEM work, most analysis on carbon corrosion is accurate. The project's future direction to look at catalyst/ionomer interactions and to do ionomer mapping is correct.
- This project has access to outstanding analytical and modeling resources through ANL, LBNL, Oak Ridge National Laboratory, CEA, the National Institute of Standards and Technology, and the University of Nancy, which have supported the team's durability studies.
- The project is focused on characterizing and improving the durability of PEMFC components and MEAs, meaning that it is directly aligned with a key DOE barrier. Collaborations are appropriate toward addressing project objectives.
- The project features a well-connected, varied, and experienced team that has access and use of state-of-the-art characterization equipment.
- The results are a robust extension of the previous work, and the principal investigators are knowledgeable of most of the work done in the area. Collaborations are good and effective.
- The project team has the expertise for characterization and developing mechanisms for degradation.
- The project features strong coordination among many national laboratories with various areas of expertise.
- The project team's characterization ability is a strength. The team has widespread capabilities.
- The project features a knowledgeable team with state-of-the-art characterization and modeling resources.

Project weaknesses:

- Much is said about the lack of cooperation with an automobile manufacturer; without a direct contract, the project investigators should work very hard to reach out to OEMs to make sure that their work remains relevant. If OEMs do not want to cooperate or do not think the investigations are valuable, it raises the question of funding this type of work in the first place.
- There is a lack of input from automotive OEMs on the most relevant durability challenges. This project is heavy on diagnostics (which is good), but it would benefit from more parametric studies and designed experiments to understand the impact of operating conditions on the various degradation mechanisms being studied.
- Existing diagnostics such as ECA and EIS have not been applied to their fullest extent to obtain useful information. Some of the results presented have been well established for the last decade and do not provide any new insights. Practical mitigation techniques for SD/SU from easily available patent literature have been ignored.
- A significant portion of the work overlaps with OEM activities. A deeper fundamental probe of failure mechanisms is needed. There is a lack of access to state-of-the-art materials, cell designs, and system operating conditions. There is also a lack of automotive OEM collaboration.
- The project features a combination of a large number of research organizations, which may be a management challenge for the prime organization.
- There is insufficient focus on state-of-the-art materials, and too much focus on characterizing degradation-induced electrode/electrocatalyst changes of previously studied, unstable cathode catalysts.
- More input is needed from industrial partners to prioritize the tasks and phenomena.
- Prioritization of resources and adaptability are weaknesses of this project.
- Mass transport losses seem to be ignored, but perhaps they are not as important anymore.

Recommendations for additions/deletions to project scope:

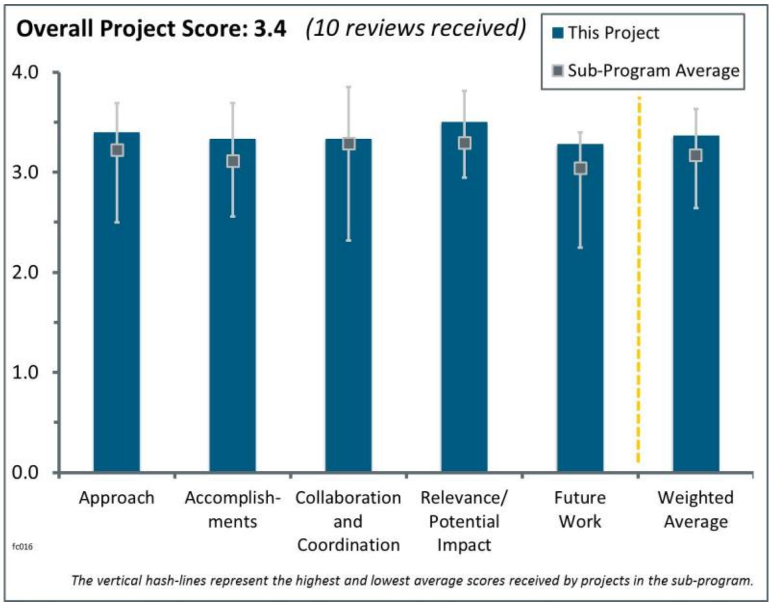
- The following should be removed: conventional carbon corrosion studies (OEM overlap); SD/SU (too system- and cell design- dependent, unless done with OEM); Ce additive work (insofar as it overlaps with what General Motors [GM] has already published); and membrane durability work based on membranes of a generation prior to ca. 2010/2011. The following should be kept: study of catalyst-layer cracks and their effect on membranes, and all catalyst/ionomer interaction and ionomer mapping work (with a focus on developing tools for doing so, and on fundamental understanding of Pt/ionomer or C/ionomer interactions). Pt alloy work should only proceed if state-of-the-art alloys are to be used. Conventional Pt alloys from even 3–4 years ago are considerably different from what has been developed. There should be collaboration with either the GM dealloyed PtNi project or the ANL PtNi nanoframe project. There may also be other supplier materials that have very recently been developed. These comments apply as well to the section regarding “Fuel cell catalyst widening materials models.” Mitigation work needs to follow the development of a mechanistic understanding of failure modes. For example, understanding how to mitigate ionomer poisoning of Pt would follow understanding whether and to what extent ionomer poisons Pt.
- The team should not include the impact of catalyst-layer cracks on membrane durability. OEMs already know they need to eliminate electrode cracks, and they know how to do it. There is fruitful work to be done on bipolar plate durability studies, especially if LANL can get access to plates made from lower-grade metals such as ferritics, which will be lower cost but may be more susceptible to corrosion.
- More emphasis is needed on quantitative analysis to better understand the observed degradation causes and mitigation effects.
- The effect of Nafion® ionomer on the catalyst durability is of the highest importance and value, and it is recommended to be the main focus of future work.
- A joint publication with an OEM regarding the SU/SD work might open the lines of communication and close some gaps.

Project # FC-016: Accelerated Testing Validation

Rangachary Mukundan; Los Alamos National Laboratory

Brief Summary of Project:

The objectives of this project are to define the correlation of component lifetimes measured in an accelerated stress test (AST) to real-world behavior of that component; validate existing ASTs for catalyst layers and membranes; and develop new ASTs for gas diffusion layers (GDLs), bipolar plates, and interfaces. The project also strives to develop accelerated testing protocols to enable projection of durability and to allow for timely iterations and improvements in the technology.



Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- The project addresses durability, a key barrier to fuel cell commercialization. The use of available bus durability data was beneficial and provided useful information, even though it is not directly related to automotive work. It was a good use of available resources, because similar data for automotive durability is not available publicly. The project has included the U.S. DRIVE Partnership drive cycle in the analysis, so comparisons can be made between ASTs and what is representative of real-world usage.
- The project has a well-defined approach that is meeting targets. Correlating ASTs with real data is essential to bring research costs down.
- A lot of work has been performed with very many results. ASTs taking the cells up to 1.5 V are questionable because this is beyond the OCV.
- The project features a sound and flexible approach with field-tested materials, some virgin materials, and other materials for ASTs.
- It is very useful to have AST protocols to help fuel cell component suppliers develop more durable products, as well as to help separate different decay mechanisms with these controlled conditions.
- The project features a logical approach that is showing good results.
- Running component-specific ASTs and characterizing the tested samples is tedious but necessary to find the statistical correlations for lifetime projection. The reliability of the obtained correlations requires a sufficiently large amount of the field data.
- The project features a good use of existing data from real operation to formulate test protocols. However, the project's over focus on GDL degradation may not pay off because it is not observed anymore on state-of-the-art materials.
- It is generally difficult to explain the correlation between materials tests and field tests. However, LANL carefully picked the materials set to show the correlation. It was good to show the differences in degradation rate by AST protocols. The important thing is to understand the AST's impact on degradation or failure.
- The wealth of data is great.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The team has achieved excellent detail and accomplishments over the last year. Analyzing ASTs to determine what is too aggressive and what is too passive based on real data is an important step. Defining gaps in ASTs and working to develop ASTs to fill those gaps is also a great accomplishment.
- LANL showed excellent data of degradation and failures with respect to AST protocols (e.g., new and old carbon corrosion ASTs with potential cycles versus potentiostatic, respectively). It is good to distinguish the membrane stress factors of relative humidity cycles and potentials.
- LANL showed good progress in demonstrating the ASTs. LANL should focus on developing relevant test protocols and not on the degradation mechanism for a specific material set, because this differs among material sets. The project has addressed the reviewers' comments very well.
- The team has made good progress over the past year, especially with respect to recommendations on alternate ASTs that should provide higher acceleration factors, which is important because more robust components and materials are being developed.
- A considerable amount of useful data has been collected and analyzed.
- The project has quantified carbon corrosion/oxidation during AST testing and has provided data to show how catalyst layers densify during aging. The carbon support corrosion work has been very beneficial. LANL has identified GDL aging modes. The membrane modeling effort looking at the growth of pinholes appears to be new. This work could be really useful when a pinhole initiation model is incorporated. It is not yet clear how the GDL degradation affects performance. More work should be done to determine how the GDL aging affects performance. It is not clear how much of the membrane degradation work covers new ground versus going over ground that has been previously covered by others. Previous work in the literature has indicated that a combined mechanical-chemical cycle is needed to simulate real-world degradation, and DuPont and others have correlated degradation under combined cycles to real-world degradation. While the ASTs have shown their value in comparing materials and demonstrated that the degradation mechanisms are similar to those in real-world use, it is not clear whether the mathematical correlations that allow one to determine real-world (or U.S. DRIVE drive cycle) durability from AST performance have been developed for most of the tests.
- The project has built up a large set of data under various AST protocols. The statistical correlations for the tested fuel cell components are not explicitly described.
- Metal bipolar plate and interface ASTs do not appear to have been addressed. However, it was mentioned that these activities were previously deemphasized. On slide 9, it appears that the carbon corrosion rate data supports the hypothesis that as corrosion proceeds, the first layer of carbon, which is more corrosion resistant, disappears first, followed by more easily corroded carbon (greater corrosion current), which has a more exposed area (corrosion lasts longer). On slide 13, it is unclear whether the lateral membrane damage is the same as delamination. It is unclear whether the mechanism associated with mass transport losses in the GDL is clear. It is unclear whether changes in carbon surface groups or accumulation of contaminants on the surface affecting porosity or surface properties, etc. have been considered as possible causes. Therefore, perhaps it is premature to claim a good correlation (slide 22). Correlations between accelerated and field data for specific metrics are presented (reviewer-only slides), although the GDL degradation seems to have been left out of the analysis. It is unclear if there is any particular reason why the analysis was not completed.
- It is pretty unclear how the results relate to the "real world."

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- Excellent collaboration is evident between Los Alamos National Laboratory, Oak Ridge National Laboratory, and Lawrence Berkeley National Laboratory. Collaboration is evident with other degradation projects funded by the Fuel Cell Technologies Office through the Durability Working Group.
- Excellent team efforts can be seen from the testing data and analytical results given in the presentation.

- LANL has worked with a variety of collaborators and is responsive to recommendations, including those from the U.S. DRIVE Fuel Cell Technical Team (FCTT).
- The project features good collaboration with materials suppliers.
- Collaboration is satisfactory, but it would have been interesting to see other fuel cell manufacturers involved. It is unclear whether Ballard is representative of the industry. The reviewer acknowledges that much of the information is proprietary and other manufacturers may not want to participate.
- The project is well connected with multiple DOE laboratories and industry partners. Additional free support and participation in the Durability Working Group help to strengthen the project.
- The team is varied with extensive experience. However, it would be desirable to gain the support of other industry representatives with additional field data for AST validation.
- Collaboration appears to be limited to ionomer suppliers. This may be because degradation mechanisms are specific to material sets and involve so much know-how that most original equipment manufacturers (OEMs) are not willing to collaborate with their best materials. The project should focus on developing accelerated test protocols that are relevant to real operating conditions. There should be more conversation between European- and Japanese-funded projects as well.
- More interaction with manufacturers/operators is strongly recommended.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- This project is highly relevant to meeting DOE goals. Cheap, quick, and effective ASTs can help manufacturers decrease the time required to assess new ideas and materials, thus helping the industry and DOE meet their targets.
- The project is relevant and addresses durability, a key barrier to fuel cell deployment. While the automotive OEMs may have their own correlations between the ASTs and real-world behavior, that knowledge is not public. It is important for DOE to have correlations between accelerated tests and real-world behavior (or as close to it as it can get with a drive cycle) to be able to understand how the durability of current technologies determined in ASTs compares to the durability needed in real-world usage, and to determine if the ASTs are applicable to the new materials being developed (i.e., accelerate the degradation modes being observed in real-world usage).
- Durability is one of the most significant gaps for automotive fuel cells. Study and basic understanding of degradation and failure modes are important. ASTs should be based on these understandings.
- Durability is a major barrier, and this project was very beneficial to the community’s understanding.
- Development of relevant ASTs is very important to accelerate the learning cycle on durability.
- In view of the recent developments in components durability, ASTs have gained increased relevance.
- ASTs are very effective tools.
- ASTs are necessary to reduce the development time of durable fuel cell components. The impact of the developed AST protocols will depend on the confidence level of the obtained statistical correlations.
- The real impact of the work on fuel cell development is missing.

Question 5: Proposed future work

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

- The project is scheduled to end this year; however, future work (presumably for a follow-on project) is described. The proposed future work will expand to include more relevant materials such as Pt-Ni alloy catalysts. The continuation of long-term drive cycle testing is beneficial. Correlation to drive cycle testing will be the next best thing to correlation with automotive usage, but data will be much more available. The evaluation of more accelerated ASTs will be beneficial. New, advanced materials degrade slower, and more aggressive ASTs are needed to determine when they fail.
- There is well-defined future work that will enhance the overall project.
- The proposed future work features a good roadmap to achieving the objectives.

- The reviewer wonders if it would be realistic to consider modeling the combined chemical and mechanical membrane degradation, which would not be trivial, to validate and subsequently use to predict operating conditions that would accelerate the AST. For instance, adding a solvent in the reactant stream would magnify the swelling of the membrane (hydrophobic rather than hydrophilic domain) and could lead to higher stresses (it is unclear if it is relatively easy to implement). Cell operation, rather than OCV, in the presence of an organic contaminant leads to significantly larger amounts of peroxide (without harming the membrane directly).
- The researchers should consider a membrane AST that cycles the current density (at constant inlet RH) instead of cycling the RH, which is more challenging; this will also provide both mechanical and chemical stress, but it will be easier to implement.
- Expanding into membrane electrode assemblies (MEAs) with low-Pt-loaded Pt-alloy catalysts makes the project more relevant to fuel cell electric vehicle (FCEV) product development in the automotive industry.
- The most important objective is to understand the degradation and failure mode mechanism and stress factors rather than evaluating (or selecting) AST protocols. It is still questionable to test with the Fuel Cell Commercialisation Conference of Japan protocols. LANL needs to identify what stress factors are apparent from these tests.
- The focus should be on a precompetitive material set because degradation mechanisms are specific to material sets. To be relevant to real operation, LANL should be focusing on increasing the frequency of stress, not on the magnitude of stress.
- The proposed future work will lead to further results; it is not clear if the impact will be improved.

Project strengths:

- LANL is making good recommendations on advanced AST protocols, which are required. It makes a recommendation on new ASTs based on real-world data, not just FCTT feedback (e.g., GDL AST recommendation).
- LANL is well connected and has a strong project focus. Defining and evaluating ASTs is an area of strength.
- The project features a good mix of accelerated tests and materials characterization to try to get a bit more information on degradation modes and mechanisms.
- The project features a varied and experienced team with good characterization capabilities. ASTs are needed in view of the recently improved materials. LANL has a good approach.
- Strengths include the effective collaboration among national laboratories (for testing and characterization) and industrial partners (for fuel cell components and field data).
- The project features good capture and analysis of the data.
- The established testing capability is consistent with FCTT test protocols.
- The project features open collaboration and good testing capability.

Project weaknesses:

- The unavailability of automotive field data from OEMs is a weakness. If this data were available, this work would be much more useful.
- Several causes could be ascribed to GDL degradation. However, the real cause(s) has not been clearly ascertained.
- It is pretty unclear how the results relate to the “real world” (i.e., support development and improvement). No correlation is seen between the results and measures to be taken.
- There is insufficient FCEV data for developing reliable statistical correlations.

Recommendations for additions/deletions to project scope:

- The project could include catalyst-layer morphology in characterization, particularly for low-Pt-loaded MEAs. The information on catalyst-layer morphology change (e.g., agglomeration) will greatly help voltage loss breakdown by proper catalyst-layer modeling.
- More interaction with manufacturers/operators is strongly recommended. Results have to transform into actions to be taken to improve durability under real conditions.

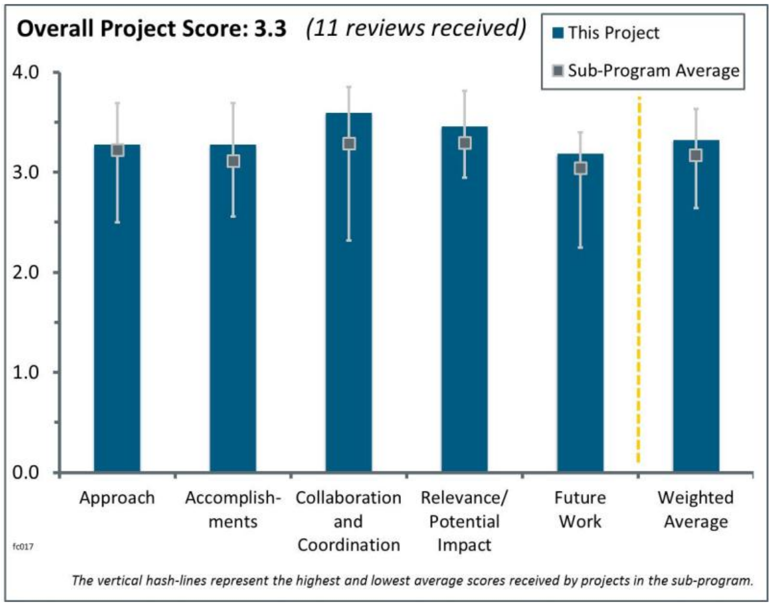
- It is unclear if there was a requirement to offer solutions to enhancing the durability and performance of the materials, but it would be useful to have ideas presented.
- The team should focus more on studying and understanding degradation and failure mode mechanisms with specific stress factors. It should analyze stress factors in AST protocols to correlate degradation and failure mode, rather than just test AST protocols.
- LANL should find ways to document learnings. There should be more conversation with European-and Japanese-funded projects.
- Work to date has focused on materials that do not meet DOE targets. Future work needs to be done with advanced materials to determine if the degradation modes and acceleration factors remain the same. It would be useful to obtain field data from other applications (e.g., stationary, backup power, and materials handling) and do similar comparisons to determine how aging from the ASTs compares to aging in these nonautomotive applications. Stationary power would be particularly interesting because the lifetime requirements are so long that real-world durability tests in the laboratory are not practical.
- Additional collaborations and field data from industry are recommended.

Project # FC-017: Fuel Cells Systems Analysis

Rajesh Ahluwalia; Argonne National Laboratory

Brief Summary of Project:

The primary focus of this project is to develop a validated system model and use it to assess the design-point, part-load, and dynamic performance of automotive and stationary fuel cell systems. The model will support the U.S. Department of Energy (DOE) in setting technical targets and directing component development, establish metrics for gauging the progress of research and development projects, and provide data and specifications to DOE projects on high-volume manufacturing cost estimations.



Question 1: Approach to performing the work

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

- The general objective of the project is of great interest for helping system designers propose an adapted fuel cell system from both technical and economical point of views, and the proposed approach is in accordance with the objective.
- Identifying operational constraints of the fuel cells with regard to pressure and temperature and the impacts on cost requires the sharp focus seen in this project.
- The system modeling approach is sound. The team has responded to input from the industry and added a heat rejection constraint on the stack/system models.
- The approach based on modeling and validation is logical and appropriate for this project.
- Overall, the approach is good and in line in addressing targets that are the focus of the DOE Hydrogen and Fuel Cell Program (the Program). On a high level, the approach of developing a model that is a versatile design and analysis tool, validating the model with data from inside and outside of Argonne National Laboratory (ANL), and applying the model to the issues of current interest is consistent with accomplishing the objectives set out in the presentation. However, the approach that is used in the inner details of the model is important to assessing the potential success of the project. It would be useful to consider including supplemental slides that describe the inputs needed for the system model, the process by which it is calibrated/fitted, and the manner by which certain aspects such as stack thermal capacitance/liquid water/phase change are handled, because they may impact the analysis. These inputs may be appropriate to include in the review slides.
- The approach uses publicly disclosed materials considered to be state of the art. The principal investigator (PI) needs to make some assumptions about the compatibility of these materials in a complete system (even though it may not be completely proven or validated). The model and approach are becoming more realistic and in line with industry expectations every year. The PI works well with industry and ANL's modeling teams to ensure the model is as accurate as possible with the given information.
- This project is well designed to continually identify barriers and set targets by applying the system-level model, based on validated cell and stack submodels, to parametric and trade-off studies. Further validation against actual stacks is desirable, as opposed to the current strategy of extracting effective kinetics on 50 cm^2 cells.
- The project should analyze fuel cell system architecture itself. Currently, some original equipment manufacturers (OEMs) are trying commercialize their fuel cell vehicles; however, their system architectures are varied. For instance, one OEM applied non-external humidification to reduce the cost; this

approach seems to be quite different from the ANL assumption. The project should cover system architecture analysis; for example, it could try to determine the proper system architecture of non-ionomer catalyst-layer membrane electrode assemblies (MEAs) that are sensitive to relative humidity (RH) vs. RH-insensitive MEAs, non-humidification and dry-out mitigation strategies, etc.
- Because this is a multiyear project, it would be good to show a spider-type plot of what is covered, what has been covered in previous years, what has high versus low confidence, etc. (e.g., low temperature, freezing operation, temperature extremes). It would be helpful to establish metrics for gauging progress or orienting reviewers.
- The function of this project is important in terms of guideline and direction making. The approach is good. The impact of newly developed materials on the system is well studied; however, there is still room for improvement.
- The approach and goals are well organized and carefully adapted to the needs of fuel cell researchers and developers. Going forward, more emphasis on end-of-life (EOL) parameters should be integrated.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The PI has been consistently upgrading this model for many years. The model is updated as new materials and better overall system modeling tools emerge. The U.S. DRIVE Partnership's Fuel Cell Technical Team (FCTT) and laboratories have worked with the PI to continually improve the model to reflect more realistic systems.
- ANL had updated the model results and compared the impact of the new heat rejection constraint on the overall system cost and showed that the new constraint does not add cost at higher pressure.
- Excellent progress has been made, especially in the area of stack heat rejection.
- The project features a good study of the new fuel cell design requirement ($Q/\Delta T$).
- ANL has updated cell model kinetics to address emerging and maturing designs as well as applied a system-level model to identify cost and operational trade-offs.
- ANL has demonstrated a significant amount of work looking at a number of material configurations that are relevant to next-generation catalysts, heat rejection constraints, and optimization studies related to the system cost/catalyst metal loadings. These are all high-impact areas for fuel cell manufacturers, and a validated system model that provides guidance for optimization in these areas is highly valuable. One issue frequently faced by fuel cell manufacturers is the degree to which variability in a component or operating condition will alter the cost analysis/performance of the fuel cell system. In slide 6, the model does not quite capture the behavior of limiting current for the higher system pressures and higher temperatures. In addition, the plot of mass transfer overpotentials is under-predicted in the low-current-density region, and a similar type of variation across the current density range can also be seen in slide 15. For the system model to accurately predict cost trade-off/system optimization, it would appear to be important to assess the effect of variability in the input data for various components of the system model, in part assessing the type of tolerances that need to be in place from a manufacturer's standpoint, but also providing an understanding of the precision to which the model can assess differences between materials.
- It seems like there has been significant progress in the development of the tool, and new results have been presented. The work on Pt-loading optimization is very interesting, even if the variable parameters used are not clearly detailed. From a general point of view, critical points for model validation should be better detailed, in particular those dealing with mass transfer. Regarding the compressor, it is unfortunate that only one compressor model is studied. Including a turbo compressor could be interesting. A cost study should be completed that also considers EOL performance. A degradation study was again mentioned but not presented—it is unclear how it will be integrated. Namely, it is unclear how the thermal design will be affected when stack EOL performance is taken into account.
- The project deserves an excellent evaluation regarding the RH and the impacts on the system performance.
- So far, the progress is good. However, the researchers can update the assumptions for system architecture and components for further cost reduction.
- The team made strong progress while integrating major changes to the model, but there was no mention of the model's availability to the general public.

- It is unclear if there is model validation for the Q/ΔT work. It may be there, but it was not completely clear from the talk or slides. PtCoMn/nanostructured thin film (NSTF) has clear reference data cited from 3M. It would be nice to provide some anchor points or reference points for previous operating conditions. The previously assumed operating points are not clear, nor are the changes relative to that point(s). Regarding ORR kinetics and mass transfer, it would be good to highlight key results better; for example, on slide 18, the key result or highlight is unclear—or perhaps the researchers were just building the ingredients for the cell modeling capability for novel catalysts. Perhaps indicating explicitly what problem is being solved on these slides would be helpful. On slide 20, the conclusion/takeaway regarding the Roots air expander is unclear. The reviewer wonders if it was superior idle power.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- The PI works with Strategic Analysis, Inc. (SA) and key laboratories to build the model and provide key feedback to the partners. The PI has worked well with the FCTT to get feedback from the OEMs and implement changes where possible.
- The collaboration is very good because it has been expanded, as recommended by the previous reviews. The coordination of the project also appears to be good.
- The project features excellent collaboration with system integrators, component suppliers (cell and balance of plant [BOP]), and technical working groups.
- The project features an excellent team working on a focus area for the Fuel Cell Technologies Office.
- The researchers are keeping good communication with other DOE funding projects, OEMs, suppliers, and the FCTT.
- The project features strong input from key stakeholders, as well as great teamwork.
- The project is interfaced with numerous other institutions, including component manufacturers, automotive companies, system component developers, other model developers, and the FCTT. Overall, the interaction is excellent and a high point for the project. It would be helpful to illustrate on an early slide what each partner is specifically contributing or using from the project—this would make it easier to assess the overall level of coordination (although slide 21 does assist in providing a general guide).
- The project is well connected; it is good to see that the researchers are using the new Eaton blower data. It is unclear if there is a reason why the Japanese fuel cell stack developers were excluded.
- The project has been conducted with relevant inputs and collaboration with other organizations.
- Each study can provide important results because of good collaboration. However, although there are a lot of collaborators, there does not appear to be an integrator who can balance all of the information, including the more realistic system information.
- The collaborations seem to be well established and cover the main areas on slide 5. An industrial partnership with a company such as Eaton is a welcome development for BOP component modeling. Because BOP can be an underestimated component for fuel cell systems, it would be great to see more of this type of collaboration.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The impact of system modeling is huge and aligns well with the Program's research, development, and demonstration objectives. The impact will be even greater when the model is versatile to different components and takes into account degradation of the system with duty cycles.
- This project clearly supports the Program in achieving its goals. The cost data generated clearly helps manufacturers know what barriers that operational changes introduce.
- The potential impact with respect to continuing to help set DOE targets is high. The dissemination of the modeling tools is an important path to even greater impact.

- The project focuses on the cost aspects of the fuel cell system and uses a system engineering approach to understand the impact of stack performance and operating conditions on cost.
- The work has provided a better understanding of the impacts of certain operating parameters on stack performance.
- The results of this project have a big impact on guiding material research and subsystem development.
- This project provides a baseline for DOE-funded cost analysis.
- The model has limited use to OEMs and fuel cell system builders. Most OEMs have more detailed models that suit their particular needs. However, the model is very useful for several reasons:
    - Provides a useful tool to DOE to help establish technical targets and research and development directions.
    - Provides guidance to interested parties (mostly non-OEMs) on what the major costs and technical hurdles are on a system level.
    - Provides a useful tool to assist all U.S. DRIVE Tech Teams to communicate needs and targets.
- The project has the potential to impact the targets for the Program because it is integrating data from various component suppliers, from both within stack and within system perspectives. There is a risk associated with setting the targets without assessing the impact of noise and the quality of the fit for the data used as input, and this should be addressed. The potential impact of the project is increased by several items that are listed for validation, such as the cross-flow module with the M311.05 membrane. However, it would be further beneficial to consider a manner by which a system validation point (or series of validation points) could be included to assess the accuracy of the model predictions for the optimized systems.
- The reviewer wants to know whether GCTool/Autonomie are available or used by other non-ANL groups. The reviewer believes that Autonomie is but is not sure about GCTool or the package of GCTool/Autonomie. Making such a package publicly available or an easy-to-use interface would be valuable for the fuel cell/vehicle modeling community.
- The impact of this model will be improved if it is used to evaluate EOL trade-offs.

Question 5: Proposed future work

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

- The project features well defined and relevant future work. It will be good to see the progress and validated model for hydrogen ejectors.
- The proposed future work addresses the overall direction in which the industry and the Program are heading, namely alternative advanced alloy/de-alloyed catalysts and the importance of setting targets for the system to optimize the overall cost. Given the importance of the latter, more effort should be undertaken to include additional validation points for the predictions that are coming from the model by either assessing existing systems or working closely with a systems manufacturer. Further, the proposed future work should also assess the effect and impact of noise in the input data on the resulting conclusions related to the cost projections and heat rejection effects/constraints.
- The proposed future work is basically good. A more system-oriented study might be needed by a “real” system designer—preferably from an OEM—to indicate the guidelines and direction for material/subsystem development from a system point of view. That can result in a more realistic projection of further cost reduction opportunities. (The accuracy of cost estimation is not that significant, but the assumption of system architecture and the consisting components is important, which affects the direction and guidelines for material/subsystem development in terms of cost structure.)
- The researchers should prioritize current-day large-cost items, BOP components, and alternative MEAs. Less priority should be given to high-volume cost projections. It is unclear if it is justified to focus resources on moving from ~$57/kW to $52/kW, given that the key issue is not how to reduce the high volume number, but how to span the gap from today’s very high costs to more moderate costs and slightly higher volumes.
- The proposed work is in line with the objectives of the project. However, the degradation work was proposed last year; it really has to be done this year because it may impact the system design ($Q\Delta T$) and the associated projected cost. Degradation should not only refer to MEAs, but also to key components such as compressors and humidifiers.

- The de-alloyed catalyst and NSTF catalyst comparison is interesting. The researchers should study system architectures with these catalyst-layer differences. For example, NSTF (non-ionomer-type catalyst layer) is sensitive to RH and there is a constraint for humidification control. The implications for system architecture are worthwhile to study, as are cost reduction opportunities.
- Additional stack-level verification and validation, at least for mature models, should be included because the kinetics are parameterized based on single 50 cm^2 cells.
- Focus is strongly suggested on evaluating the effects of various design points—especially stack heat load—on system durability/degradation.
- “Durability considerations” will improve the value of this tool.
- The proposed future work sounds fine; however, it has not changed since the 2103 DOE Hydrogen and Fuel Cells Program Annual Merit Review. It is unclear how long this is going to remain as “proposed future work.” The team needs to prioritize alternate MEAs with advanced alloy catalysts, dealloyed PtNi on NSTF (3M collaboration), and dealloyed PtNi on corrosion-resistant carbon support (ANL catalyst project with Johnson-Matthey Fuel Cells Inc. and United Technologies Research Center as partners).
- The PI will continue to update the model as relevant data and information is provided.

Project strengths:

- There is a high level of collaboration with other institutions. The project addresses the coupling of the system and the stack, which is hugely important to balancing stack design aspects versus system constraints versus total cost. Other strengths include the project’s analysis of various catalyst materials, and that it directly discusses the impact of specific design constraints on overall cost and targets for cost.
- The project features a systematic analysis of a fuel cell system, which can be a strong and important guideline for material research and subsystem development. Another strength is the collaboration with various suppliers, which facilitates study of each subsystem/material cost from “realistic” information.
- A detailed modeling framework has been established, and thorough modeling capability and sensitivity analysis has been presented. Developing physical modeling capability for novel catalysts is valuable. Interaction with an industry partner for the BOP component is welcomed.
- The model is continually updated and refined as information becomes available. The PI clearly communicates his assumptions and makes the information readily available to those who are interested.
- It is a very complete model that takes into account many mechanisms involved in the operation of a fuel cell system. The parallel cost estimation with SA is appreciated.
- Project strengths include the validated system model baseline and communication with information sources.
- Strengths of the project include its detailed analysis, and the fact that it is highly focused and connected.
- The project features excellent teamwork and good fundamentals.
- Strengths of this project are its technical approach and collaboration.
- This project has proven to be adaptable and seeks input from a diverse group of stakeholders.

Project weaknesses:

- The assumptions of the analysis may be old, or too stack/MEA/material oriented. Although there are a lot of collaborators, there does not appear to be an integrator who can balance all of the information, including the more realistic system information.
- The following are more presentation critiques rather than project weaknesses: the presenter should highlight key points on slides, provide reference values from previous years, and provide better context for key problems being solved.
- ANL does not consider the impact of variability in input data (such as catalyst characterization). Additional validation of fuel cell system (FCS) outputs for various systems is needed. Another weakness is the lack of specific details related to the approach used to treat different components of the stack/FCS.
- Durability constraints have not been implemented, even though this was mentioned two years ago. Data used for the optimizations rely only on 50 cm^2 single-cell data. Stack validation is lacking.
- The components and subsystems selected may not always be compatible in real life.
- The researchers should consult the FCTT for validated $Q/\Delta T$.

- The lack of progress is an area of weakness.
- The project focuses on too many parameters.

Recommendations for additions/deletions to project scope:

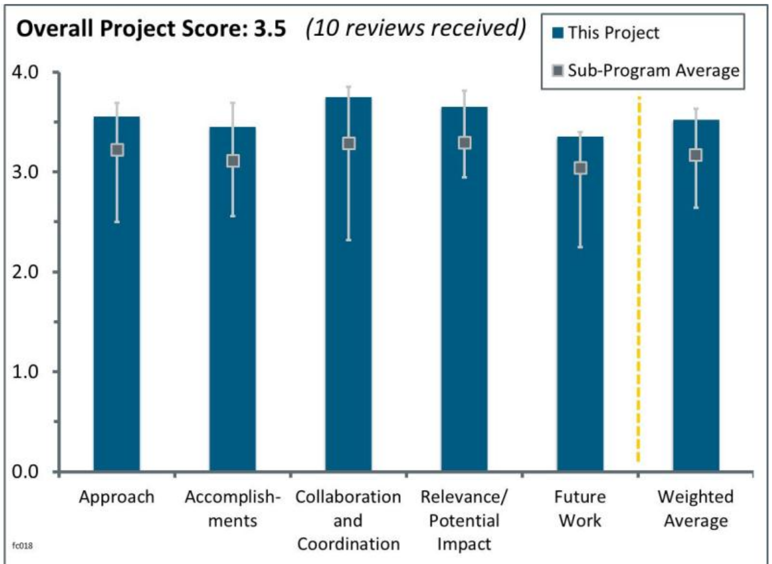
- It would be better to have one person from an OEM serve as the total system integrator. The assumptions of the current analysis may be old or too stack/MEA/material oriented. A more system-oriented study would be better; one that first considers overall system architecture and standard materials. Of course, a material-oriented study is important, but a more systematic study can be done. For example, if air pressure is reduced to around 150 kPa, the system designer will add a blower, not a compressor, to the system to reduce costs. The humidifier can be eliminated if the membrane and/or water management are improved. Therefore, the accuracy of cost estimation is not that significant, but the assumptions for system architecture are important, which affect the direction and guidelines for material/subsystem development in terms of cost structure.
- This year the PI switched to a neural model; perhaps a bit more information could be provided regarding how this change affects the outcome of the model—a bit of a sensitivity analysis of the old versus the new. He did mention that it provides a more realistic result when using data points outside of the empirically available data. The presenter should provide more information next time on where this observation was noticed most or had a key influence in the outcome.
- The project scope should be expanded to cover a system architecture study. The system architecture was not changed for the last 2–3 years, but based on technology advancements, the system architecture itself is also advancing, such as non-external humidification and passive anode recirculation (single stage). MEA technology is also advancing, including the incorporation of non-ionomer catalyst layers into RH-insensitive MEAs. This project is expected to apply technology opportunities from real-world industry as well as DOE funded projects. More information collection and benchmarking are recommended.
- The team should pursue additional validation points for the FCS outputs on a system-/stack-level. They should include, for example, analysis of the effect of experimental variability/component manufacturing variability on the FCS output and predictions for cost.
- ANL needs to consider moving this model to an open-source-code framework. The team needs to collect data in a cell (National Institute of Standards and Technology/General Motors) with coolant ΔT and validate the data against the model.
- The researchers should focus on incorporating degradation parameters and EOL trade-off studies related to stack and system components.
- The model should be validated against stack data.
- The reviewer questions the general motivation of this work—not about the modeling work in and of itself, but rather for DOE management. This work seems to be optimizing costs around the cost target of $50/kW for fuel cell electric vehicles, but today’s challenge is not how and whether the system can be optimized from $50/kW (+/- $10/kW), but how to move from a $70,000–$100,000 vehicle cost with high stack costs and durability and manufacturability issues to a $20,000–30,000 vehicle cost. The way to model durability was brought up and the lack of data was mentioned. It would be good to see more programmatic focus on how to bridge the large-scale cost chasm in real and practical ways (e.g., how to move from >$1,000/kW stacks to $500 to $200/kW), which may include detailed modeling. Modeling the dynamics of optimized fuel cell systems is clearly important, as are the kinetics and mass transport of novel catalyst materials, but the Program should strongly consider cost limiters and pinch points that exist today.

Project # FC-018: Fuel Cell Transportation Cost Analysis

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:

Strategic Analysis, Inc. (SA) updates its cost analysis of automobile and bus fuel cell systems annually and explores alternative subsystem configurations. The cost analysis is used to assess the practicality of proposed power systems, determine key cost drivers, and provide insight for direction of research and development priorities. The project team completed the 2013 Automotive & Bus Power Systems Cost Report and analyzed two new subsystems: the Johnson-Matthey Fuel Cells Inc. (JMFC)-style dispersed PtNi-on-C catalyst and the Eaton-style twin-lobe air compressor-expander.



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- The approach has been vetted continually by the U.S. Department of Energy (DOE), the U.S. DRIVE Partnership's Fuel Cell Technical Team, and many individual organizations and companies. Adjustments are made based on feedback.
- This project conducts a thorough cost analysis of automotive and bus fuel cell systems using well-defined component, stack, and balance-of-plant (BOP) cost assumptions at various production volumes. The revised cost analysis using a Pt cost of $1,500/troy ounce is more realistic. The vetting of stack and BOP cost assumptions by industry throughout the project is an important part of the project.
- The project is well designed and well integrated with the systems analysis work at Argonne National Laboratory (ANL).
- The project features a rigorous approach with sharply focused objectives.
- Generally the approach is very good, but more emphasis on alternative systems and technologies would help the community to understand what system options might make the most sense from a cost perspective, and what technology improvements might be most impactful. Some examples would include the following:
    - Low-pressure polymer electrolyte membrane fuel cell (PEMFC) systems (i.e., no air compressor, just an air blower).
    - Transportation systems with different degrees of hybridization and fuel cell sizes (e.g., an 80 kW bus system [or even smaller]).
    - Tornado plots that include the most impactful parameters (i.e., the most expensive components), such as the membrane, which is not included on the plots on slide 34. The membrane is more expensive than all of the other materials listed other than Pt (e.g., more expensive per square meter than the plates, EPTFE, and gas diffusion layers [GDLs]).
- Changing to W. L. Gore & Associates (Gore) membrane electrode assemblies (MEAs) and Pt-Ni-C catalysts improves the believability of these studies. In terms of examining the cost of support Pt, it is unclear if that can be evaluated by looking at cat-convertor technologies.
- The production capacities spacing looks uneven (systems/year). The steps are currently 900% increase, 200%, 167%, 25%, and 400%. The researchers may want to consider something a bit smoother past the 10,000 systems. For example; 30,000; 70,000; 200,000; and 500,000 systems/year. As it stands, the difference between 80,000 and 100,000 systems/year is not as important as the difference between 100,000

and 500,000 systems/year. A twin-lobe compressor is a more adequate choice for the application over centrifugal—it is good to see the update. The reviewer is concerned about back-leak of this compressor style, especially at low flow rates, as well as noise levels. The technology may have come along significantly, but the reviewer's personal experience with compressors from 15 years ago was of low efficiency and extreme noise. The team accomplished great work on the catalyst processing costing. It was unclear if the peak stack temperature is the average temperature of the stack. It was also unclear if the peak stack temperature is the hottest point of a cathode catalyst, and if not, what is.

- This has been a good and consistent continuation of the costing effort.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- Important accomplishments include updating the cost/volume numbers for buses, imposing the Q/ΔT limit, and adding a plate-frame membrane humidifier to BOP.
- The project is accomplishing project goals, and the research team has creatively identified pathways to a greater understanding of the cost of PEMFC systems.
- Incorporating Q/ΔT, Gore MEAs, and carbon-supported catalysts are all good improvements made in fiscal year 2014.
- This was a great update—this type of consistent multiyear analysis is very useful for other DOE Hydrogen and Fuel Cells Program (the Program) technology development guidance.
- The annual update was performed successfully. Changes to inputs were executed as necessary.
- The accomplishments give a good indication of the cost to be expected.
- Although the cost projections went up (bad!), the justifications provided for these increases were valid and reasonable. However, it would have been good to see more progress on alternatives that could also help the cost projections, such as the use of dispersed Pt/C catalyst layers (instead of nanostructured thin film [NSTF]), because these are presumably lower cost, and the evaluation of low-cost molded-carbon-composite bipolar plates.
- The project team continues to do an excellent job in refining and updating the input parameters and assumptions. In regard to the catalyst, an explanation of the benefit of a dealloyed PtNi versus an NSTF PtCoMn system would be helpful. If the PtNi catalyst enables lower Pt loading, it is unclear how this would impact the current Pt loading assumption of 0.153 mg/cm^2 of Pt. There appears to be a significant decrease in the power density from 692 to 641 mW/cm^2. It is not clear why operating at a lower power density would result in a lower system cost.
- The components design and fabrication process for bus application should select those that have low cost at low fabrication volume. Current analysis uses the same process for automobiles and buses; this is not suitable, because their production volumes are different.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.8** for its collaboration and coordination.

- The national laboratories (i.e., ANL and the National Renewable Energy Laboratory) provide crucial support for model input. The collaboration with the different industries (i.e., stack, stack component, and BOP) is crucial in vetting the cost analysis results and assumptions.
- SA collaborates with all types of organizations and stakeholders. SA's excellent integrity and reputation facilitates collaboration.
- Collaborations are good; the way to make them better is to get real input from the automotive original equipment manufacturers (OEMs), which will be difficult to obtain.
- The feedback loops via industry and national laboratory collaboration seem to be well orchestrated.
- The collaboration with many relevant stakeholders (e.g., OEMs and institutes) supports the viability of the results.
- The project has an extended list of collaborators.

- 3M is not listed as a collaborator; hopefully this is an oversight. 3M is a leader in membranes and manufacturing processes for unitized MEAs (i.e., GDL + catalyst coated membrane + seals). 3M also provides NSTF (which is less important because it is not technically viable yet.)

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- This is absolutely critical analysis for the Program, especially at a time when investors would like more eyes on the potential cost of fuel cell electric vehicles—one of the most important factors in market adoption.
- The project is an independent study of fuel cell system costs for the transportation markets. It fully supports the Program objectives.
- SA's cost model is a bedrock of Fuel Cell Technologies Office (FCTO) program planning, helping to identify high-cost areas.
- The project gives a clear indication of expected cost.
- The project is critical to the Program because it projects the cost for PEMFC systems at a production level consistent with industry demands. The validation of the approach needs to be continually reviewed by DOE to ensure the quality of the price projections.
- This project is required in some form for DOE to understand where investments are required and how to best use its funds. However, the researchers should also evaluate what effect DOE can have in those areas compared to OEMs.
- This project would have much greater impact if certain recommendations were followed. The reviewer would also expect more progress per year than what was delivered this year.

Question 5: Proposed future work

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

- The proposed future work is a logical and consistent continuation of work.
- The beauty of this work is its consistency from year to year.
- The "detailed cost analysis" on high-impact BOP components should be compared in terms of high-volume manufacturing and automotive components (e.g., fuel injectors). When looking at the complexity of a fuel injector, it is hard to understand why they are not more expensive.
- Going forward, the team should prioritize validation of the stack model against experimental results and optimizing operating conditions for both car and bus applications, as well as analyzing high-impact BOP components.
- The researchers should proceed with the plan to add a detailed cost study on high-impact BOP components.
- Production quality control should be better defined and updated.
- There seems to be a difference between SA's cost projections and those developed by others. Future work should address this difference and account for it in terms of different cost definition, assumptions, methodologies, etc.
- BOP has become more important, if not equal, in terms of cost than the fuel cell stack, but it remains the last analysis on the list.

Project strengths:

- This has been a well-executed project that helps evaluate the state of the technology for DOE guidance purposes. It was good to examine the cost based on a more traditional catalyst (i.e., Pt-Ni-C) and Gore MEAs. That provides a more realistic expectation of where the Program stands in terms of automotive fuel cell cost.
- The experience of the analysis team and its ability to access and analyze component and system information are the greatest strengths for this project.

- The project features a comprehensive approach with well-planned feedback from critical industry partners.
- This project has very good collaboration with OEMs and relevant institutes.
- The project carries out very rigorous, detailed, and technically supported calculations.
- This project strives to use a valid model of the systems and also validate costs with industry.
- The project provides a fuel cell cost breakdown and directions for future further cost reduction.
- The strength in this analysis is the collaboration with industry to vet the assumptions and results.
- The project's flexibility and integrity are areas of strength.

Project weaknesses:

- The project does not do enough to make a connection between its cost estimates and the cost of actual fuel cell manufacturers. There is only one comparison made—that of the bus fuel cell projection and a general statement from a fuel cell manufacturer that its estimates are in the same neighborhood. The community would like to see SA's cost estimates extended to lower manufacturing volumes and compared with real-world actual fuel cell prices.
- The costing approach for the bus power systems is mainly a simple expansion of the automotive work; for example, it used the same MEA. It is very unlikely that the MEAs for the two applications will be the same, specifically in terms of membrane and catalyst loading. The hybridization for buses will also be significantly different.
- The project team has spent several years on the cell stack, with only a small portion of its time on BOP and systems. The effort should increase its focus on BOP.
- 3M and JMFC catalyst layers need further validation with respect to stack performance and durability.
- The progress over the past year was not too impressive.
- The researchers did not separate the high-volume automobile and low-volume bus applications analyses.
- There are no detectable weaknesses.

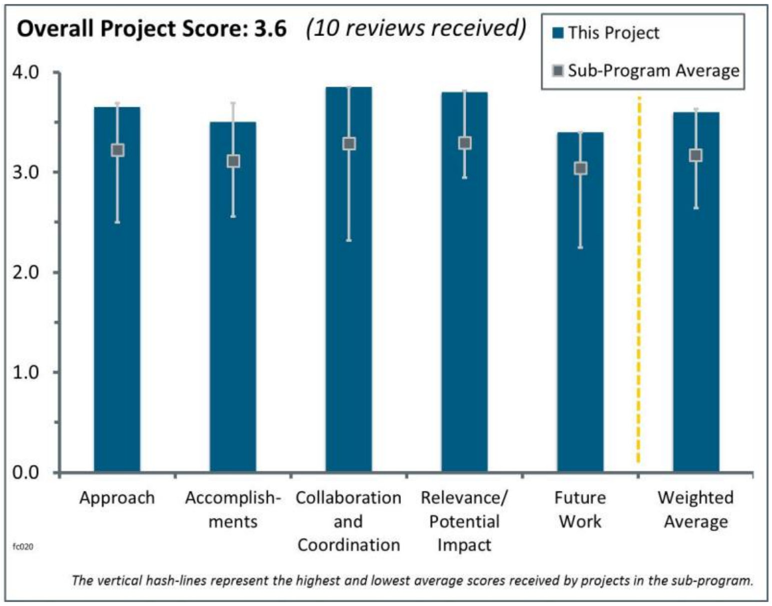
Recommendations for additions/deletions to project scope:

- The project should continue as proposed.
- It is probably time to just leave NSTF out of cost studies related to current cost estimates. The researchers should get guidance on the compressor work from the automotive OEMs in terms of what turbochargers cost. A valuable expansion of this project would be to examine the smaller portable power fuel cell applications, as well as low-temperature stationary fuel cells, to determine if/as those start to be commercialized in the thousands of systems/year range, whether this could increase the speed of fuel cell technology commercialization in the transportation sector.
- The automotive fuel cell cost curve seems to be leveling off at $\$40/kW_{net}$, compared to the target of $\$40/kW_{net}$. A waterfall chart could be instructive in identifying how to get to a lower cost curve.
- Cost analysis should present the trade-off of system versus MEA materials to achieve lifetime durability requirements.
- The project (and the Program) should focus on reporting cost estimates at the 100,000 systems/year level, rather than 500,000, to be consistent across DOE Office of Energy Efficiency and Renewable Energy programs.
- The reviewer wants to know if it is possible to analyze the limit of cost reduction based on foreseeable future technologies.
- The project should reduce emphasis on cathode catalysts and increase emphasis on BOP.

Project # FC-020: Characterization of Fuel Cell Materials

Karren More; Oak Ridge National Laboratory

Brief Summary of Project:



Research objectives for this project are to: (1) identify, develop, and optimize novel high-resolution imaging and compositional/chemical analysis techniques and unique specimen preparation methodologies for the µm-to-Å scale characterization of material constituents comprising fuel cells; (2) understand fundamental relationships between the material constituents within fuel cell membrane electrode assemblies (MEAs) and correlate these data with stability and performance; (3) integrate microstructural characterization within other U.S. Department of Energy (DOE) projects; (4) apply advanced analytical and imaging techniques for the evaluation of microstructural and microchemical changes to elucidate microstructure-related degradation mechanisms contributing to fuel cell performance loss; and (5) make capabilities and expertise available to fuel cell researchers outside of Oak Ridge National Laboratory (ORNL).

Question 1: Approach to performing the work

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

- This project uses electron microscopy for the characterization of fuel cell materials. It has two main missions: (1) developing methods and procedures in electron microscopy for fundamental studies on fuel cell microstructure and nanostructure (e.g., to better understand mechanisms of material degradation), and (2) to be a resource to the fuel cell research community in applying these methods and procedures in support of other research projects. Normally, a rating for "approach" would be based on which technical means are being employed to answer a given question or achieve a desired result; here, this project is bounded by electron microscopy. However, the tools and the techniques it enables are so clearly essential that it is an outstanding "approach" to have a project focused on advancing such crosscutting metrology capabilities.
- This project develops novel microscopic techniques and methodologies to fundamentally understand nanoscale materials interaction within the MEA—an important aspect in understanding fuel cell performance and degradation. The support provided to other DOE project teams and industry is excellent.
- The work is effectively applying state-of-the-art electron microscopy techniques to the characterization of MEA material constituents from µm-to-Å scale. It is also utilizing advanced analytical and imaging techniques in the evaluation of microstructural and microchemical changes to elucidate microstructure-related degradation mechanisms contributing to fuel cell performance loss. This degradation effort is a very important activity.
- The proposed approach is appropriate in the way that it is fully collaborative in nature and aims to benefit the entire fuel cell community in order to have a deep understanding of materials evolution in the MEA components. The project share with 25% for "open research" is also appropriate to allow for the evolution of more instrumental investigations that will be possibly applied to the main projects' issues and, in particular, the materials degradation.
- This ongoing characterization project supports other DOE projects and provides the fuel cell community with access to state-of-the-art microscopic capabilities. Understanding fundamental relationships between

microstructure over the range scales and performance is of the most importance to any progress in the commercialization of fuel cells.

- The investigators continue to listen to their partners and are developing methods that are in line with the critical needs of fuel cell research. This project seems to balance its resources between new method development and supporting requests very well.
- The use of advanced microscopy to examine the MEA, catalyst, and ionomer in the electrode layer will enable the design of new fuel cell components that will have higher performance and durability and lower cost, thereby meeting the three goals of the fuel cell program
- The work has been focused on addressing durability and performance—two key barriers to fuel cell commercialization. The work is integrated well with other efforts. This group provides electron microscopy services and expertise to numerous DOE Fuel Cell Technologies Office (FCTO)-funded projects. The microscopy work at ORNL has been critical to evaluating carbon corrosion and catalyst-layer degradation/densification during aging. Recent work has focused on developing techniques to characterize the ionomer in the catalyst layer, an area about which little is known but may have a large impact on performance and durability.
- Currently, mass transport and durability (retention of high oxygen reduction reaction [ORR] activity) is one of the most important technical focus areas. The project is trying to establish ionomer and catalyst dispersion in the catalyst layer in high-resolution microscopic/spectroscopic approaches.
- This project does not have specific milestones; instead, the focus is to test and analyze new materials as they become available from the project partners.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- This is clearly a productive project, and the presentation was rich with results from the past year. Slide 4 groups various projects into the broad headings of component durability, novel catalysts and supports, and ionomer studies, with much of the presentation given to discussion of the latter (a theme this year in response to reviewer comments from last year). Here, the DOE goals are not system specifications, but rather the metrological goals of being able to provide quantitative diagnostics on the composition, distribution, and degradation of ionomer coatings. This project develops a unique source of information from which rational progress toward the system specifications may be achieved.
- The team has made excellent progress with characterizing the ionomer layer. The practical limitations related to sample preparation should be discussed in the Program's Annual Merit Review forum. The applicability of these methods to degraded materials is critical, and the limitations related to the microscopy methods discussed are an important component toward an evaluation of progress.
- The project accomplished a great deal; imaging soft ionomers in the presence of metal catalyst particles and the initiation of in situ studies are technologically extremely challenging, but these efforts have been accomplished in this project. A great deal of data was collected on relevant catalysts and electrode structures for project partners.
- The project has achieved excellent outcomes for ionomer dispersion analysis with energy dispersive X-ray spectroscopy (EDS) maps to support catalyst-layer study for the enhancement of mass transport and ORR activity.
- The project contributes significantly to a fundamental understanding of the degradation mechanism. Carbon corrosion is utterly important to state-of-the-art fuel cell catalysts. Achievements in in situ electrochemical microscopy will present new, invaluable insight into electrode-layer structure and degradation. The high-resolution EDS capability adds very important chemical information to the transmission electron microscopy's (TEM's) excellent morphological information. The progress in the imaging of ionomers is of great importance. It is not clear whether the conditions for imaging ionomers in TEM (i.e., cooling and low electron doses) will be transferable to in situ electrochemical microscopy.
- Three main points have been pointed out this year:
    - Nafion® characterization by TEM: This is difficult to observe because of Nafion degradation under the beam. Therefore, the operating conditions have been successfully adapted by using electron energy loss spectroscopy (EELS) and EDS. The quantification of the profile obtained at

low electron dosage seems correct even though the proposed interpretation might need to be confirmed due to the level of noise. Mapping at higher electron dosage is nice, but the proposed conclusion about the ionomer instability is not convincing. Actually, because the aging test also degrades carbon, evolution in the ionomer distribution is normal and a conclusion cannot be drawn on its stability.

  - Carbon corrosion analysis: The given conclusions are that agglomerated particles enhance carbon corrosion and a better dispersion is beneficial. These are answers to a recommendation from last year. The presented pictures dealing with agglomerates are not really convincing. The effect of a better Pt distribution on electrochemical surface area (ECSA) seems important, but the associated pictures have not been shown. The reviewer asks if they were confidential. The optimized catalyst on low surface area carbon (LSAC) degrades less during carbon corrosion accelerated stress tests (ASTs), but it is unclear regarding catalyst degradation ASTs involving the Ostwald mechanism.
  - In situ observations: These are the first results presenting catalyst evolution in in situ TEM. Congratulations! The resolution limitation is unclear. It is also unclear whether the observation in a liquid medium would enable study of the ink distribution (solvent effect).
- Researchers have made progress in characterizing ionomers in the catalyst layer. ORNL has developed techniques to minimize sample damage during analysis. The group continues to provide valuable data to the fuel cell community. The in situ electrochemical microscopy has been slow in development and has not yet demonstrated its potential in fuel cell studies.
- The work to understand ionomer damage by the electron beam is very important. It seems that there is still some F loss under LN_2 conditions, so the question arises whether this methodology can be used as a quantitative measure for ionomer degradation. Furthermore, it would be interesting to map out S to understand its location in reference to the Pt particles as well; changes with degradation could give an indication of the local loss in ionic conductivity in the catalyst layer. In regard to the in situ TEM, this reviewer's understanding is that the electrolyte has to be drained before imaging, so changes in the electrode component composition and structure will not be captured in real time and thus will be difficult to correlate with in situ fuel cell results.
- This project is an ongoing project. The principal investigator (PI) uses in situ microscopy to analyze catalyst dispersion, compacting compression effects in cathode layers, ionomer distribution quantification within the catalyst layer, etc. It is up to the partners to use the information from the results to improve materials and subsystems.
- ORNL completed the parametric study of ionomer thin films with General Motors (GM) and collaboratively published the results. This study establishes baseline conditions to quantitatively assess ionomer structure and composition in an electron microscope.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.9** for its collaboration and coordination.

- This project features excellent, extensive collaborations with the following: Los Alamos National Laboratory; General Motors; 3M; Automotive Fuel Cell Cooperation; Ballard; Nissan Technical Center North America; Ford Motor Company; the University of Tennessee; Argonne National Laboratory; CEA-Grenoble, France; IRD Fuel Cells; Fuel Cell Energy; and McMaster University, Canada.
- Collaborations are numerous and of high quality. It is good to establish new collaborations, even international ones, to provide access to unique imaging/analysis (i.e., microscopy) capabilities in order to more quickly meet the project targets.
- This project is highly collaborative, with service to the fuel cell community being one of its main missions. Slides 2 and 4 list many of the major partnerships.
- This project is a model of excellent international collaboration, due to the work with CEA (France) on EDS map microscopic analysis on ionomer dispersion.
- The approach of the project is to collaborate with industry, academia, and national laboratories on their needs and problems and to make state-of-the-art microscopic capabilities available to collaborators.
- This project features good collaboration with both national laboratories and industry. This group is utilized by many FCTO-funded projects.

- A large number of collaborators from industry, national laboratories, and universities are associated with the project and cover the essential stakeholders.
- The PI collaborates with key original equipment manufacturers (OEMs) and academia. There is no need for more partners to ensure the quality of work/progress, etc. The PI should be open to providing other OEMs with her tools as needed.
- The project features a very comprehensive set of national laboratory and industry collaborators. However, there are no university-led teams, so there may be an issue with imaging next-generation materials that come out of more basic science projects as they become incorporated into the more applied DOE Office of Energy Efficiency and Renewable Energy mission.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- This project is highly relevant for achieving the FCTO Multi-Year Research, Development, and Demonstration Plan (MYRDDP) goals and objectives of better fuel cell performance, greater fuel cell durability, and lower fuel cell costs. On the cost front, microscopy is essential for understanding the behavior of catalysts and how catalyst loading might be reduced. On durability, a central theme of this project is to diagnose and better understand degradation mechanisms at the microscale and nanoscale (i.e., atomic scale). This project is essential to understanding the relationship between materials synthesis, microstructure, and performance.
- Understanding fundamental relationships between microstructure over the range scales and performance is of the utmost importance to any progress in the commercialization of fuel cells, which is directly relevant to the Program's goals and objectives. The relationship between the structure of electrode layer components, the chemical and morphological changes occurring during fuel cell operation, and its durability is key in advancing durable materials and devices.
- This project is highly relevant to the Program's goals because it is essential to image catalyst, electrodes, and MEAs at the length scales only accessible to advanced microscopy so that these can be validated and the changes occurring during degradation can be quantified.
- The project aligns well with DOE objectives. It supports a number of DOE projects and industry by looking at the microstructure and nanostructure of MEA components, which is crucial to understanding fuel cell performance and degradation.
- The project aligns well with the Program's objectives and has the potential to advance progress toward the Program's goals and objectives. A new technique has been developed for understanding a variety of problems in the areas of MEA synthesis, microstructure, and performance.
- This tool represents ideal collaboration between the national laboratories and industry. This is a good example of a tool that is not available to most industry partners yet still provides valuable insight into material and system performance that will allow the OEMs to improve their designs.
- Microscopy expertise and state-of-the-art capabilities are important to identifying materials degradation mechanisms, which are critical for improving materials structure for enhancing stability and performance. The team did a very good job of communicating the results through numerous paper publications. This increases the impact of the project to the entire fuel cell community.
- This project is critical to understanding fuel cell materials structure and how that structure evolves with usage.
- Relevance: This project remains focused on the needs of fuel cell developers. Impact: The lead time for a partner to get data was not discussed. Short lead times will drastically improve the impact as these new methods become available.

Question 5: Proposed future work

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

- The ionomer work on the understanding of ionomer distribution within the catalyst layer and ionomer degradation with aging is important work that needs to be correlated with experimental data from fuel cell testing. It is not clear that the in situ tool will provide the desired insight into structural and compositional changes in the catalyst layer.
- The proposed future work is good and logical. Now that proper imaging and analysis conditions for studying ionomers are in place, the plan is to expand ionomer studies to specifically focus on aging effects as well as electrode/membrane interfaces.
- Continued focus on the ionomer layer is a great plan. Statistical descriptions of ionomer coverage and thickness are highly desired.
- The extension of the work to understand the interaction of the ionomer will be very important and will shed much-needed new light on electrode functionality and degradation. The in situ catalyst studies, if they can be shown to be fuel cell relevant, will be extremely beneficial to catalyst development.
- The project's future work will continue to be motivated by challenges from the U.S. DRIVE Partnership's Fuel Cell Technical Team. The immediate future work, as laid out on slide 24, is appropriate.
- The future work is in line with current progress and deficiencies.
- The proposed future work is appropriate. Because the analysis technique is now mastered, studies on ionomer degradation should go on (e.g., with evolution of ionomer profile and distribution around C). The current studies do not answer the question of how ionomer degrades if it degrades. Catalyst degradation ASTs involving the Ostwald mechanism should complete carbon corrosion ASTs for the optimized catalyst on LSAC.
- It is recommended to implement EDS mapping equipment to establish the capability for ionomer dispersion microscopic analysis domestically.
- The researchers should continue to analyze new materials as they become available.
- A large portion of the future work appears to be focused on ionomer characterization issues, which are a major need for the fuel cell community. The in situ work appears to be aimed at solution work, or rotating disk electrode (RDE)-type conditions, which are quite different than actual fuel cell conditions.

Project strengths:

- Areas of strength include the relevance and high quality of the involved partners developing advanced characterization techniques to better understand degradation mechanisms at the materials level. The consortium is open to new collaborations if suitable for the project and disseminates the achieved results to the fuel cell community.
- The project provides a strong analytical tool to OEMs that otherwise is not available to most industry partners.
- The project's strength is the development of core competency in the area of microscopy and the crucial support to other DOE projects and industry.
- A strength of the project is its ability to use high-resolution microscopic and spectroscopic analysis to support fuel cell research. It has the components and facility to support fuel cell research.
- Strengths of the project include its development of crosscutting metrological techniques and its collaborations.
- The project offers outstanding characterization capabilities and expertise in the fuel cell materials.
- The project features advanced microscopy and a world-leading, state-of-the-art facility.
- The project has good collaborations and provides a valuable service to the fuel cell community.
- The project reacts quickly to the needs of fuel cell researchers.
- ORNL has the capability to accomplish this work.

Project weaknesses:

- This project has no weaknesses.
- No weaknesses have been identified.
- The project team needs to do in situ catalyst testing in the ionomer to be fuel cell relevant; RDE studies are limited to benchmarking because the catalyst in a fuel cell—especially next-generation MEAs operating at drier conditions—will be more influenced by the ionomer, making aqueous electrochemistry less relevant.
- The project is not focused enough on aged MEA in real operating conditions.
- Some measurements and projects rely on facilities found overseas.
- The project relies on foreign capability for ionomer analysis (EDS mapping).
- The capability for ionomer characterization at a high resolution is not available at ORNL.
- The project may lack in-depth focus in some areas with such a broad mandate.

Recommendations for additions/deletions to project scope:

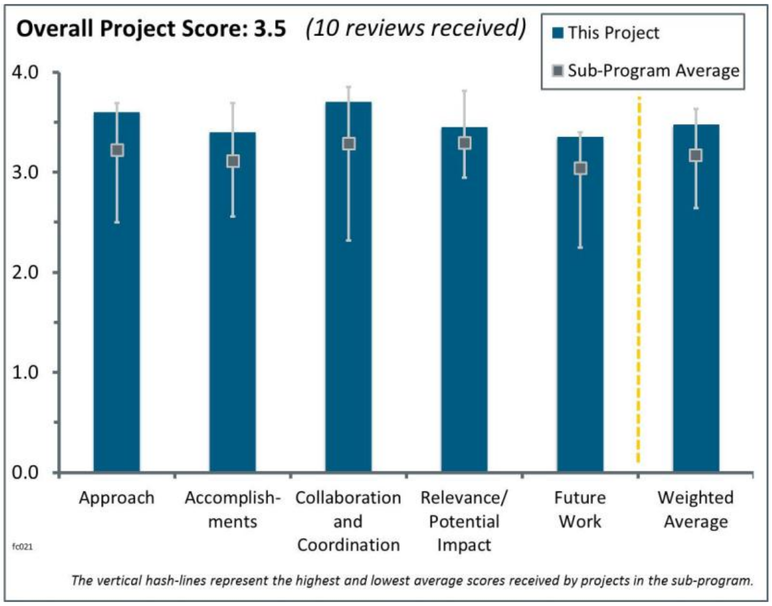
- More of the same would be excellent.
- The researchers should conduct analysis on aged components in real operating conditions, allowing them to validate the mechanisms deduced from ASTs. The team should also conduct analysis on bimetallic catalysts. These could be investigated in the following year.
- Acquisition of high-resolution EDS mapping is of critical importance and should be encouraged by DOE.
- It is recommended to budget for implementation of EDS mapping to establish an ionomer dispersion analysis capability at ORNL.
- The researchers should determine the distribution and content of S within the catalyst layer and the changes with degradation.

Project # FC-021: Neutron Imaging Study of the Water Transport in Operating Fuel Cells

David Jacobson; National Institute of Standards and Technology

Brief Summary of Project:

Neutron imaging of water in fuel cells allows the study of dynamic water transport in the flow fields and manifolds, liquid water distribution in the anode versus in the cathode, cold start and freeze-thaw effects, and catalyst degradation induced by liquid water. Objectives of this project include: (1) studying water transport in single cells and stacks, (2) enabling the fuel cell community to utilize state-of-the-art neutron imaging capabilities to study water transport phenomena, (3) tailoring neutron imaging to the needs of the fuel cell community, and (4) improving the spatial resolution to provide more detail of the water content in commercial membrane electrode assemblies (MEAs).



Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- The team has developed a very effective approach—continual improvement of the characterization techniques and testing infrastructure, while allowing a substantial amount of time for user access to benefit the community at large.
- This team has done very well at pushing back boundaries and drawing on advances and expertise in other fields to further neutron imaging capabilities. The project features an excellent balance of instrumentation, technique, and data processing development, which benefits the entire fuel cell community.
- Using neutron imaging to identify where the water is and the dynamics of water flow in the MEA and throughout the stack is crucial to overcoming the barriers of increased fuel cell performance, durability, and water management in the stack. This activity is well integrated with other activities that are developing new fuel cell components and with stack studies.
- The principal investigator (PI) continues to improve the resolution and response time of the neutron imaging facility for detecting water in fuel cells.
- The National Institute of Standards and Technology (NIST) has an excellent tool for understanding and addressing water management in fuel cells. The tool and technique are well suited for addressing one of the critical barriers for fuel cell operation.
- The approach is clear and solid. Integration of the team with the fuel cell developers is at a high level. Resolution challenges were overlooked from the beginning of the project; the team is now trying to catch up.
- Neutron imaging work performed at NIST addresses fuel cell performance, which is directly related to the cost barrier. NIST work to improve resolution addresses one of the key barriers for neutron imaging of MEAs and, if successful, will allow one to differentiate the catalyst layer from the membrane and the gas diffusion layer (GDL). This will improve understanding of flooding and should enable improved performance. Several approaches to improve resolution are being pursued, which will reduce risk (grating approach, lens approach, and increasing detector efficiency, which will help both approaches).
- Currently, neutron imaging is one of the primary methods for determining in situ water content. The facility has provided access to many different groups, ranging from academia to industry, and from open to fee-

based private research. The analysis to date has successfully resolved the water content under various conditions and scales. One open question is the importance of understanding the distribution of water inside the MEA, particularly the catalyst layer. The extension of the facility's capability to image water with a resolution approaching 1 micron (μ) is very important, and the approach is aligned with the needs of the user base. It would be beneficial to include an overview slide in the Approach section that discusses how this advancement (1 μ resolution) is being targeted. In one approach slide (slide 6), the various test stands are mentioned along with the available reactant gases; it was not clear what aspect these test stands represent in regard to the approach for the application/development of neutron imaging related to the fuel cell community. If the focus was the development of the large-scale test stand, it may be more appropriate to focus on the approach of constructing it and the user need.

- Neutron imaging continues to be a critical technique for evaluating in situ water content in fuel cells. The approach to obtain higher resolution is good and necessary for this technique to go to the next level. Results for the overall project in terms of Fuel Cell Technologies Office (FCTO) milestones are heavily dependent on users, which might limit the applicability if the experiments are not designed correctly.
- This is a useful tool, but limited resolution has somewhat limited the value of the results generated to date. If resolution and acquisition time can continue to be improved, then this work should continue.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The progress made this year is truly outstanding; NIST achieved impressive spatial resolution in water imaging as well as sped up the time frame over which measurements can be made. The infrastructure of the facility has also been improved. The community looks forward to seeing these dramatic improvements being utilized in the future.
- The project features excellent capability and data to date. The limitations are understood and there are plans to address these; however, any work and effort that could expedite the higher resolution capability will help advance fuel cell development faster.
- NIST has enabled enough user access to support multiple presentations and publications, while improving the spatial resolution.
- The team continues to advance the time and resolution limits of this important technique for imaging water transport in an operating fuel cell.
- The progress on improving resolution has been excellent over the past year.
- The two primary aspects for the accomplishments related to the improved resolution detector were items (1) and (4) on slide 7. In regard to Milestone 1, the use of an MEA is being used as a standard to assess the difference between the sub-10 μ and the 25 μ detector. Given that the actual water content of the MEA is not known and that this method is meant to determine it, it would be better if an engineered porous test sample that was more controlled was chosen for purposes of comparison. While slide 12 clearly shows differences, it is not possible to ascertain if the data collected from the new detector is an accurate depiction of the real distribution. On slide 13, the image analysis and correction points to the importance of the uncertainty that can arise in the water content measurement. With the use of a control sample, it would be ideal to establish an estimate of this error and, in addition, with the improved image analysis and corrections, determine the overall improvement in accuracy for data collected prior to the improvement. It is unclear if users should be reanalyzing past data to reassess. On slide 14, Milestone 4 relates to the desire to study drive cycles, accelerated stress tests (ASTs), etc. The improvement in the image time versus a drive cycle or AST cycle is of key interest because water management has been shown to affect durability and the hysteresis effects in polarization will affect system considerations. It would be useful to cast the acquisition time in the context of the period of an AST or drive cycle and comment on where gains in the image time are expected to assist in these areas (e.g., cost saving for manpower or additional study capability to analyze dynamic load cycling). The use of a series of standardized engineering porous media test samples may be ideal in the development or confirmation of the new detectors and the improved resolution compared to the current ones.
- NIST has made good progress in increasing resolution; NIST has increased resolution to <10 μ, though signal to noise must be improved and time resolution is poor. It is unclear whether the low time resolution

for the grating method for increasing spatial resolution may lead to a high signal-to-noise ratio, because performance variations over the 15 hours needed for the <10 µ resolution may lead to significant variations. The four-fold improvement in image acquisition time that is to be implemented by the start of fiscal year 2015 is a big improvement.
- The accomplishments toward higher resolution are good, although still not substantially improved compared to previous years. The corrections for systematic errors are good and worthwhile, and having that integrated into the software is also a good improvement. The use of standardized hardware is also good to see. Progress was minimized due to the government shutdown. Overall, the accomplishments are on the right track, but specific accomplishments this year are slow.
- The focus this year is on improving system resolution to achieve 1 µ resolution. This requires improved gating techniques, increased flux (using focusing lens versus shutters, etc.). It is unclear how much resolution is required to achieve usable results for most fuel cell applications, and whether the extra detail will allow more in-depth analysis of the fuel cell operation.
- The milestone for high-resolution MEA water content to 1 µ is indicated at 100% complete, although it was eventually shifted to the newly formed milestone on improving high-resolution time, which is 50% achieved. At the same time, the freeze management milestone reported last year at 75% was not mentioned at all.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.7** for its collaboration and coordination.

- The NIST facility is available to all, and NIST has collaborated with quite a few fuel cell researchers. Additionally, the fact that NIST provides substantial funding makes this a good "bargain" for the DOE Hydrogen and Fuel Cells Program (the Program). It is a good example of excellent collaboration between multiple government agencies.
- The access/ability to access the neutron imaging facility is commendable. In addition, the list of users is a testament to the need to image and understand liquid water and its distribution for fuel cells from both performance and durability perspectives. The results demonstrating the application of the facility are always a high point in the presentation.
- The collaborations with universities, national laboratories, and industry are impressive and show that this is a true user facility that is being utilized by all relevant stakeholders.
- The PI is collaborating with the necessary partners to make this tool available and useful to partners who need it. The work on setting up the beam is very specific to the knowledge, skill set, and resources of the PI.
- The project features good collaboration with a large number of stakeholders including universities, national laboratories, and industry.
- By its nature, this project is highly collaborative because it provides infrastructure to support other teams.
- The project features great collaboration and access to industry partners.
- There is a long list of collaborators, both in terms of advancing the instrument capability and solving real-life fuel cell problems, with various users from industry, academia, and national laboratories. It would have been good to see one more example from the user program highlighting another interaction with industry.
- There is a very high level of collaboration, including with major original equipment manufacturers (OEMs) and researchers. It would be good to see a breakdown in terms of researchers and their beamtime (e.g., percentage for OEMs, laboratories, academia).
- Ongoing collaboration with national laboratories and universities is significant and recurring; however, there was no mention of industrial partners returning to use the facility more than one time, which would be real proof of the long-term usefulness of the method. Unfortunately, there were no examples of direct collaboration with the GDL developers, for which the current resolution would work best.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- This is one of the keystone projects of the portfolio because only neutron imaging can show where water is in operando in the stack and in fuel cell components. The project has now become indispensable to achieving the goals of the Program.
- Liquid water has been shown to play a key role in performance and durability, particularly carbon corrosion, Pt dissolution, and membrane degradation. One of the key bottlenecks has been the ability to differentiate the membrane and catalyst layers; assuming the new detectors are capable of addressing this need, this project has the potential to dramatically impact the advancement of the Program's targets and objectives. One of the key aspects will be strong technical management of the path to the low-resolution detector.
- This project is critical to understanding water transport in fuel cells. Without this imaging capability, optimizing water transport would prove very difficult.
- Improved understanding of water movement within fuel cells can potentially yield valuable insight into future improvements on both power density (cost) and durability. The NIST facility alone will not provide the fundamental understanding required, but if these results are combined with modeling and creative experiments, as has been the case to date, then the results do add value.
- The user program highlights demonstrate the immediate impact that neutron imaging is having on fuel cell development. Ongoing technique developments will further the impact neutron imaging can have on this field.
- Water management creates analytical challenges, and the use of this experimental technique helps accelerate fuel cell development.
- Completion of the test stand for automotive-scale cells is a major step forward.
- The project is focused on improving neutron imaging rather than overcoming barriers in the FCTO Multi-Year Research, Development, and Demonstration Plan. It is an enabling technology that depends on its use to make gains in terms of FCTO milestones. Higher spatial and time resolutions are critical for continued understanding of water and thermal management.
- While not the only tool to analyze the water content of a cell, this remains an effective tool for researchers to analyze fuel cell performance.
- The project currently only partially supports the Program and is mainly useful for water transport studies. The project still did not show any progress in durability studies.

Question 5: Proposed future work

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

- The use of a standard fuel cell for high-resolution imaging will be extremely useful for all researchers who wish to utilize this technique to evaluate their new electrode and MEA concepts. The second cold neutron imaging line will add much-needed additional capacity, and if it can discriminate water from ice, it will also add yet another nice option to the suite of possible experiments.
- The team has proposed ongoing development to extend the time and spatial resolution of neutron imaging. The future of neutron imaging looks very bright.
- The proposed future work aligns well with the advanced capabilities requested by collaboration partners. The presenter clearly outlined a path to achieve the higher resolutions.
- Work should continue on improving the flux of the beam to allow faster image capture times. It appears that capturing the dynamic response of fuel cell performance may be of more importance to OEMs now that increasing resolution makes this possible.
- The proposed cold-neutron imaging facility, which could discriminate ice from water, will be beneficial. The Wolter optics lens will be a big advance, allowing much greater temporal or spatial resolution. Collaboration with Los Alamos National Laboratory to develop a high-resolution fuel cell is necessary to take advantage of the higher resolution.

- The continued focus on improving the performance (both resolution and acquisition time) is excellent. However, the project date of 2018 for developing the new lens system appears to be excessively long. While one realizes that this is new technology, it is not apparent why this should take four years.
- Justification for the cold neutron source in terms of liquid and ice is not critical, because ice is not a major concern. The use of the microscope looks interesting, and it is critical to get the time resolution down for higher spatial resolutions. For the new improvements, it would be good to know if there is going to be an increase in any issues that cause systematic errors, and thus limit the actual improvements.
- In the future work, the use of a standard high-resolution fuel cell is shown. This cell shows the use of serpentine-based flow fields. It is recommended that for the purposes of a standard cell, serpentine-based flow fields are not used. In particular, serpentine flow fields can elicit significant in-plane pressure drops/reactant transfer across the landings, depending on the MEA used. As such, this becomes a much harder test cell to understand and design around. The use of a straight flow field is likely more appropriate for the purposes of a differential cell. In the event that this is not feasible, it may be ideal to develop both a straight cell and serpentine standard cell that then allows choice in the included mass transport mechanisms. In addition, the standard test cell should be chosen/developed such that it elicits good comparison/correlation to a larger-scale differential cell. In relation to the development of the high-resolution MEA water content based on CHANDRA, there are outstanding questions about what the comparison is between NIST's schedule for the low-resolution detector, the National Aeronautics and Space Administration's (NASA's) development, and the needs of NIST from NASA's development plan.
- It is unclear what the specific plans are. However, the emphasis on image acquisition time, in addition to resolution, is encouraging. Better time resolution to eventually capture some dynamics of local water accumulation and transport would be valuable. A high-resolution test cell with channels and lands would be much preferred to the porous flow-field concept outlined in the presentation. The effects of channels, lands, the local compression of GDL, and other constraints on the water distribution are masked by porous flow fields, but many designs still rely on channels.
- There is considerable risk in the proposed path forward, which involves engaging more and more complicated equipment in this research. The team should focus its scientific efforts on resolution and faster data imaging, while also expanding education to the industry on what features of the method could be useful in specific applications.

Project strengths:

- Strengths of this project include the following: its strong collaboration with users, its extensive users' list from both academia and industry, it is the only non-destructive liquid water imaging method currently available, novel work is being completed at the facility by users, and it has made good progress toward the development of a sub-10 μresolution.
- This is a tool that provides an in situ look at water accumulation in a fuel cell. Understanding water balance is a critical factor in design.
- The project features great collaborations both inside and outside the fuel cell community. It has very strong expertise and focus on technique development.
- There is collaboration with many industry partners. It is great that NIST and DOE help to make this tool available.
- The project features a state-of-the-art high-resolution neutron imaging facility for in operando fuel cell studies.
- The project provides a valuable state-of-the-art tool for characterizing transport in fuel cells. The team is responsive to the needs of the fuel cell community.
- Strengths of the project include its financial support from NIST, its continued progress on improving the facility's capabilities, and that it provides useful insights into fundamental phenomena.
- The project provided good diagnostics of water management for researchers in the national laboratories and academia.
- Water imaging is critical for understanding mass transport, and the project is focused on understanding this at increasing time and spatial resolutions.

Project weaknesses:

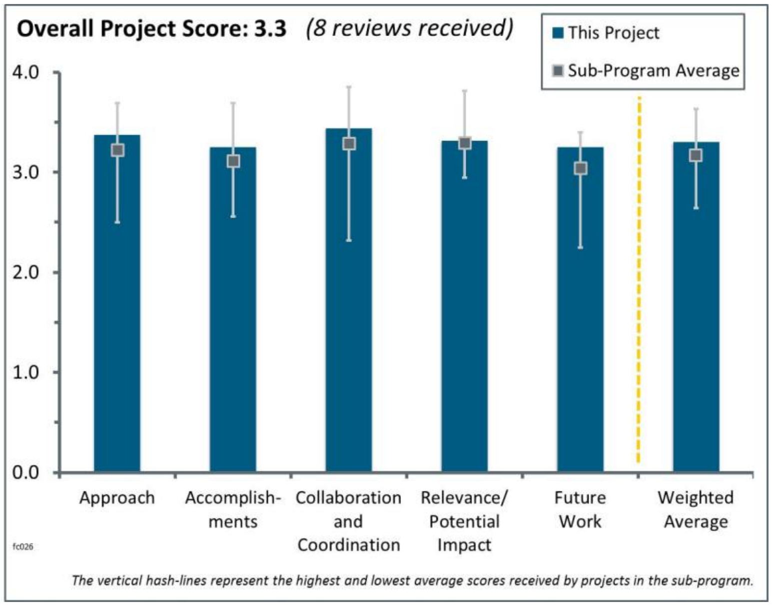
- There is a lack of a standardized porous test sample to provide confirmation for low-resolution detector/high-resolution detector comparisons. Image acquisition times are not fast enough to study realistic cycle transients. The proposed standard test cell is not consistent with industry standards. There are still significant hurdles to overcome for the high-resolution detector. There is a potential mismatch in development path timing between NASA and NIST.
- NIST is dependent on users and their ideas for understanding and improving fuel cells and fuel cell components. It would be good to see the amount of accepted versus submitted proposals and the typical reviewer base and criteria. For issues such as interfaces and ionomer and catalyst layer limitations, it is not clear that the neutron lens will reach the resolution needed for understanding.
- This technique is clearly not mobile or accessible to many partners—it will always remain a fairly expensive tool. It is unclear how useful it will be as an analytical tool when fuel cells achieve high-volume penetration.
- The team needs to make sure that the aggressive technique development does not detract from user work. Highlighting new users and more user program highlights would be strongly encouraged for the next DOE Hydrogen and Fuel Cells Program Annual Merit Review.
- The pace of degradation studies in fuel cell research is already ahead of what could be supported and explained by this diagnostic without increasing high resolution even further below 1 µ, which does not seem feasible for another year.
- Issues beyond NIST's and DOE's control (budget issues and government shutdown) have delayed upgrades and limited the availability of the imaging facility.
- The improvement in resolution appears to be leveling off, and substantial progress on acquisition time will apparently take a very long time.
- The timeline for magnifying neutron lens development is long.
- There is a lack of capacity for everyone to do experiments.

Recommendations for additions/deletions to project scope:

- NIST should develop an engineered porous media test sample with controlled water distribution on a sub-5 µ scale, which can be used for benchmarking images quantitatively. NIST should also develop a straight flow field standard test cell rather than a serpentine-based cell and benchmark the straight flow field test cell with a similar, larger-scale differential cell.
- NIST should prioritize either 10- or 1 µ resolution work. It should also elevate awareness of the method capabilities to GDL developers and stack developers. In addition, it should reassess the efforts required to maintain the fuel cell infrastructure for automotive-scale testing and, if utilized at a low rate, redirect the efforts.
- The PI should elaborate on which attribute is more important to OEMs—resolution or response time.
- The team should think about adding some flexibility into the standardized hardware in terms of compression, pressure control, and utilizing next-generation material sets.
- The project team should try to accelerate the schedule on the new lens system.
- Researchers should pursue more of the same type of work.

Project # FC-026: Fuel Cell Fundamentals at Low and Subzero Temperatures

Adam Weber; Lawrence Berkeley National Laboratory



Brief Summary of Project:

Primary objectives of this project are to understand transport phenomena and water and thermal management at low and subzero temperatures using state-of-the-art materials and to elucidate the associated degradation mechanisms due to cold and cool operation. Improved understanding is expected to enable U.S. Department of Energy (DOE) targets to be met with regard to cold start, survivability, performance, and cost. Water management is examined in thin-film catalyst layers and various fuel cell components.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- This project uses a solid approach of combining ex situ constitutive property measurements to obtain the parameters of the individual components to input into the component submodels and in situ fuel cell testing with advanced diagnostics to validate the two-dimensional (2-D) cell model. The researchers are focused on the fundamental thermodynamics, transport, and kinetics that govern fuel cell performance and cover the range of relevant operating conditions.
- The approach combines in situ/ex situ diagnostics and measured data as validation for submodels and then as input for the cell model. This provides a solid basis for the provision of input parameters and confirmation of the physics submodel per submodel. The overall validation being done using cell data and various in situ studies provides a range of conditions appropriate to test the included physics/phenomena.
- The approach is well organized, with multiple partners performing specific tasks.
- The approach for model development and validation is sound.
- Developing models via model-experiment comparisons at both the cell and component levels appears to be an effective approach. However, the land/channel 2-D cell model only applies to differential cell conditions. It is insufficient in simulating down-the-channel distributions of current and water, which are normally non-uniform when a cell starts at cold condition.
- The general approach of applying ex situ and in situ diagnostics to develop inputs for a cell model that is then applied to explain trends observed in operando is sound. Two recommendations:
    - The project team should not use serpentine flow fields in any aspect of this project, because of the known problems with U-bend water accumulation and reactant short-circuiting. A flow field with pure counterflow in the active area (developed by General Motors and used by Los Alamos National Laboratory and others) eliminates these issues; produces nearly one-dimensional (1-D) distributions of current density, temperature, concentration, etc.; and is much easier to model and understand. Moreover, perhaps most importantly, this geometry closely approximates the design of most application hardware.
    - The project could benefit from closer collaboration with a system integrator or original equipment manufacturer (OEM), who could provide insight into issues related to full-size stack hardware.
- The connection of diagnostic data and materials characterization to the developed cell model is still not as clear as one would like. For example, regarding the "new" anode gas diffusion layer (GDL) versus "baseline" studies, there is GDL characterization and cell-level water characterization, but there is no clearly presented example of a model plus varying GDL properties predicting the trends. Similarly, to

address water management in an operating cell, transient and spatial (at least the proposed 2-D + 1-D) effects should have been considered in the model from the beginning.

- Stack studies are defined in Task 3 but not in the milestones. It is unclear whether the model is being compared to stack data.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The demonstration of the differences between the improved and baseline GDLs for the nanostructured thin-film (NSTF) catalysts is a key finding. The identification that the kinetic nucleation plays a key role in the onset of freezing versus a purely thermodynamically controlled process is key to improving freeze start capability.
- The additional experimentation and testing is great to help validate the model.
- Some interesting data is being collected and no doubt progress is being made, but some of the claimed accomplishments are not substantiated by the material presented at the DOE Hydrogen and Fuel Cells Program Annual Merit Review. Specifically, there was no evidence of a validated steady-state model over a full range of operating conditions. The water balance data for different microporous layers (MPLs) is interesting and shows how low temperate performance can be improved. It is not clear whether the model predicts these improvements or how the GDL properties should be further modified to make additional performance improvements. Validating that freeze kinetics are important is a nice, if not surprising, result. It was not clear whether the model predicts the isothermal and adiabatic freeze start experiments from United Technologies Research Center (UTRC).
- The project's GDL characterization is quite comprehensive. Water droplet detachment velocity appears to be a characteristic parameter suitable for new GDL screening. Easier droplet detachment results from lower fiber density and thus preferential water pathway; however, the model still uses Darcy's law for liquid water transport in GDL.
- Identifying the importance of anode GDL transport characteristics for NSTF water management is valuable. The GDL ex situ characterization is adding some quantitative information for these particular materials, but the trends and interpretation are essentially the same as a number of previous studies. There was no mention of the anisotropy of transport properties of the GDLs.
- The team has developed a good modeling framework and conducted tests to validate the model. The team now has to exercise the model to improve the performance of the NSTF electrode; it is unclear how this model will be applied to solve this remaining challenge.
- This project has made a clear impact toward the key mechanisms that limit NSTF cold start performance. However, the quantitative impacts against DOE metrics were not disclosed.
- Regarding slide 13—other DOE projects and recent literature (e.g., Caulk and Baker, *Journal of the Electrochemical Society*, 2010) have shown that through-plane thermal conductivity and an effective dry diffusion coefficient have controlling influence on GDL water transport and accumulation. The reviewer wonders if these parameters been considered in the performance of the baseline and improved materials, with and without PTFE. Regarding slide 14—it is suggested that the project team apply some effort to studying the patent literature. A number of companies have shown that in-plane variations in GDL properties can significantly improve fuel cell performance. Regarding slide 18—this experiment seems to be based on the assumption that the MPL controls the rate of liquid water transport. It is unclear how the researchers know that the MPL does not promote water transport in the vapor phase, as has been reported in the literature.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- This project has excellent collaborations among national laboratories, universities, and industrial partners for component characterization and cell diagnostics. It is well coordinated in terms of getting data for modeling.

- There is significant collaboration on this project. The presenter did a nice job of describing the interactions and why specific collaborators were selected.
- The project has a great team working together from industry, academia, and national laboratories.
- The change to an adiabatic cell design is positive, in that it implies better connection between the effort to understand water transport and the experimental design.
- There is a strong list of collaborators for the project. There are a number of researchers that are linked to no-cost work on Nafion® thin films—it would be useful to comment on how this work will be included and to what degree it is believed to be important/a critical path. The project appears to lack the inclusion of an OEM or stack manufacturer (in addition to UTRC); it would be beneficial to consider including another for added perspective.
- The project is benefiting from the contributions of many collaborators, especially in the area of advanced diagnostics and experimental methods. UTRC is the lone OEM collaborator, and it is not clear whether it is providing guidance that is representative of the broader OEM community on the most critical issues that should be addressed.
- Collaborators are well recognized for their expertise. A GDL supplier should be engaged to consider the manufacturability and cost aspects of novel concepts. Also, the stack/system level input from UTRC was not obvious in the presentation.
- Generally this is a strong group, but the team could benefit from close collaboration with an OEM water management expert.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- This is a nice project to help understand the fundamentals around water management, and it could help accelerate technical advancements.
- Cold start issues related to thin electrodes remain a critical issue in the state of the art.
- This work is excellent and one of the projects pushing the envelope in understanding fuel cell transport understanding. The team should use simpler hardware designs that are more representative of real systems.
- The fundamental understanding of freeze-thaw and of NSTF water management issues will be of strategic value to the community as a whole. Without temporal and spatial effects in the developed model, its application to improving water management in an automotive cell will only be to provide qualitative trends.
- Robust cell performance at low and subzero temperature is essential to the successful commercialization of fuel cell vehicles. Although the project is focused on NSTF electrodes, which have shown poor performance in cold environment, the learnings on other cell components (e.g., GDL and membrane) will be applicable to the cells utilizing conventional electrodes with dispersed catalysts.
- The relevance of this project is limited by its focus on NSTF-based membrane electrode assemblies (MEAs). The biggest challenge to enabling low-Pt-loaded MEAs is the local O_2 transport losses in the cathode. It is not clear how this critical issue is being addressed within this project. Also, it is not clear how the Nafion morphology studies will help address any of the key problems currently facing OEMs. There is no evidence that the rate of water transport from the thin electrode layer into the membrane is impacting low temperature performance, and, even if it is, it is unclear how the morphology studies being done will help reduce that limitation.
- The project focuses on aspects related to the key barrier for inclusion of the NSTF catalyst into commercial products. In addition, the project addresses aspects that may be related to the loss seen in low-loaded catalysts. It would be ideal to add additional catalyst layers, such as high-surface-area or low-surface-area Pt/C catalysts, in order to elicit different mechanisms for water transport and subzero operation.
- The relevance of this project is not aligned with the Fuel Cell Technologies Office focus area; this project is addressing the performance issues of NSTF, which is not the primary gap area for fuel cell technology as a whole. Performance challenges of NSTF, stack water management, and start-up issues are not critical path items for supported catalysts. All automotive OEMs have shown that the current system can meet the performance goals set by DOE.

Question 5: Proposed future work

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

- The proposed future work includes developing a 2-D +1 down-the-channel model and characterizing a traditional dispersed catalyst, which is in the right direction toward finding optimization/mitigation strategies for fuel cells in real-world applications.
- The project covers many of the previous years' comments. Expansion of the traditional CLs to consider the effect of different types of carbon support may be well suited. In addition, development of a list of key inputs/outputs from the model would be very beneficial. Furthermore, slides discussing the confirmed submodels and the associated validation could be included to demonstrate the successful confirmation for key areas, such as water management.
- Much of the future work is relevant. The team should especially pursue "Soliciting input and advice from automotive companies." It would also be good to see the team prioritize "Examining properties and uptake with low-EW ionomer and ionomer thin-films" of dispersed catalyst layers. Validating the transient model, especially for NSTF electrodes at cold start conditions, should also be a priority. The only future work that should be eliminated is studying the "interfacial resistance and membrane morphology with different environments."
- The future work is very ambitious, considering the project is already extended and many of these objectives have carried through the project since it began. It is appropriately focused, but the timeline and feasibility are unclear. Many of these fundamental questions will linger. Perhaps a sensitivity study can help to optimize resources. Also, this project should consider the inner layer presented by 3M.
- The future work is largely focused on additional characterization and model improvements. There should be more application—identifying materials solutions and operating strategies to improve cell performance.
- Because there are a lot of pieces to this project, it would be helpful if the future work also referenced the work plan tasks defined in the project approach.
- The proposed future work is generally solid, but the research team should consider a more even split of the effort between NSTF and conventional electrodes, despite the originally proposed project scope.
- "Understanding stack location effects" (and others) are outside the scope of this fundamental activity, and that is not a key challenge area. The project is almost ending, so the focus needs to be on exercising the model to understand how the performance of these thin catalysts layers can be improved.

Project strengths:

- The project has the following strengths: (1) a fundamental approach to studying water management and sub-zero operation; (2) various levels of inputs, diagnostics, and validation; (3) strong collaboration between various organizations; (4) addresses the impact of both the catalyst and the GDL; and (5) considers different mechanisms for the formation of ice.
- There is a clear linkage between characterization and modeling. Investigators have applied and developed state-of-the-art methods toward the critical aspects of the problem.
- The project has a great team with a strong publication record. It is clearly starting to close the loop on fundamental water management issues.
- The project features strong fundamental modeling and physics, coupled with good experimental characterization.
- Strengths of the project include its solid approach, very strong team, and access to world-class experimental and modeling facilities.
- The combined modeling and experimental approach and strong coordination between highly capable modelers and experimentalists are the project strengths.
- The project investigates fundamental understanding of key barriers.
- The project features a good team and strong fundamentals.

Project weaknesses:

- The project has the following weaknesses: (1) importance of interfaces between various components and the role in water management/subzero operation; (2) lack of a stack developer or automotive OEM; and (3) unclear leverage of thin-film tests from no-cost partners, and an inconsistent timeline versus the critical path.

- The project seems very focused on basic science; there is not enough application of what has been learned to investigate what a next-generation material or future operating strategy should be.
- There is a lack of direct input from automotive OEMs. Models, to this point, seem to focus on explaining data rather than driving mitigation strategies for key issues and materials and design improvements.
- Proton conductivity on wet Pt is a difficult challenge, and this model requires a validated mathematical description of this phenomenon.
- The researchers should have a more direct relationship with a water management expert working on full stacks and systems so the fundamental experiments/modeling comprehend “real-world” issues.
- With the proposed future work, this project does not seem to have a weakness.
- The focus area is not the critical path.

Recommendations for additions/deletions to project scope:

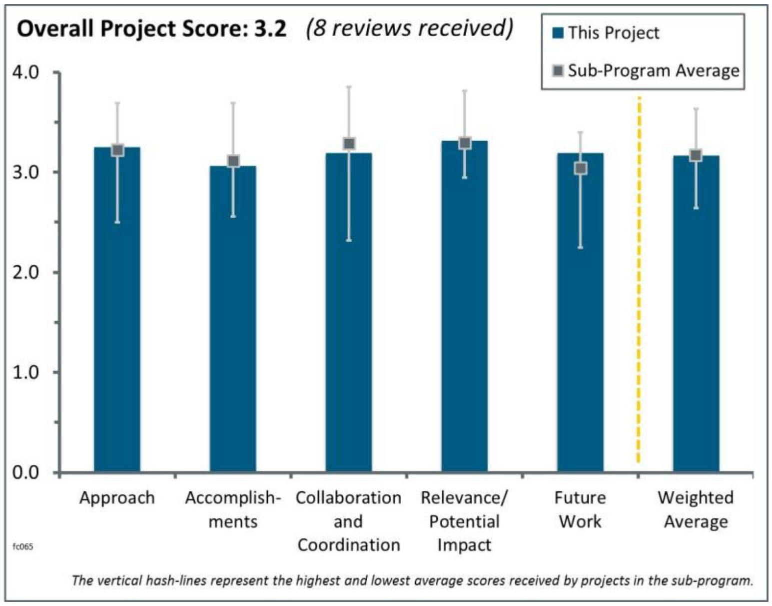
- The droplet detachment velocity sensitivity of the improved GDL with and without PTFE relative to the baseline is intriguing. If the materials cannot be revealed, it is essential that the key parameters involved are investigated. Neutron imaging also seems like an ideal validation tool. There must be a tradeoff related to passively shifting the water balance to the anode during normal operation. It is unclear whether this is really a solution to the cold start issue from an overall system efficiency point of view. This project should consider the inner layer presented by 3M.
- The researchers should consider the use of different surface area carbon supports when considering conventional Pt/C layers (including low loaded). They should also include an additional OEM or automotive partner.
- The team should remove studying the interfacial resistance and membrane morphology with different environments from the future work. Instead, it should focus on dynamic testing and model validation.
- Because the NSTF electrode is not ready for automotive application in the foreseeable future, more work should be done on the electrode with traditional dispersed catalysts, including quantifying liquid water distribution.
- It would be nice to see additional testing for the transient states.

Project # FC-065: The Effect of Airborne Contaminants on Fuel Cell Performance and Durability

Jean St-Pierre; Hawaii Natural Energy Institute

Brief Summary of Project:

The objective of this project is to identify and mitigate the adverse effects of airborne contaminants on fuel cell system performance and durability. The adverse impacts are mitigated by providing input into air filter specifications and into fuel cell material selection, design, and operation. The Hawaii Natural Energy Institute (HNEI) obtained detailed characterization information about the performance impact of eight contaminants to support the development of mitigation strategies.



Question 1: Approach to performing the work

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

- The project features a clear approach with well-defined goals and barriers.
- The systematic approach to identify and characterize potential airborne contaminants is sound.
- The project is focused on durability and performance issues and addresses key barriers to fuel cell commercialization. Recovery techniques are being investigated, which is appropriate. The project has focused on Pt catalysts at relatively high loadings in comparison to U.S. Department of Energy (DOE) targets, though pure Pt catalysts are unlikely to meet the cost and loading targets. The initial data on Pt have been useful, but as the project progressed the focus should have shifted to lower loadings and current state-of-the-art alloy catalysts (or nanostructured catalysts derived from alloys) at low loadings (comparable to DOE targets). The use of methanol as a model contaminant for water scavenging of all the organic impurities is not a good choice (slide 14). Methanol is completely soluble in water, where several of the organic contaminants (propene, acetylene, bromomethane) only have slight solubility or are insoluble (naphthalene). Removal of these species by water should be quite different than removal of methanol.
- The approach (no overlap with air contaminants that have already been studied, selection of contaminants based on probability in air and effect on membrane electrode assembly [MEA]) has been established in prior years and improved this year with some incorporation of low-loaded cathodes. The use of a gas chromatograph to analyze cell effluent is an improvement. The problem with the project is that low loading (0.1 mg/cm^2) has not been fully incorporated for all contaminants, and the project may not get a chance to complete low-loading tests. Because it is known that voltage losses are different for low-loaded cathodes, it is reasonable to say that contamination mechanisms may be different for low-loaded catalyst layers.
- The project has addressed a large list of potential airborne contaminants that are a potential barrier to the use of fuel cell systems in certain regions. Within the approach, each contaminant is tested individually—comments or analysis may be beneficial to rule out the potential for reaction between different contaminants on the catalyst surface.
- Systematic study of the classes of contaminants will lead to improved understanding of the types of poisoning and recovery methods. Perhaps this effort may guide development of advanced air filters that target removal of particularly nasty airborne contaminants. While it is understandable that NO_x, SO_x, and NH_3 are excluded due to the vast research out there, it would be good to include them in very brief form to set a baseline to which the other contaminants can be referred.

- The applied strategy needs significant improvements. The only way to learn how trace levels of organic impurities may affect interfacial properties is to first apply a surface science strategy that incorporates a variety of spectroscopies (e.g., FTIR [Fourier Transform Infrared] for monitoring adsorbed oxidation products) and electrochemical measurements on well-defined surfaces, and then transfer the knowledge gained to real systems. The same applies for the effect of Ca_2+ and/or $CaCl_2$ and/or SO_4-, which is of paramount importance for the development of fuel cell systems. The systematic addition of these "impurities" in clean electrolytes and the monitoring of changes in adsorption/reactivity properties of the three-phase interface is the only way to develop a true understanding of the role of these impurities on the oxygen reduction reaction (ORR) and peroxide production.
- The selection of contaminants for research in this project appears to be somewhat arbitrary. Most impurities do not seem to be of particular concern, occurring rather sporadically; some are rather exotic.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The team has achieved excellent findings with regard to the Pt loading and contaminant effect. There was a clear presentation of the contaminants and how they respond within the cell over time.
- The team has done well with its focus on recovery from airborne contaminants, with several sound guidelines and discoveries that will benefit the fuel cell community as a whole.
- Previous targets seem to have been met, and the project is on course to meet current milestones with minimal delay.
- The project has demonstrated that for the contaminants considered, the primary impact is on the catalyst surface and not on impacts on the porous media. It would be useful to consider if there are any suspected compounds that are known to bond to carbon that may alter wettability or contact angle and would result in a different conclusion. In slide 8, commenting on the role of current density on Pt surface area, basis may assist in eliciting the turnover effect that happens when moving to lower loadings. Regarding the organic species inhibiting the oxygen path to water, it is unclear whether this is due to competitive co-adsoprtion effects, and, if so, whether it is possible to consider this effect as a change in the energy necessary for adsorption as a modification to existing platinum oxide analysis/ORR pathway models. The test data from the segmented cell are run in constant current mode; in this mode, the cell segments will experience a voltage response in order to produce the appropriate amount of current. In essence, each cell becomes more or less efficient depending on its local conditions. However, if the mechanism of the contamination is electrochemical, versus chemical, differing responses could be expected by a change in the local potential. As such, perhaps a constant voltage test may be more appropriate to controlling the state of the surface of the catalyst related to the contamination. For foreign ion contamination, it is unclear if there has been confirmation of which compounds tend to de-protonate versus ones that are simply water solubility and have the potential to be solvated within the water phase of the ionomer; this may be important for compounds that are doped into the ionomeric materials for membrane durability.
- The project has obtained data on a variety of contaminants and provided useful data to the community. The 1:1 correlation between the kinetic loss and what is attributed to a mass transfer loss on slide 7 is questionable. It appears more likely that the "mass transfer" loss is a kinetic loss for which the researchers are not correctly accounting. Other tests to determine if this loss is mass transfer related would be beneficial (limiting current measurements, different gas compositions).
- The principal investigator (PI) presented data that may serve as a foundation for understanding the effects of organic impurities on the ORR and peroxide formation, as well as the effect of peroxide formation on membrane degradation. The PI has not pointed out how reproducible these data are. This is very important because the effects of impurities are usually difficult to reproduce, even on single crystal surfaces. The proposed mechanisms are rather speculative and more in situ information is required. The interesting part is the role of cations on interfacial properties and how hypothetical (but as of now unproven) accumulation of cations at the interface is competing with the adsorption of anions—in particular, CO.
- Progress in the project has been moderate, which may in part be related to significant underspending of the project funds; the project is 80% complete based on schedule, with only approximately 56% of funds spent. The focus has been on collecting test data with relatively little in-depth interpretation of the mechanism of

performance loss and recovery or development of viable mitigation strategies. Performance recovery protocol based on cycling up to 1.2 V is unrealistic.
- The project still does not understand whether lower loadings introduce different contaminant mechanisms. Unfortunately, the standard Pt loading is 0.4 mg/cm^2, and this is the basis for many of the percent loss numbers. For many contaminants, the project is still trying to understand contamination mechanisms. However, at least some work has been done for at least one concentration for each contaminant at low catalyst loading. Good work has been done to show that for the contaminants in the project, the mass transport losses are associated with contamination on the Pt surface (adsorption). The relationship of contamination to H_2O_2 has been discussed in the literature. Data may be more useful if more concentrations were investigated. For example, the table on slide 6 shows very high acetylene concentration and thus high degradation.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- A wide range of collaborators have contributed. It appears that General Motors contributed with respect to test stand design and modeling of cation contamination. Ballard, the University of Connecticut, a few defunct organizations (UTC Power, ClearEdge Power), and numerous other governmental and supplier organizations have all contributed information. It appears that the investigators have done a thorough job of gathering information from as many sources as possible.
- Collaboration is excellent and partners are well coordinated with the leading PI.
- The strong partners and collaborations continue to be a strength of the project.
- Activities seem well integrated between collaborators; the lack of explicitness in the presentation makes this harder to judge. In giving the score, the reviewer has assumed the lack of individual organizations' slides means the project functions as a unit, which is the best way it can be run. The lack of short stack testing to see how recovery from the most common contaminants is affected by an extended system is an area for improvement.
- There is good collaboration with those in the project. Collaboration with automotive original equipment manufacturers (OEMs) and catalyst developers would have been useful.
- Collaboration exists, but the collaborators' roles were not clearly discussed and the impact of their contribution was not mentioned.
- There are many organizations involved in this project. Though difficult to assess, the degree of effort and coordination seems sufficient.
- The project includes two OEMs—Ballard and United Technologies Research Center. It would be advisable to have an additional automotive partner with a high level of involvement. The need to go to lower-loaded catalyst layers will be impacted by the tolerance to airborne contaminants due to competitive co-adsorption- and turnover-related phenomena; this is primarily the direction needed as the automotive industry nears commercialization of fuel cell fleets.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- Air contaminant effects are relevant to understanding fuel cell degradation. There are also cost considerations. If certain contaminants need to be scrubbed or filtered, the extra equipment needs to be considered in calculating the cost of a fuel cell system. Another alternative would be the addition of Pt or active area to compensate for the extra expected performance degradation due to air contaminants. The work is also relevant because at this stage it does not overlap tremendously with OEM efforts, especially for contaminants that have not been exhaustively studied in the literature. It is easier for OEMs to have national laboratories and universities survey a wide range of contaminants.
- This project is relevant to Fuel Cell Technologies Office (FCTO) goals, as described in the FCTO Multi-Year Research, Demonstration, and Development Plan.

- This is a unique study in the fuel cell community that has already yielded valuable information on the impact of and recovery from airborne contaminants.
- The effect of airborne contaminants on the catalyst layer could potentially be a huge issue with the rollout of a fuel cell fleet, depending on the locale. Much of the effect of catalyst-based contaminants will only become more influential as the industry drives to lower loading. It would be advisable to consider adding low-loaded catalysts to the test protocol. In addition, the generated data can differentiate between contaminants that are reacting from an electrochemical versus a chemical pathway. In this sense, it should be possible to construct a data table that consists of the contaminant, the reaction pathway, the rate of reaction or adsorption, and the method of cleaning with the associated rate. This table would provide the capability to cast the data into other performance models such as the Argonne National Laboratory system model to establish the design/cost effects on the fuel cell system.
- This is an excellent project—understanding which contaminants impact performance is imperative. The team should have a slide that indicates where the contaminant is typically seen, how likely it is to be encountered, and what filtration would be required to remove it.
- The role of impurities in electrocatalysis—particularly in the OER—is huge, affecting both the activity of catalysts and the stability of interfaces in hostile electrochemical environments. The PI demonstrated that a small amount of organic impurities and cations have a dramatic effect on reactivity and peroxide production, and consequently the stability of Nafion®. However, the project fails to address an equally important issue— namely, how the very same impurities may affect the stability of the support and the catalysts.
- The project is good at identifying possible sources of performance-diminishing contaminants, which is an important first step. More focus on the development of mitigation and recovery strategies (rather than merely ceasing to supply the contaminant) will enhance the impact. So far, there does not seem to be a focus on the influence of contaminants on MEA degradation/durability (not just poisoning effects), though it is proposed in future work.
- The project helps identify impurities that may be a concern; however, more relevant catalyst materials and loadings are needed.

Question 5: Proposed future work

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

- The database is a key aspect of the future work and is valuable to many different groups with the industry and fuel cell programs. The format of the database is key and certain key information should be included. The solubility of the airborne contaminants should also be assessed because it will be a key aspect to including the reaction mechanisms and appropriate mechanistic delivery to the Pt surface. The effect of the peroxide production on the membrane should be identified, and consultation with the U.S. DRIVE Partnership's Fuel Cell Tech Team and other automotive and industry partners should be done on the appropriate membrane durability protocol. Consideration of the peroxide production/airborne contaminants on the seals for the cell should also be pursued.
- The project features a well-defined and relevant plan. Disseminating the data to industry would be very useful. Mitigation strategies will also be important—it will be good to see these.
- Further insight into the mitigation of contaminant effects is a key aspect of the future work, as is a mechanism to disseminate the project database to industry.
- The proposed future work will focus on a key need for helping automotive fuel cell commercialization—adding robustness to the system toward airborne contaminants. Perhaps the only thing to add is the possibility of crossover from these contaminants to the anode side of the MEA, where a much lower Pt loading is more at risk to contamination. Therefore, the effect of these contaminants on the hydrogen oxidation reaction would also be a great addition.
- The project has correctly identified that many measurements have to be repeated at lower loading. The investigators have indicated that they are racing to achieve this before the end of the project. Similar to the other contaminant project, this project is seeking to disseminate a large database to industry in order to assist with filter designs that will be needed to prevent air contamination of a fuel cell stack.
- The future work plan is logical and has achievable goals.
- The project is concluding in nine months. The work proposed is appropriate to complete the project.

Project strengths:

- The main strength of this project is its thorough approach to testing fuel cell performance with selected contaminants. The mechanism of the increase in peroxide yield as a function of catalyst contamination is convincing.
- Strengths of the project are the well-defined contaminants, impact on cell performance over time, recovery of the cell after contamination, and future mitigation plan development.
- The new mitigation and recovery strategies are an important result and highlight the strength of the project approach and expertise of the team members.
- The project features an extensive database of contaminants. Other strengths include the identification of electrochemical and chemical reaction pathways, identification of distributed reaction effects in the segmented cell, and potential for the identification of specifications for contaminants in the air stream.
- The PI is highly qualified to lead the project. The experiments are reasonably well organized and most of the conclusions made are in line with experimental observations.
- The project has widely surveyed a vast number of air contaminants. It has a considerable breadth of collaborators. Efforts do not overlap greatly with OEM activities.
- The project features a systematic, thorough study, as well as good collaborations.

Project weaknesses:

- The project still needs to complete testing at low Pt loading on the cathode. It could possibly benefit from looking at some contaminants at lower concentrations. There is still some effort needed to identify mechanisms for some contamination. The project needs to clarify contamination mechanism pathways.
- A weakness of the project is the lack of consideration for low-loaded platinum group metal (PGM) or non-PGM catalysts. Other weaknesses include the competitive co-adsorption effects (interaction) between contaminants, and the consideration of the effect of airborne contaminants in the dissolved phase on the MEA seal materials.
- There is limited emphasis on the mechanism of performance degradation. There are also some errors in the interpretation of the causes of performance loss.
- The lack of a clear plan for dissemination of the project's findings to industry is a major weakness of the project.
- There is still a lack of the fundamental knowledge that is required to understand, and ultimately predict, the role of impurities.
- Hopefully this will be amended in future work, but efforts need to be made toward mitigation (filters) and recovery. There is also a lack of short stack "real-life" validation.

Recommendations for additions/deletions to project scope:

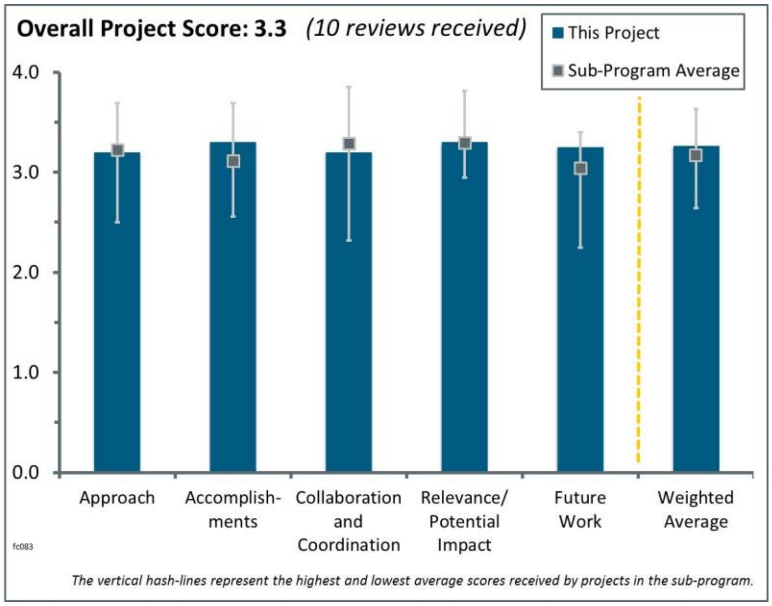
- It would be good to see added to the scope the effect of common poisons to the anode, unless crossover can be proven to be nonexistent. The team should also include common contaminants such as NO_x, SO_x, and NH_3 in brief form to set a baseline to compare to the literature.
- The researchers should include the effects of low-loaded/non-PGM catalysts, include interaction effects between contaminants, categorize data as per recommendations in other comments, and consider the effect of dissolved contaminants on seal materials.
- HNEI needs to continue testing at low Pt cathode loading. In case some contamination mechanisms are not linear with concentration, low concentrations should also be tested.
- Future work should concentrate on scientific aspects of performance degradation, the understanding of which is required for the development of effective mitigation strategies.
- The team should add an overview slide identifying where the selected contaminants are likely to be encountered.
- If the project is going to be continued, a surface science approach must be a part of this project.

Project # FC-083: Enlarging Potential National Penetration for Stationary Fuel Cells through System Design Optimization

Genevieve Saur; National Renewable Energy Laboratory

Brief Summary of Project:

The goal of this project is to build an open-source tool (Distributed Generation Build-out Economic Assessment Tool [DG-BEAT]) that helps combined heat and power (CHP) fuel cell developers, end users, and other stakeholders determine the appropriate sizing to reduce cost; facilitate integration into commercial building control and heating, ventilation, and air conditioning (HVAC) systems to maximize durability; compare performance relative to incumbent technologies; determine optimum system configuration; and evaluate potential market penetration.



Question 1: Approach to performing the work

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

- The approach involves the development and dissemination of an executable generic CHP system installation and operations/emissions analysis tool with several input modules. The model will have broad applicability.
- This project builds an open-source tool that will enable an economic analysis for CHP systems, optimization of the system configuration, and essentially establishment of the business case for fuel cell CHP systems. This is an excellent concept and sound approach.
- The approach to the model formulation was good. The project uses a sound, realistic approach and analyzes a vast amount of data. The excellent additions to the model this year make it even more useful.
- The approach is conducive to delivering a compiled Windows executable version of the model to the User's Group, and it includes 1,024 building energy load profiles.
- A valuable database has been established for siting fuel cell systems.
- The approach for engaging the community through development of the open-source application tool is ambitious and clear. The practical engaging of real industrial entities seems challenging and limited to only several names.
- The general objective of the project is of great interest to help system designers propose the right fuel cell system, depending on the location and the building. The proposed approach appears to be correct, but there is some lack of explanation about the fixed and variable parameters offered to the software users and how optimization between fuel cells and batteries is handled.
- The approach is to develop and evaluate a model for assessing stationary fuel cell performance in supplying energy to buildings. Efforts to develop, evaluate, and distribute the model to users groups are all on schedule. A significant new component of the approach is the addition of information and modeling to address emissions reductions. However, the results of this effort in the optimization calculation are not yet included.
- The reviewer admits to being confused by this project, and this is reflected in the reviewer's scores. The U.S. Department of Energy (DOE) goal as stated on slide 3 is to, by 2020, develop medium-scale CHP fuel cell systems that achieve 50% electrical efficiency, 90% CHP efficiency, and 80,000 hours of durability at a cost of $1,500/kW for operation on natural gas, and $2,100/kW for operation on biogas. It is not clear how the development of the DG-BEAT tool helps to achieve that goal. That needs to be explained by the PI

in much more detail. This could be a poor selection of the DOE goal being addressed. If the goal is to support implementation of CHP systems in the marketplace, then this model would be quite useful. However, that is not what the selected DOE goal states.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The research team has made excellent progress. The addition of emissions control benefits is useful and important. Sizing optimization work is very good. This work does a good job of addressing the load profile in modeling optimum performance. The model now has good coverage both for geographic and temporal variability.
- Significant accomplishments include the following:
    - Implementation of greenhouse gas and criteria pollutants emissions reporting—t including hourly emissions profiles and an emissions minimization control strategy.
    - Implementation of non-predictive load-following strategy. Foundations are in place for higher resolution demand profiles (<1 min) for better examination of system response lag.
    - Implementation of new building profiles for 16 building types in 16 climate regions and 3 building vintages.
- The project team made good additions to the tool with the emissions data and more building profiles.
- This year's additions to the model greatly improve its utility.
- It seems like there has been significant progress in the development of the tool by integrating new parameters in the model. New results have been presented. However, the slides are not very easy to understand. Fortunately, the presentation helped one better understand the possibilities of the model. It is unclear how the extrapolations are done to adapt to the real requested powers. There is no information on durability even if it is critical for stationary application. It is also not clear how optimization of the hybridizing level is handled. Regarding emissions, particle matter of less than 10 µ diameter (PM10) is mentioned in the Approach but not integrated in the Accomplishments, even though PM10 is important regarding health issues.
- The progress has been excellent. The implemented control strategy models fuel cell systems' response and also allows the use of different fuel cell types. A selected user group is identified to test the model and detect areas for improvements. In order to understand the accuracy of the model, a sensitivity study around the different system elements and input parameters would be beneficial.
- Progress on modeling is very good—the model is live, new blocks were added, and resolution was improved. It would be good to see more sensitivity testing with the different case scenarios not limited to single numbers or one company.
- The model is sophisticated; however, it is hard to see a "general" strategy among all of the data.
- The project ends in October 2014. All milestones have been met except for final model delivery, which is on track.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The coordination with partners is solid—the team started to refer to the model and results directly into conversations with industry. Developing work-case scenarios and educating partners on how to do the analysis needs further work.
- The project features excellent collaborations from the following: University of California, Irvine; Lawrence Berkeley National Laboratory; Strategic Analysis, Inc.; Battelle; IDIQ; and several end users who are referred to as the User's Group. The strong collaboration with the User's Group draws from different types of fuel cells, cost and systems analysis, and building energy experts.
- The project features a good mix of many collaborators with different/complementary backgrounds.
- The broad involvement of industrial users to provide feedback is very valuable. However, the commitment level of the User Group is not clear.

- The collaboration and coordination appear to be correct. The User's Group should be enlarged with fuel cell producers and end users.
- There was some brief discussion of collaborations, but the presentation did not provide much detail. Moreover, there was limited discussion on what value was added as a result of collaborations with other national laboratories and vendors.
- It is not clear how involved the User's Group or manufacturing analyses projects are in the project. The presentation identified collaborations, but the involvement was unclear.
- It is not clear what distinguishes a "core" partner.
- Additional collaboration with industry is needed to help validate the model.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The analysis is well done and generally aligns with the DOE Hydrogen and Fuel Cells Program (the Program) and DOE research, development, and demonstration (RD&D) objectives. Upgrades to the model this year greatly improve its relevancy.
- The project aligns well with the Program and DOE RD&D objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- The project is well targeted and beneficial to the research goals of the Program.
- The model facilitates analysis of more than 1,000 CHP scenarios, providing a basis for system selection and design optimization.
- The project develops a tool that enables the optimization of CHP system configuration and cost based on building energy use and thus indirectly supports the DOE CHP fuel cell system targets.
- This project aligns well with helping one understand the impact fuel cells could have in various applications.
- Once the model is experimentally validated, it will contribute to the dissemination of the fuel cell technology in buildings by demonstrating the energetic and economic advantages of this technology. A great impact will only be possible if the documentation is clear enough for one to use this model quite easily with a huge amount of possibilities.
- This project seems to lead to a valuable tool; however, it still has to be proven and validated.
- This is a valuable tool for evaluating the potential for a system in a building, but that does not mean it will help meet the DOE performance goals defined in slide 3.
- Alignment with the DOE objectives is not fully clear or quantified. Introducing some metrics would be useful.

Question 5: Proposed future work

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

- This project is a springboard for broad industry involvement in beta testing and dissemination.
- The proposed future work is good and a straightforward continuation of the efforts.
- There is a good pathway forward to completing the tool and improving its database.
- The proposed plan forward is logical and addresses the remaining challenges adequately.
- The project has a good plan for future work.
- The proposed work is in line with the objective of the project. Quality control of the different modules and field validation of an existing installed system should be the main focus of the future work.
- Overall, the proposed steps for future work were well described and relevant to the project goals. There was a brief discussion of model validation without many details on how it will be carried out. It is concerning that there are challenges in doing a model validation without better insight on parameter sensitivity and uncertainty. The researchers should consider a model performance assessment (particularly a sensitivity analysis) as a part or even in place of the model validation effort.

- The future scope is well thought through. More prioritizing is recommended toward model validation and testing and engaging the user group in this activity.
- The proposed future work will continue to enhance the utility of the model. The researchers should make an effort to make the model more readily available.
- The proposed future work should be expanded to include more validation of the models, either through test cases or collaboration with an industry partner.

Project strengths:

- The researchers have made timely and significant progress. The work is very relevant to the research goals of the Program. The consideration of avoided emissions (CO_2, SO_x, NO_x), along with the cost/value of the energy produced, is an important and useful addition. The work is well targeted to the needs of users. The software provides users with a broad range of parameters and scenarios and has a well-structured graphic interface.
- The model appears to be very flexible and not limited to already existing components in the National Renewable Energy Laboratory database.
- The project is built upon a solid foundation and well thought through. The component sizing work is worth further pursuing.
- The modeling approach was excellent. This should be a valuable tool for modeling the efficacy of fuel cell CHP systems.
- The collaborators are a strength of this project.
- Strengths of the project include the scope of collaboration and the detailed model.
- This is a useful tool for evaluating a potential installation.
- A key project strength is that the tool is developed in open-source software.
- This is really cool, versatile, widely applicable, user-friendly software.

Project weaknesses:

- This reviewer admits to being confused by this project, and this is reflected in the reviewer’s scores. The DOE goal as stated on slide 3 is to develop medium-scale CHP fuel cell systems that achieve 50% electrical efficiency, 90% CHP efficiency, and 80,000 hours of durability at a cost of $1,500/kW for operation on natural gas, and $2,100/kW for operation on biogas by 2020. It is not clear how the development of the DG-BEAT tool helps to achieve that goal. The PI needs to explain this in much more detail. This could be a poor selection of the DOE goal being addressed. If the goal is to support implementation of CHP systems in the marketplace, then this model would be quite useful. However, that is not what the selected DOE goal states.
- Quality control of the different modules has to be validated. The proposed designing and cost optimization have not been validated through existing installed fuel cell systems.
- More could be done to make the model available to others and to increase the amount of validation it receives.
- The role of collaborators and the value added by collaborations was not clear. There seems to be limited effort to address model uncertainty and the information value of model design choices.
- The planning for model validation is not clear. Priorities were not clearly identified for 2014 or future work.
- Quality control of the different modules is still ongoing. Validation procedures need to be developed for assessing the fidelity of the model and the data.
- The work does not seem to have been adopted yet by manufacturers—this has to be accomplished.

Recommendations for additions/deletions to project scope:

- Addressing quality control and quality assurance is important. The ability to value (in dollars) the avoided emissions would be a useful addition to the model. The researchers noted that they will be engaging in a model validation. This type of model should be the subject of an ongoing performance evaluation. More realistic assessment of what emissions are offset would be a useful but challenging addition to the model—this is something that should be done in collaboration with other laboratories.

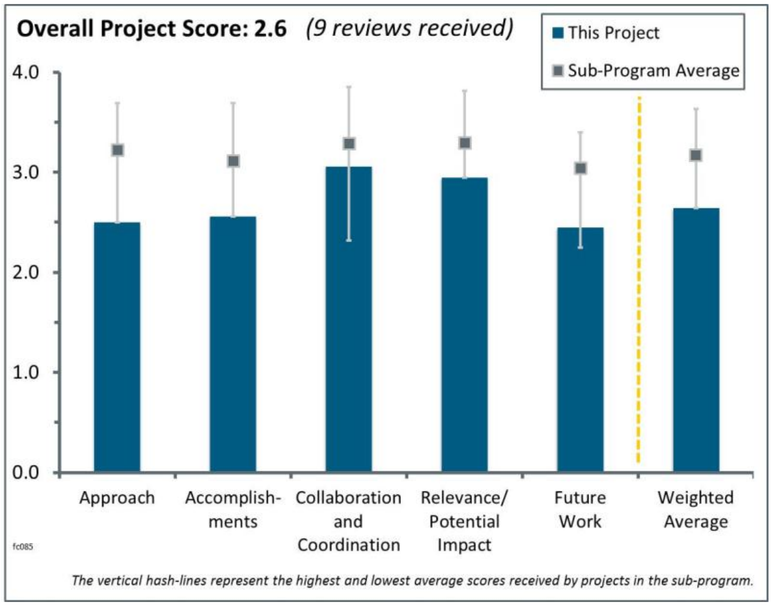
- The team should focus more on mainstream validation testing of the model; it should engage the User's Group for this purpose. It should also decrease efforts on new blocks that still present high uncertainty, such as emissions, unless specifically requested by the model customers.
- The team should present a validation of the model from experimental data.
- The researchers should revisit and adjust the DOE goals and the project goals to bring them into alignment.

Project # FC-085: Synthesis and Characterization of Mixed-Conducting Corrosion-Resistant Oxide Supports

Vijay Ramani; Illinois Institute of Technology

Brief Summary of Project:

Research objectives in this project are to: (1) develop and optimize non-carbon support materials with high corrosion resistance, high surface area, and high electron conductivity; and (2) develop cathode catalysts consisting of Pt deposited on the novel non-carbon supports, and determine the performance and durability of these supported catalysts in in situ and ex situ testing. This project also will develop a cost model for the optimized support materials.



Question 1: Approach to performing the work

This project was rated **2.5** for its approach.

- The approach has been narrowed to focus on the most promising high-performance supports: RuO_2-TiO_2 (RTO) and indium tin oxide (ITO). The team has done very well in meeting the desired support properties and milestones for these materials. However, there are still critical catalyst performance obstacles to overcome, as well as a severe issue with ITO performance in membrane electrode assemblies (MEAs) that needs to be identified and resolved.
- Investigation of oxides as replacements for carbon as fuel cell support materials has merit. The shifts in the project to decrease the amount of Ru used by further supporting it on TiO_2 and to investigate ITO both make sense from a cost consideration standpoint. The investigation of oxide supports has seen reasonable efforts to date, and while the choice of materials investigated has some rational basis, it is not overly compelling.
- The approach is well justified by utilization of both rotating disk electrode (RDE) and MEA in evaluation tests for different oxide material supports for the Pt catalyst.
- It is certainly necessary to explore oxides as support for the oxygen reduction reaction (ORR) catalysts. The selection of oxides could have been different had the researchers considered some experience from other uses of the selected materials. For example, in chlorine production it is known that RuO_2TiO_2 mixed oxides are excellent electrodes for Cl_2 evolution, but that the surface dissolves in O_2 evolution at potentials above 1.4 V. RuO_2 is not stable in the ORR, while SnO_2 is not stable in Nafion®.
- The approach is to replace carbon supports with metal oxides functionalized with proton conduction to eliminate issues with carbon corrosion. The metal oxides explored seem to be primarily SnO_2 and ITO; previously RuO_2-SiO_x and combinations of these were explored. Comments were made that SiO_2 and SnO_2 are chemically stable; previous fuel cell results would indicate that this is not really true. Sn included as an alloy agent in Pt dissolved out (work from Professor Ross [Berkeley]), SiO_x has been problematic over the years in regard to Si gaskets, and SiO_x fibers in microporous layers have shown Si migration all over the cell. The potential cycling approach follows relatively standard methods; however, in these cases, exposure to the oxidizing and reducing atmospheres needs to be included.
- The approach of using oxides to improve corrosion resistance is challenging because these oxides have low conductivity. The project is meeting milestones for improved conductivity but the performance of the MEAs is still far short of U.S. Department of Energy (DOE) targets. The corrosion resistance and stability shows improvement over the reference Pt/C catalyst.

- The project looks at mixed metal oxides as Pt supports. The Pt/ITO catalysts are evaluated using durability protocols that are divergent from the DOE protocols. It is important to compare the DOE and Nissan protocols with state-of-the-art Pt/C catalysts to understand their differences. The Pt/ITO catalyst durability seems to be compared to Pt supported on high surface area carbon (HSAC) support, which does not exhibit good corrosion resistance. A comparison with a corrosion-resistive graphitized C support would be valuable.
- This project continues to rely on supports with questionable stability (ITO in particular) and generally unimpressive conductivity. The approach is generally not innovative. Ruthenium oxide (or mixed oxides) is a no-go not only because of the likely prohibitive cost, but also because of the strongly inhibitive effect of Ru on the ORR.
- The approach of using Ru and ITO for Pt support is not based on fundamentals—thus, it has resulted in poor results.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.6** for its accomplishments and progress.

- The project team has made excellent progress toward meeting the milestones and scaling up the synthesis of the catalysts.
- Better performance and durability were achieved for the Pt/RTO system versus other supports. However, the total cost that includes durability and performance is still higher for Pt/RTO than for Pt/C.
- The progress over the last year is good. It is confusing that the MEA performance is so poor. Literature suggests that the ITO hydrolyzes and causes structural deformation. It is important to understand whether the increased Ohmic resistance is the only contributor to the poor MEA performance. Thus, it may be worthwhile to conduct analyses of the different voltage loss mechanisms of the MEA and the structural changes of the catalyst layer structure with aging (changes in porosity, limiting current). Furthermore, it is important to understand the interaction between ITO and the ionomer. X-ray photoelectron spectroscopic (XPS) measurements of as-prepared, conditioned, and aged catalyst layers would provide information on the changes in surface speciation with operation.
- Syntheses of oxides and deposition of Pt are apparently successful. However, Pt loadings of 44 µg /cm^2 are prohibitively high. It would be better to use 10–15 µg /cm^2. All deficiencies would have been observed within a short time. Information on the oxide-Pt interaction also depends on loading. Therefore, the data for the loading that cannot be used is not helpful. It is not difficult to obtain high activity with that loading. The cost analysis is also affected by a large loading.
- The technical accomplishments are relatively minor in terms of advancing the technology in the target area of improved performance and durability. A number of new materials were synthesized and screened, but the project has not advanced the science of these materials in a meaningful way. Down-selecting to the materials studied is sensible, but the materials set that was investigated is still quite disconnected in terms of characteristics and properties, requiring a broad focus rather than detailed investigations of fundamental limitations that could be used to overcome shortcomings for a single materials set. There has been a general lack of surface science applied to these studies to determine what key variables dictate performance and durability. The Transmission electron microscopy (TEM) shows small Pt particles (3–4 nm), but Pt surface areas have been limited to ~20 m^2/g Pt in almost every case. At this ECA, it is highly unlikely that these materials could ever exhibit sufficient mass activity, even at beginning of life, regardless of durability. The differences between RDE and MEA are interesting, but it is unclear whether it is possible to understand or resolve them.
- Accomplishments for fiscal year (FY) 2014 are mostly related to making ITO and supporting Pt on ITO. A number of materials were synthesized and characterized, mass activity was measured, catalysts were subjected to accelerated stress tests (ASTs), and MEAs were fabricated and tested. Pt electrochemical surface area (ECSA) is low (by ~4x) compared with traditional Pt/C-supported catalysts. Pt/ITO was evaluated for ECSA decrease during AST measurements, and was nearly identical to that of Pt/C in terms of percentage of ECSA loss. The MEA performance is very low, showing poor kinetic performance, high resistance, and what could also be mass transport limitations.

- The team has dedicated significant effort toward developing and testing the new ITO supports. Unfortunately, several obstacles remain—most importantly, the poor performance in MEA. While the team had some good insight into the cause of the poor performance (formation of hydroxide species due to hydrolysis of the ITO surfaces), it did not propose any possible solutions or paths forward. Good progress has been made in controlling particle size and dispersion on ITO and RTO, and it is exciting to see both the reproducible synthesis with scale-up and retained ECSA and mass activity upon Pt loading reduction. Going forward, greater attention needs to be placed on understanding catalyst-support interactions and their impact on performance if ECSA and mass activity are to be improved.
- This project is in its fourth year, and so far it has not generated materials that would be remotely viable for polymer electrolyte fuel cells. There is very little fuel cell performance testing in this presentation; the results shown (slides 17 and 26) attest to very low activity of the Pt catalyst, even on O_2. ESCA is very low for all ITO-supported catalysts. At present, there seems to be no path toward bringing the mass activity of the catalysts to the level required.
- All of the catalysts shown here have low ECSA and mass activity compared to baseline Pt/C; having a durable but really poor activity is a nonstarter. This project is not a good investment of DOE funding—it is unclear how the project made the go/no-go gate.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The collaboration with Nissan is apparently excellent, involving experts in the fuel cell electrocatalysis.
- The collaboration between Nissan and the Illinois Institute of Technology (IIT) appears to be working well, although this is a limited set of partners.
- The partnership with Nissan provides the ability to test the materials under real-world conditions. There is close cooperation between Nissan and the IIT team.
- The collaboration with the Nissan Technical Center North America (NTCNA) has gone very well. It is great to see that NTCNA housed a student this year. It was not clear why the ITO mass activities measured by NTCNA (150 mA/mg) were so much higher than those reported by IIT for the various synthesis protocols.
- Input from all participants in the project is evident from the slides, and the work was well executed and supports the overall project goals.
- The collaboration with Nissan seems to be effective and working well.
- The project has a reasonable team working together.
- The project only involves two institutions—IIT and Nissan. The involvement of other institutions (perhaps even as in-kind contributors) would be very beneficial. Catalyst suppliers or other research projects have developed catalysts based on oxide supports (e.g., Pt/SnO_2 from Tanaka Kikinzoku Kogyo (TKK) and TiO_x at Los Alamos National Laboratory), and using these materials as baseline comparisons in testing could provide useful comparisons (the table on slide 9 was useful, but a broader materials set with specific values filled in would have increased its value). Additionally, organizations that could offer surface science techniques (particularly in situ techniques) or modeling studies to help understand degradation processes would be useful.
- Collaboration is limited to only two partner organizations: IIT and Nissan. No other entities appear to be involved.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.9** for its relevance/potential impact.

- Information on the stability of some oxides in MEAs and their usefulness as Pt support is useful background knowledge.
- The project addresses the corrosion of the carbon supports—one of the key aspects of fuel cell durability with cell start-up/shutdown operation.

- The supports developed under this project have the potential to greatly improve stack durability.
- This project is relevant to DOE Hydrogen and Fuel Cells Program goals, but its impact has been insignificant to date.
- More durable supports would help the project team achieve the performance and durability targets related to the degradation of carbon supports. However, for this impact to be achieved, the material performance needs to be competitive in terms of mass activity.
- Improving the durability of supports would allow less stringent engineering approaches to be put in place to avoid the issue of carbon corrosion that limits current materials in fuel cell systems. The materials developed also need to meet all system requirements (e.g., mass activity and Pt dissolution).
- The project is well aligned, but it has significant technical hurdles to overcome.
- Increasing the stability of supports for Pt catalysts is a critical barrier to the implementation of fuel cell technology. This project offered promising alternatives to traditional carbon based supports that, to date, remain uncompetitive. However, new directions for further research have been identified.
- This project is not solving any of the key challenges highlighted by the Fuel Cell Technologies Office. Even though the project team claims that it is solving the durability aspect, low degradation is not valuable if the catalyst activity is very low.

Question 5: Proposed future work

This project was rated **2.4** for its proposed future work.

- Considering that this is the final year of the project, the scope of the proposed work is appropriate. If completed, it will answer outstanding important questions and initiate technology development.
- The future work outline is adequate. It is important to understand the cause of the poor MEA performance of the ITO catalyst. Work on reducing the ionomer concentration with ITO-based catalysts only makes sense once the poor MEA performance is understood and mitigated.
- The success of the ITO depends on whether the MEA performance issue can be resolved, so clearly this is an essential part of the future work. The path forward for the RTO support was not as clearly established. An effort dedicated to understanding microstructural changes to the catalyst and support in MEA should be strongly considered.
- The addition of XPS studies and the investigation of the difference between RDE and MEA studies are both very sensible. With the little time and funding left for the project and the quality of materials generated to date, it does not make sense to pursue scale-up and large cell studies or Nafion content optimization. It is best to stay focused on a single material set and try to get some fundamental understanding of the system.
- According to the FY 2013 Future Work section, several of the work items listed in this year's Future Work section were supposed to be finished by now—specifically, the ITO supports and the cost model. It is unclear if there is a reason these were not completed as stated in FY 2013. It was probably required in the funding opportunity announcement to make large-scale MEAs, but seeing that the mass activities of Pt/ITO etc. are not comparable to Pt/C (close to Pt/graphitized carbon) and do not come very close to the DOE mass activity targets, it seems that the priority should remain developing high-mass-activity catalysts rather than scaling the MEAs. Additionally, the MEA results were poor, so improving the MEA performance is required, but not needed to scale.
- Reasons for the disappointing performance of ITO-supported catalysts in the MEA should have been addressed much earlier in the project. They are almost certainly related to Sn leaching (and possibly the instability of In, as well). Cost modeling and MEA fabrication scale-up are out of place as long as supports are not stabilized. In the time remaining, the research effort in this project should concentrate on the development of new support formulations.
- To complete this study, the researchers have to complete the initial plan, but they should also test 15 µg $/cm^2$ or less.
- The project needs to focus on improving the specific activity. The Pt loading is still very high compared to the reference catalyst.

Project strengths:

- The collaboration between academic and industrial partners was strategically executed and has led to valuable feedback on the utilization of metal oxide materials as feasible alternatives to carbon supports. Notable progress was achieved in support durability.
- The project has looked at several different materials and processing conditions. There is a reasonable premise for the materials chosen for investigation.
- The interaction between IIT and Nissan is very strong and well-focused. The team has been very successful in producing durable supports.
- The project features a good team of experts and good collaboration.
- The corrosion stability of these supports is the major strength of this project.
- The strength of the project is the good/clear presentation.
- Investment in alternative supports is needed and should continue.

Project weaknesses:

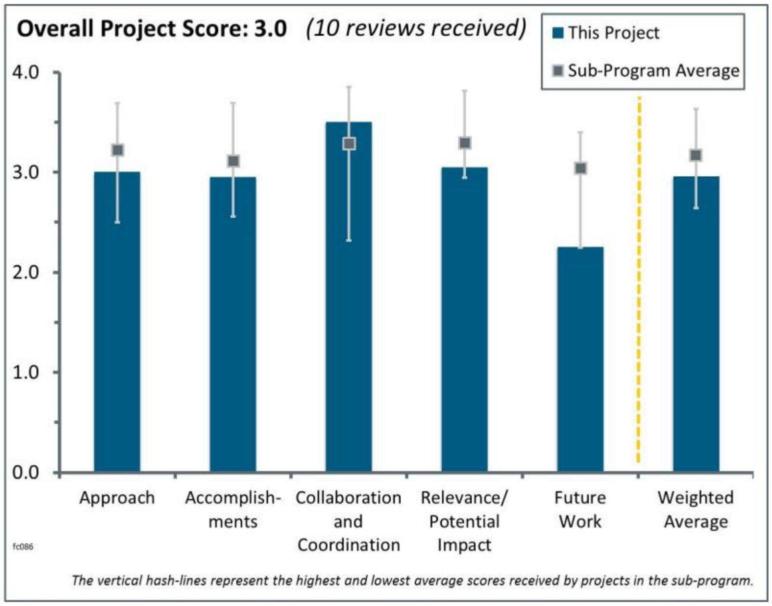
- Besides the obvious MEA performance issue, more attention needs to be given to the morphology and interaction of the particles and support in order to make further headway on improving activity and reducing Pt loading. It would be very useful to study the catalyst morphology after testing to further validate the durability results.
- The stability of these support materials in the fuel cell environment needs to be demonstrated, including at potentials from 0 for 1.0 V, and with exposure to O_2 and hydrogen. These are exposed not only to acidic media, but also to varying potentials and both oxidizing and reducing atmospheres.
- Weaknesses of the project are the relatively limited team and the lack of specific understanding of degradation mechanisms and fundamental materials limitations.
- Even though performance is satisfactory, the total cost analysis does not indicate a path forward for implementing the results in technology.
- To date, this project has not provided any materials of potential value to polymer electrolyte membrane fuel cell technology.
- Selected oxides are not stable under Nafion; Pt loadings are too high.
- The low ECSA is a major weakness.
- This project features the wrong fundamentals.

Recommendations for additions/deletions to project scope:

- The project needs to focus on alternate methods for deposition of Pt to achieve higher mass activity. The researchers should determine if the lower performance could be caused, in part, by the alloying of Pt with the support.
- Due to the instability intrinsic to the chemical formulation of ITO and the well-established detrimental effect of Ru on the ORR, both supports need to be abandoned and new formulations need to be developed.
- Researchers should limit efforts to ITO with an increased focus on surface studies—this is in line with the proposed future work. The team should not pursue scale-up of materials or large-cell MEA studies.
- The researchers should develop a fundamental understanding of the interaction between the metal oxide support and the ionomer.
- This project should concentrate on improving the Pt mass activity and increasing the Pt ECSA. Otherwise, the MEA design and performance, and especially scaling, is irrelevant.
- The team should select the low-coverage oxides for further study.

Project # FC-086: Development of Novel Non-Platinum-Group-Metal Electrocatalysts for Proton Exchange Membrane Fuel Cell Applications

Sanjeev Mukerjee; Northeastern University



Brief Summary of Project:

This project will develop new classes of non-platinum-group-metal (PGM) electrocatalysts to meet or exceed U.S. Department of Energy (DOE) targets for activity and durability. The approach includes: (1) development of novel reaction centers for oxygen reduction, (2) controlled metal support interactions for efficient mass transport of charged and solute species, (3) control of the electronic structure of reaction centers, (4) computing transport and reaction dynamics, and (5) spectroscopy to elucidate electrocatalytic pathways in complex reaction centers and quantify degradation with element specificity under in situ operating conditions.

Question 1: Approach to performing the work

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

- The team has developed a multifaceted approach for the synthesis of non-PGM electrocatalysts. The researchers have demonstrated scale-up of their catalysts. The approach involves modifications of the catalyst at both the nanoscale and the mesoscale to address intrinsic activity as well as transport issues. This is key to the success of this team.
- The approach is well thought out and takes into account multiple barriers that need to be addressed in order to develop active and durable non-PGM catalysts.
- The approach has been adequate for the magnitude of progress that must be made to achieve success. It is not adequate for the last year of the project, with so much distance still to cover.
- The elements within the project approach are all good, but they are huge in scope if they are to be as effective as implied by the relevance and impact statements. The targets for evaluating the success of the catalysts, based on volumetric current density, are not sufficient for effective screening and determining go/no-go for automotive applications. If the project proposes to be as effective as implied by the relevance and impact statements, then the catalysts should be evaluated in the same way as PGM catalysts, both from performance and durability standpoints. This does not mean that a low-current-density performance would automatically be declared a failure. What is important is that the actual gap between the current performance and where it has to be to be realistically effective is made clear. This will allow better judgment of what is critical to be worked on, whether it is fundamentally possible to overcome the gaps, whether it is worthwhile. Finally, it is not clear why this approach will still not be subject to the fatal flaw of carbon corrosion at cycling potentials up to 1.5 V as expected in starting/stopping.
- The project uses a solid approach combining advanced synthesis, state-of-the art analytical tools, and molecular modeling to make significant improvements to non-PGM catalyst performance. The researchers have also appropriately focused on high-current-density performance in air. The biggest flaw in this approach is the use of Fe in the catalysts, which can accelerate membrane degradation if it leaches from the catalyst. It is strongly recommend that the team eliminate Fenton's active transition metals.
- The technical approach is comprehensive and seems reasonable to achieve the goals of the project. Because it is now well established that in addition to the low activity, durability is a serious issue, all samples should be screened for corrosion by using a simple test by rotating disk electrode (RDE) where the catalyst is

subject to a sweep from 0.0 V to 1.8 V in acid and the onset of corrosion currents are compared to that of Pt/Ketjen Black and/or Ketjen Black. This might save a lot of time before activity studies are started. Activity data should include the catalyst layer resistance obtained from electrochemical impedance spectroscopy (EIS) over a range of frequencies under N_2. This will provide a measure of the thickness of the catalyst layer, which is a serious problem and has been bypassed by using an areal activity instead of volumetric activity. If the catalyst layer is too thick, it will have high resistance and high O_2 diffusion overpotentials. Also, the stack components will increase in number or size, leading to a high stack cost even if the catalyst has zero cost.

- The approach includes several classes of non-PGM catalysts from the various partners. Screenings were performed with O_2 RRDE measurements with peroxide selectivity measurements included, at least for some of the catalysts presented. The durability testing by Nissan is also very interesting. The non-PGM catalysts tested do well in load cycling, but understandably poorer in start-stop testing. Power plants that employ known voltage management techniques during start-stop may enable these catalysts. However, it would be interesting to do some microstructural characterization of the accelerated-tested catalysts to see if they maintain their structure. The approach could be augmented with more diagnostic performance testing with varying O_2 contents and helox in order to better calibrate the Michigan State University (MSU) modeling. Also, more microstructural characterization of the fabricated electrodes would be helpful. It is not readily apparent why catalysts that achieve the 2010 volumetric current density target still struggle so much in membrane electrode assembly (MEA) testing using 2.5 bar, fully humidified air. It seems that the C and ionomer ratios make a big difference, so more focus here may yield better-performing electrodes. Perhaps adding hydrophobic constituents may improve the porosity and flooding tendencies.
- The project has shown useful results using relevant tools and includes some materials of promise for the development of non-PGM catalysts, but the vision of the overall approach was not clearly conveyed in a way that leads to a high degree of confidence in the rationale of the materials set being investigated. More specifically, slide 6 shows four distinct approaches for Tasks 1.1–1.2, but the three approaches presented in slide 7 do not specifically map to those in slide 6, and it is unclear how they are connected, what synergies they have, or how decisions have been made on choices for topical areas/approaches of focus within the material development effort.
- The emphasis on scale-up (at Pajarito Powder, LLC [PPC]) of materials with readily measurable kinetic activity and high-current-density performance is something the non-Pt field really needs and should significantly advance the field by allowing the start of detailed mechanistic studies. This project originally planned too wide a range of approaches, some of which are dubious but probably sounded good to some reviewers of the original proposal (e.g., biomimetic oxygen reduction reaction [ORR] catalysts—standard FeNC non-Pt ORR catalysts started off biomimetic, but needed to diverge significantly from hemoglobin to obtain half-decent activity and durability under realistic fuel cell conditions), but the project seems to be settling down to a more-restricted set of reasonable activities. The high-current-density target of 1 A/cm^2 at 0.4 V would be useless in practical application due to low efficiency and excessive heat generation. It needs to be stressed that this “target” is just a starting point needed to get the work off the ground. The target for transportation applications, the highest-value market for fuel cells, would likely be at 0.7 V or higher. Because these are C-based catalysts, they should not be expected to provide a materials solution to start-stop (i.e., the team should not spend a lot of time testing cycling 1.0–1.5 V). Such catalysts would clearly require systems mitigation of these issues, which should be possible.
- Although the results obtained are very good, or excellent, the approach of involving a number of dissimilar molecules, treated in a variety of ways (e.g., ball milling, pyrolysis, metal-organic framework [MOF], and blending) looks like a trial-and-error procedure. A more coherent choice of molecules, supported by an underlining theory, would allow for more-efficient research.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- The progress toward demonstrating the DOE non-PGM targets and understanding possible mechanisms has been impressive. But when the accomplishments are looked at in terms of whether they (the accomplishments) help overcome the larger barriers to automotive fuel cells, they are only modest at best. Automotive applications require cell potentials above 0.6 V, and these current densities under air are an order of magnitude too low. An attempt should be made to project the highest possible reduction potential and site density to estimate the highest possible current density such a non-PGM system could give and see if an order of magnitude improvement is possible. Also, the cathode non-PGM catalysts should be evaluated in MEAs that have Pt anode loadings consistent with, or slightly higher than, what PGM cathodes are required to use in order to hit the 0.125 mg/cm^2 of total PGM targets. The high anode loadings used to evaluate these non-PGM cathodes can skew the results. Also, as higher-voltage cycling or stop-start cycling is done to more realistically evaluate the non-PGM cathode stability (e.g., to 1.5 V), Pt dissolution from the anode and diffusion to the cathode will occur and further confuse and skew the results.

- The researchers made excellent progress last year, in particular regarding catalyst activity. Catalyst stability, however, remains a major problem, further aggravated by the H_2O_2 observed. The understanding of the behavior of these catalysts is far from complete—in particular, the role of the Fe_2+/Fe_3+ redox couple in determining the open-circuit potential. It appears that a mixed potential is operative. If one process is corrosive in nature, that is not good news for stability. It remained unclear why CN- adsorption causes only a partial blocking of active surface, even at its high concentration in the solution phase.
- The project has done an excellent job achieving the very challenging targets for hydrogen/air fuel cell performance. The principal investigator (PI) recognizes that there is still a long way to go and has ideas to hopefully bridge the gap to meet the ultimate automotive targets. It is still a longshot to meet these ultimate targets, but the potential benefit is worth the effort.
- These non-PGM catalysts have come a long way from where they have started. However, they are still a ways away from being practical replacements for PGM on cathodes. The problems do not seem to lie in the fundamental ORR kinetics. The mechanistic studies are interesting, but it is not clear how these insights are leveraged toward other potential non-PGM catalyst formulations.
- Very good progress has been made toward achieving the DOE activity targets at 0.8 V, both in O_2 and air. The high-performance target (at 1 A/cm^2) has not been achieved. The researchers have made significant progress toward revealing the mechanisms of the ORRs on non-PGM catalysts, as well as excellent progress with catalyst scale-up.
- The team has made significant progress on multiple fronts. The researchers have synthesized large batches of catalysts, and they have also investigated the durability of these catalysts. These catalysts, along with conventional Pt catalysts, do not appear to survive during start-stop cycling.
- Reasonable performance and results have been obtained with a number of materials. The mechanistic studies and spectroscopy results are by far the most interesting. Unfortunately, the PI left little time for detailed discussion regarding the mechanism and active site; instead, the PI spent too much time on less interesting topics such as background introduction and scale-up, which could have been handled much quicker. From just the slides and question-and-answer (Q&A) session, the validity of the proposed mechanism and active site findings is harder to judge, but even the inclusion of such a mechanism with some supporting data is useful to the field.
- Meaningful progress has been made in generating non-PGM cathodes that give measurable performance at high current density. The team has also made good progress on scale-up, which can be a very difficult problem with this class of catalysts. A start has been made on mechanistic interpretation, with a claim that two types of sites are needed in acid. The conclusion that ORR activity correlates with the potential of the Fe II–III couple for FeNC systems seems inconsistent with some reliable data in the literature. Active catalysts of this class often do not show any discernable Fe II–III couple when cycled in inert-purged acid.
- It is clear that some elements of progress have been demonstrated, but much remains to be done in order to achieve targets.
- There are no slide numbers on the slides themselves. Slide 13 has DOE standard conditions (0.5 bar O_2?) on the figure, which are questionable. It looks like the slides were made by different people and were not checked carefully by the PI. It would be good if Pt/C data was superimposed on all ORR I–V plots for comparison under the same conditions. It is difficult to judge the results without it. Slide 16 has units of pressure as bar-g! Units should always be the International Standard of Units (SI), which are absolute units and do not have gauge pressure, such as 100 kPa. (Otherwise, the ambient pressure in Nm needs to be known and reported.)

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- The project involves multiple institutions that are all contributing to the project. The collaboration among the team members is excellent, leading to a synergistic research effort.
- There appears to be a strong collaboration among the team that includes academic, industrial, and national laboratory partners.
- The project's collaboration involves several leading specialists in this area of electrocatalysis, one catalyst producing company, and one car manufacturer. It is well structured and coordinated.
- A good mix of academia, suppliers, national laboratories, and original equipment manufacturers (OEMs) are involved.
- The team members are very strong leaders in the fields of electrochemistry, spectroscopy, and non-precious catalysis.
- This is a very well-coordinated effort between academia and industry.
- The collaboration seems sufficient, especially for scale-up.
- This appears adequate thus far.
- This is a complex project, but it seems to leverage the strengths of the partners particularly well. There is good focus on scale-up of the catalysts by PPC. One will need to see more impact by Nissan toward the end of the project in testing larger-scale MEAs in cells and stacks.
- Collaborations generally seem to be working well. They are a bit heavy on academia and small spinoffs. Interactions with the non-funded collaborator, Canetiq, should be explained. The role of theory within this project seems a bit unclear from the presentation. It appears that MSU's modeling of electrode layer performance needs to be better integrated into the project as a whole, if it is to continue.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- If durability issues at start-stop cycles and performance targets at 1 A/cm^2 are achieved, the project has the potential to significantly advance progress toward commercialization of polymer electrolyte membrane fuel cells (PEMFCs).
- Developing non-PGM catalysts is very important for broader application of fuel cell catalysts.
- The removal of PGM from fuel cell cathodes would be a major development toward the cost and supply viability of automotive fuel cells. While more progress is needed, this project has made some impressive strides.
- Development of these catalysts will take some time, and the potential impact could be delayed.
- This is a more exploratory effort in the DOE Hydrogen and Fuel Cells Program, but it is aligned.
- There is an obvious benefit to eliminating PGM from the electrode. This is a high-risk, high-reward project.
- This project addresses the holy grail of ORR research: non-noble-metal catalysts with adequate activity, performance, durability, and robustness. A high-current-density target of 0.4 V at 1.0 A/cm^2 is way too low for practical application and does not fulfill the quoted "Relevance to DOE Mission" on slide 3: "This will enable decoupling PEM technology from Pt resource availability..." It is, however, a realistic goal within the original time frame of this project. This project, and others, has made substantial improvements to the engineering status of non-PGM catalysts, but very substantial reengineering of fuel cell systems would still be required to allow use of such radically different catalysts in practical applications, even if all project goals are met. Success of this and other DOE Office of Energy Efficiency and Renewable Energy non-Pt catalyst projects could facilitate the start of serious work to address durability and robustness issues with non-PGM catalysts.
- Non-precious-metal catalysis has made meaningful progress toward improved performance and durability, but they remain far below the levels required for MEA transportation targets. With improvements in Pt-based catalysts resulting in the potential to achieve ultra-low loadings in vehicles, the cost driver to replace Pt is greatly reduced, meaning that non-PGM materials will have to essentially be equal to Pt-based systems; this is an unlikely advance, even with the significant advances that have been made to date.

- Because of their low activity and durability, non-Pt catalysts have a long way to go before they can be considered useful for automotive PEMFCs; however, they need to be worked on for the next 10 years or so. At some point, there should be a clear break in funding if it is realized that non-Pt catalysts will never be stable enough for load cycling durability or if the Pt loading is low enough (10 g/100 kW) that the Pt cost is not a concern and, with recycling, is not an issue anymore. It is a bit confusing and disconcerting that the targets for activity are reported in different ways in different slides, with incomplete information on temperature, P_{O2}, etc. The targets are being changed from volumetric to areal units, but it should be clearer and well defined. RDE targets/benchmarks do not exist and need to be established for screening catalysts at a rapid rate.
- The relevance and impact seem to be highly overstated. If the project meets all of its objectives, it would not accomplish the things stated in the relevance and impact sentences. These sentences seem to be generic relevance and impact statements used by DOE to justify working on non-PGM catalysts. However, in order for the catalyst materials to overcome those automotive barriers, they will have to meet all of the much more rigorous performance and durability tests that PGM catalysts have to go through, which are outside the scope of this more basic research effort.

Question 5: Proposed future work

This project was rated **2.3** for its proposed future work.

- The future work is well directed, but it needs to focus on better definition of active sites and methods to increase active site density. It is not clear if the large Fe nanoparticles play any role in the catalyst performance.
- There was no future work slide. The presentation by the PI was professorial—it was similar to a university lecture, and he did not finish his talk and go through all the slides, because he got caught up in long discussions about the work, but not on the work. It would be nice if this changes—it is becoming a tradition. There are far too many technical slides presented together in a rush, and it is hard to figure out the accomplishments and future work.
- No future work slide was provided. From the plan, it seems like stack testing is part of the future work. Catalyst scale-up and stack testing is premature at this point. Significant performance gains are still required beyond the project’s high-current-density target. Rather, the project should focus on finding ways to increase the active site density and developing active catalysts that do not contain Fe.
- The future work is not clearly laid out, just the completed percentage of milestones provided toward the beginning. Therefore, it is not clear exactly what steps will be taken to address the high-performance-target gaps. The project may be trying to do too much—it seems like a focused project will either focus on structure-function determinations of novel catalysts for the ORR, or attempt to optimize the down-selected University of New Mexico catalyst. This reviewer would vote for the latter. If the high-performance gap can be significantly bridged, then these catalysts may be taken more seriously by OEMs.
- The presentation lacked the mandatory future work slide, and due to time management the speaker rushed through the slides at the end of the presentation. Based on comments made throughout the presentation and the Q&A session and the approach demonstrated by the team to date, it is expected that the future studies will have some relevance; however, the lack of a future work or remaining barriers slide is a major oversight by the PI.
- The proposed future work is not described, but it is linked to the milestones and go/no-go decisions. Modeling, density functional theory calculations, and increased spectroscopic characterization will provide a new quality to the data.
- The efforts required to achieve targets in this final year are quite large, and the plans to achieve them were not documented and explored.
- No future work is provided. Quarterly milestones that are related to future work identify only milestones, but not the path to accomplish these milestones.
- Unpyrolized MOFs seem to be a big divergence from what has been productive in the past. More attention could be given to the development of methods to quantify the volumetric density of active sites.
- There were no slides addressing future work.

Project strengths:

- This project features a notable mix of synthesis, performance evaluation, and characterization. It also features scale up of catalyst preparation to levels that could enable detailed mechanistic, durability, and robustness testing.
- Project strengths include the rich set of non-PGM approaches, the focus on scale-up, and the characterization of fundamental ORR activity.
- Strengths of this project include its good mechanistic studies and presentation of a plausible ORR mechanism with supporting data.
- The project features good collaboration within a strong team. In addition, a broad range of characterization techniques are used.
- Using advanced characterization techniques and modeling allows for fast progress toward revealing reaction mechanisms and identifying reaction active centers.
- The researchers have developed novel approaches for the synthesis of non-PGM catalysts and demonstrated performance that successfully passed the go/no-go benchmarks.
- The PI has assembled a strong team that is using a sound approach to address the most challenging aspect of non-PGM electrodes—high-current-density performance in air.
- Strengths of the project are the clever PI and the good set of collaborators.
- This project has a good approach.

Project weaknesses:

- There is no clear path to achieve the performance target at 1 A/cm^2. Iron nanoparticles even surrounded by carbon shell might cause durability issues due to Fe dissolution in a strongly acidic environment and generating peroxide radicals. Peroxide generation has to be reported not only as a ring current, but also as H_2O_2 yield on slide 22.
- The voltage target for high current density in air is too low to ensure any practical utility. The project needs more emphasis on the durability of high-current-density performance in air at practical operating voltages (above 0.6–0.7 V).
- No clear direction was presented regarding a hypothesis for further improving performance/durability, nor were there clearly planned steps. There is a lack of clarity regarding future direction.
- The catalyst is quite heterogeneous and contains multiple sites and functionalities. The lack of a definition of the active site may hamper future efforts at improving the catalysts.
- The slides need work, as do the benchmark values reported and the general comparison to a solid baseline ORR curve.
- Project weaknesses include the basic limitations of non-PGM materials and their poorly understood site densities and ORR turnover mechanisms.
- There is a lack of coherence in the systems selected for detailed studies.
- One weakness is the apparent dependence on Fe for high ORR activity.
- The project needs more fundamental work on electrode optimization.

Recommendations for additions/deletions to project scope:

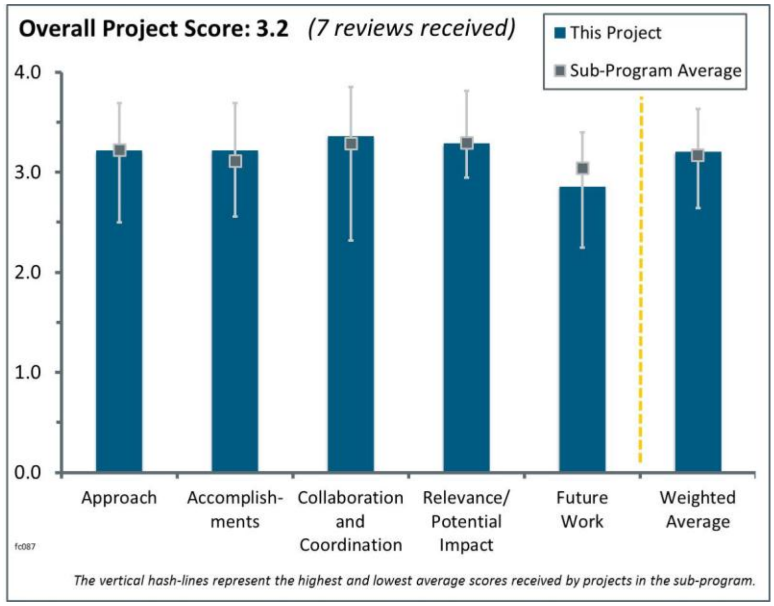
- The researchers should make the targets more rigorous and consistent with the PGM catalyst approaches, because it is the application that is important, not the explicit approach to get there. Therefore, they should incorporate high-voltage cycling tests to address the potentially fatal flaw of carbon corrosion at 1.5 V. They should also add a goal to determine the maximum projected activity capable with this catalyst approach based on realistic estimates of redox potential limits and site densities per unit volume, and then see if it is even remotely possible to get where PGM catalysts are today.
- The team should add an explicit attempt to quantify the concentration active sites in the catalyst layer (this is admittedly very difficult). It should also actively coordinate with the more recently initiated Los Alamos National Laboratory non-PGM catalyst project to rationally divide up tasks. It should be careful not to expend too much time on non-pyrolyzed catalysts with inadequate performance and durability.
- The team should increase the understanding of the behavior of Fe_2+/Fe_3+ and the role of H_2O_2 in stability. Partial inhibition by CN- offers additional information on active sites.

- The PI should move away from Fe-containing catalyst systems that are likely to accelerate membrane chemical degradation.
- Durability tests should be conducted at 0.05 mgPt/cm^2 loading at the anode.
- The project team should conduct further characterization of the active sites and site density.

Project # FC-087: High-Activity Dealloyed Catalysts

Anusorn Kongkanand; General Motors

Brief Summary of Project:



The goal of this project is to reduce catalyst cost while achieving the required durable performance. The objectives of this project include: (1) demonstrating reliable oxygen reduction reaction kinetic mass activities greater than the U.S. Department of Energy (DOE) target in hydrogen/oxygen fuel cells; (2) using manufacturable synthesis and dealloying procedures; (3) achieving high-current-density performance in hydrogen/air fuel cells adequate to meet DOE heat rejection targets and Pt-loading goals; (4) demonstrating the durability of the kinetic mass activity against DOE-specified voltage cycling tests in fuel cells; (5) determining where alloying-element atoms should reside with respect to the catalyst-particle surface for durable activity; (6) demonstrating the durability of high-current-density performance; and (7) scaling up to full-active-area fuel cells, to be made available for DOE testing.

Question 1: Approach to performing the work

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

- The General Motors (GM)-led team has an excellent approach to membrane electrode assembly (MEA) development and is addressing the technical barriers of cost, durability, and performance. The project is key to MEA development and meeting DOE fuel cell performance objectives. The GM-led project integrates efforts from Technische Universität Berlin (TU Berlin), Johnson-Matthey Fuel Cells Inc. (JMFC), the Massachusetts Institute of Technology (MIT), Northeastern University (NEU), and George Washington University (GWU).
- This project is nearly complete. It appears that significant science and technology accomplishments were made by all partners.
- The approach of this project is based on the well-established expertise of each participant in order to enable the dealloying protocol in practical nanoscale materials.
- The dealloyed catalysts appear to provide the desired manufacturability and long-term durability.
- The linchpin of this project is the core-shell alloy study by TU Berlin, from which a number of important publications were produced. The net outcome, however, led to a significant transition metal reduction in alloy through acid leaching, causing concern about the merit of such an approach. The Pt will undergo constant phase and oxidation state changes under polymer electrolyte membrane fuel cell operating conditions, and the transition metal will continue to be leached out. The team should verify at what level, if at all, the transition metal could be stabilized in such a structure.
- This is elegant research in electrocatalysis; however, it is on the type of catalysts that have been thoroughly investigated over at least a decade. There is still no definite answer to the question of whether dealloyed catalysts provide a long-term advantage over single-component Pt catalysts. It is unclear whether similar or better performance could be achieved using Pt catalysts with porosity induced by other means than leaching a transition metal.
- DOE funds a number of projects pursuing dealloying approaches. The claims of dealloyed core-shell structures that will be stable over long-term fuel cell operation are doubtful.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The project team has made good progress toward exceeding the DOE fuel cell goals for MEA catalyst loading and durability. The $PtNi_3$ catalysts are well understood, and the GM team has done an excellent job in transitioning the advanced catalyst to MEAs.
- This project is making excellent progress in using expertise from multiple partners to derive fundamental understanding that could lead to improved catalysts.
- The team has been actively engaged in the synthesis and testing of the dealloyed catalysts. The researchers appear to be devoting a significant amount of time and effort toward the characterization and fuel cell testing of the materials. The distribution of particle sizes and shapes is huge. The data from transmission electron microscopy (TEM) cross-sectioning is quite weak. The samples are very heterogeneous and it is hard to draw conclusions. The surface areas of the catalyst were modest. The x-ray absorption spectroscopy (XAS) Pt shell penetration potential data is unproven. Strain measurements via extended x-ray absorption fine structure (EXAFS) are inaccurate compared to measurements made by diffraction techniques. The diffraction data presented was not analyzed for average lattice parameter or particle size. This data could be cross correlated to validate the x-ray absorption fine structure (XAFS) results. The accelerated stress test (AST) testing conditions were changed from 0.6 to 0.925 V to improve the durability. This is not standard protocol.
- The accomplishment was excellent and the team met most of the milestones. The performance improvement was clearly demonstrated. The characterization study relied heavily on an imaging method that is not a statistically meaningful. X-ray diffraction (XRD) and EXAFS studies are good, but they do not directly address the core-shell structure. The work by JMFC was somewhat hodgepodge and not different than what has been done by others in the past.
- The amount of experimental data over the past year has been impressive. At the same time, questions remain about the stability of PtNi (and PtCo) catalysts and, thus, their long-term advantage over transition-metal-free formulations. The impact of multivalent cations on ionomer conductivity is also unclear. Lowering the voltage limit to 0.925 V to ensure better durability, especially at lower loadings, does not seem like a viable approach.
- The accomplishments appear to be adequate in comparison to targets. It is not possible to say anything about durability. It also is difficult to know what the absolute status is. Manufacturing partner success is a plus.
- The progress of the project has been made by introducing additional Pt transition metal alloys besides PtNi. Particular improvements have been made by achieving better control of particle size distribution compared to the previous year.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- GM has led the project team well. The milestone/go/no-go chart gives a good overview of each institution's role. The coordination with JMFC has worked well.
- The collaboration was excellent, with meaningful contributions from multiple institutions.
- There were a large number of participants, and it appears that all of them made progress.
- The project brings together many partners, with the goal of using expertise in different techniques. However, some of the results seem contradictory. The Pt/Ni core-shell structure is different for the catalysts prepared at TU Berlin versus GM. There appear to be inconsistencies between the EXAFS and TEM-energy dispersive spectroscopy (EDS) data. While the Ni content leads to increased initial activity, there is an optimal ratio, as implied by the volcano plot. The researchers need to characterize the end of life (EOL) catalyst to make their conclusions because these structures likely evolve considerably during use.
- While the level of contributions to the project by some partners, such as NEU and GWU, remains unclear, the project appears well integrated. This is a team of organizations with established leadership roles in their respective fields of research.

- The collaboration effort is satisfactory, although the coordination does look a bit compartmentalized. It is hard to tell how much cooperation and interfacing is occurring within this project.
- For some participants, particularly MIT, the role in the project was not obvious from the current report.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The project is highly relevant to the Fuel Cell Technologies Office's (FCTO's) goal for fuel cell cost reduction and durability improvement.
- This project is very well aligned with the DOE objectives described in the FCTO Multi-Year Research, Development, and Demonstration Plan.
- The research being led by GM is critical for meeting DOE Hydrogen and Fuel Cells Program (the Program) goals. Addressing the operation of advanced catalysts in fuel cell stacks is particularly important.
- This project provides demonstrated performance on industrially relevant catalysts that can be manufactured on a large scale.
- The project mainly helped to validate already known mechanisms of dealloying and activity enhancement in a more controlled manner. It also implemented developed materials in MEAs. Protocols in Pt alloy catalyst pretreatment are now well established for practical use in fuel cell technology. The project managed to span from atomistic insight to integration of materials into fuel cell electrodes and stacks.
- This effort does appear to duplicate the Argonne National Laboratory (ANL)-led effort on dealloyed catalysts without achieving the same control of materials synthesis or level of characterization. However, it does offer more fuel cell testing.
- This project is in alignment with the goals of the Program.

Question 5: Proposed future work

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

- Proposed activities are in agreement with the duration of the project, which ends in September 2014.
- A thorough assessment of the effect of transition metal(s) on the electrode ionomer and polymer electrolyte membrane should have been one of the primary objectives of this project from its inception. That effect should be studied between now and the project end date in September. If that is not planned, inclusion of that task in the ANL-led MEA project is the second-best option.
- The investigation of core-shell alloy stability should really have been completed within the framework of this project. In fact, it should have been done at the rotating disk electrode (RDE) level in the beginning of the project because it serves as the foundation for the rest of the effort.
- The proposed future work transitions to an ANL-led project. MEA/stack development is expected to continue with a focus on the PtNi system. This transition is not entirely clear.
- The team is aware of some of the shortfalls of the effort and has defined a future work effort to attempt to overcome them.
- A better understanding of the working catalyst is needed.
- The stability testing should be completed.

Project strengths:

- There is a strong team that includes leading institutions from industry and academia. The project has shown the ability to employ fundamental principles in material development.
- A strength of this project is its combined synthesis and fuel cell testing rather than reliance only on RDE data.
- The combination of catalyst synthesis, testing, and characterization techniques is a strength of this project.
- Strengths of this project include its milestone delivery and multi-institution collaboration.
- The main strength of this project is the high quality and thoroughness of the performed research.

- GM and JMFC have formed a strong group.

Project weaknesses:

- The lack of control throughout the project of the particle size distribution and the homogeneity of elemental composition turned out to be a critical factor that limited the ability to interpret results.
- The main weakness is the reliance on the still-unproven long-term benefit of dealloyed catalysts, especially in the ionomer environment of the fuel cell cathode.
- Most characterization presented is on the initial catalyst, not the working catalyst.
- There is a lack of control over materials homogeneity.
- No clear strategy was identified for long-term core-shell catalyst stability.
- The transition to ANL is not clear.

Recommendations for additions/deletions to project scope:

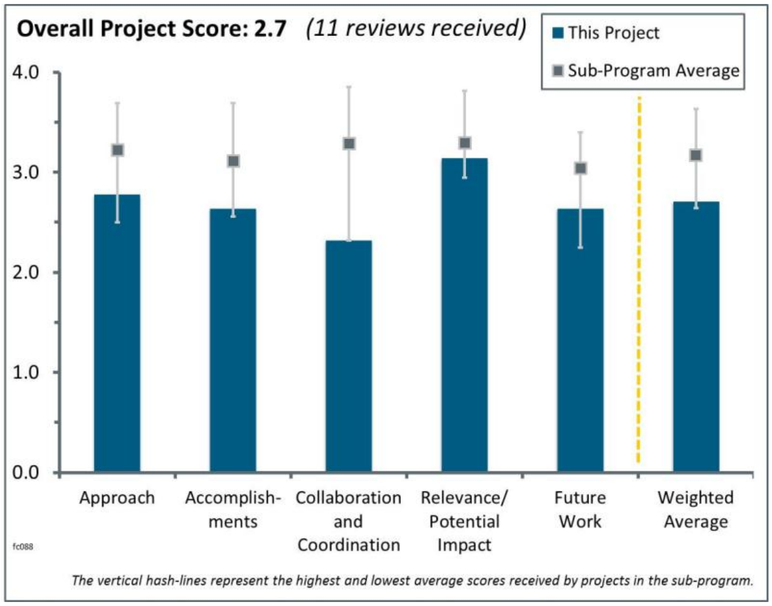
- The researchers should relate the EXAFS, XRD, and TEM-EDS data to derive a better picture of the working catalyst.
- The project team should wrap up the project with some definitive conclusion, particularly on the prospect of long-term stability.
- The project is ending soon, and the remaining proposed work is appropriate in scope for that short period of time.
- The team should continue the MEA development work with ANL and focus on stack testing at GM.

Project # FC-088: Development of Ultra-Low Doped-Pt Cathode Catalysts for Polymer Electrolyte Membrane Fuel Cells

Branko Popov; University of South Carolina

Brief Summary of Project:

The goal of this project is to develop a low-cost catalyst with optimized average mass activity, stability of mass activity, initial high current performance under hydrogen/air (power density), and catalyst and support stability able to meet 2017 U.S. Department of Energy (DOE) targets. The objectives are to: (1) develop a cost-effective, high-volume synthesis procedure to manufacture highly stable activated carbon composite catalyst (A-CCC) support, and (2) to develop low-cost procedures to synthesize a catalyst with enhanced activity due to the synergistic effect of pyridinic nitrogen catalytic sites from the support and suppressed Pt-lattice catalyst.



Question 1: Approach to performing the work

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

- The approach is focused on overcoming barriers by lowering the cost of Pt and developing a more durable catalyst support combination.
- The approach is to develop the non-corrosive fuel cell support to enable high mass activity with low platinum group metal (PGM) loading that is close to the DOE target.
- The hybrid catalyst approach to use a stabilized support with Pt deposited on top looks like a promising approach to improve the stability of the Pt nanoparticles. This approach combines the stability of the support and the anchoring site to improve the stability of catalysts in the fuel cell environment.
- The principal investigator (PI) should consider whether the approaches tried over the past two years are likely to achieve targets in a realistic fashion. If not, then the PI should make a well-reasoned move to another approach. It is known that shrinkage is still a problem; activity loss during tests is also still a problem. The approaches in this work have been to pyrolyze materials with carbons to place metals in the carbon, to vary temperatures of pyrolysis, to modify the surface of the carbon to include oxygen or nitrogen containing groups, or to use a polyol and modified polyol process (but no details were presented on this). This is not a winning approach, especially not when attempting catalyst and support development and potential scale-up.
- The approach is based on creating a transition-nitrogen-carbon electrocatalyst and using it as support to deposit Pt and to create a Pt alloy catalyst. It is unclear why so many synthesis steps are needed to achieve another type of PtNi material like that studied by other projects. In addition, even though the initially created materials meet DOE target goals, no clear progress has been made relative to last year's report. The best material so far is the same as reported last year. The uniqueness of the approach is described by vague terms such as "controlled heating," "modified" synthesis, etc.
- The approach of this project as stated appears to combine two synergistic effects to address improved catalyst support durability and improved catalyst activity. It was not entirely clear from the presented material what the experimental evidence is for whether such synergism really exists. The Pt-A-CCC still has carbon corrosion to contend with at the highest potentials (1.5 V) expected during start-stopping of vehicles. It is therefore not clear how this approach will provide any significant advantage from a durability standpoint. It is good that the project has a focus on both the catalyst and the support. This is an interesting

concept. The Pt-free activity of the support is quite surprising. However, it is not clear how, in the end, one gets a catalyst that is intrinsically different from commercial PtCo/graphitized carbon, except for the presence of Co doping of the support. One advantage of the approach stated on slide 8 is that it increases the Brunauer-Emmett-Teller (BET) surface area and pore size uniformity for uniform Pt deposition. The BET surface area reported is only moderate at 200 m^2/g of support material, and the Pt catalyst surface area reported of 31 m^2/g-Pt appears a bit lower than commercial Pt/C on highly graphitized carbon supports.

- This work focuses on a parallel improvement of catalyst supports and Pt catalysts. On the support side, the researchers used doped material with pyridinic nitrogen sites to improve the Pt dispersion and treating the support under high temperature to increase stability. On the catalyst side, the researchers sought the synergic effect of Pt/non-PGM materials for oxygen reduction reaction (ORR) catalysis and attempted to maintain high Pt dispersion via Pt-transition-metal interaction. The development concept is mostly sound and reasonable although it is not necessarily innovative, because most of the ideas have been known in the field for quite some time. The PI should address the following concern regarding his approaches: the synergic effect between Pt and non-PGM materials needs to be elaborated through the experiment, and the contributions -of each of the components at different regions (kinetic and mass-transport limited) are unclear. The PI should also present experimental data to elucidate this point, particularly on the "support" side.
- The team is well focused toward addressing the key barriers of performance, cost, and durability. The work to develop alloys appears novel and logical, based on the carbon composite catalyst (CCC) fabrication approach, and it has been reasonably successful. It is of moderate concern that most or all of the polarization curves used to show the effects of durability accelerated stress tests (ASTs) are presented iR-free, suggestive of possible high-frequency resistance (HFR) increases. To eliminate this concern, the PI should present results with measured, rather than iR-free, cell voltages. Additionally, it is concerning that in some cases, hydrogen/O_2 polarization curves are shown rather than hydrogen/air; hydrogen/O_2 curves can mask degradation due to changes in electrode porosity and/or hydrophilicity, which can occur after ASTs. It is recommended that hydrogen/O_2 measurements be limited to evaluation of ORR activity and performance evaluated only under hydrogen/air.
- The approach of combining activated C with doped Pt is good for trying to lower Pt loadings, but close inspection of the approach generates some consideration of whether there are too many processes involved and what time is involved for each process. The reviewer wonders what the cost would be of both pyrolysis followed later by Pt doping. There appears to be a long heating time required for obtaining the single phase of PtCo for the doping (2–4.5 hours). Furthermore, the best activity catalyst in this project involves leaching for 12 hours. The project could benefit significantly from having a major industrial catalyst supplier as a partner to identify which processes are tenable from a manufacturing standpoint, and what lengths of time are realistic for each step. The reviewer questions whether the materials can be practically made at high volume.
- The project is focused on the development of Pt/Co catalysts through the embedding of Co in the C particle, but there are also aspects of catalyst development that seem focused on either non-precious catalyst development or the use of active support materials for improved Pt ORR. The order and arrangement of the slides as well as the lack of clarity in the presentation and apparent disregard for the required guidelines make this and every other aspect of the project hard to judge appropriately. The presenter claimed a benefit by putting the Pt down in a subsequent step and then getting Co into Pt through annealing, but from a fundamental scientific standpoint (and from the data presented), there is no reason to believe a significant improvement would be obtained compared to standard Pt/Co catalysts.
- The PI is adhering to the project's statement of project objectives (SOPO) but is resistant to evaluating materials on updated durability protocols. The hybrid catalyst is showing good results.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.6** for its accomplishments and progress.

- Excellent progress has been made toward generating high-performance alloy-CCC electrocatalysts. Leaching development has yielded substantial improvements on kinetics and membrane electrode assembly (MEA) performance. It is unclear, however, why the leaching results in substantial improvements in activity, and this should be studied further for possible further improvements in activity. The development work to generate homogenous Pt distribution via support surface modification appears to have been successful.

- Very good progress has been made toward synthesis of doped supports and combined catalyst-support structures. Durability and surface area of support was significantly improved versus commercial Pt/C. Synthesis of uniformly dispersed small Pt nanoparticles is not really an accomplishment—it has been done many times using other synthesis methods.
- The project has shown a catalyst (30% wt.%) with reasonable mass activity in a fuel cell MEA. While many catalyst projects do claim high mass activity in rotating disk electrode (RDE) tests, those claims are hard to realize in an MEA—this project has achieved that.
- Slide 21 shows that most technical targets have been met with a doped Pt/A-CCC cathode catalyst.
- The accomplishments of this project are generally considered good, based on the performance data provided. The PI should address the following questions regarding the achievements:
    - Catalytic activity of the "support" measured by RDE was reported. The PI should have also included the "support" activity study measured in MEA at the single-cell level.
    - The PI demonstrated an excellent improvement in stability against commercial Pt/C material. Representative data of the commercial Pt/C catalyst/MEA should also be included in the presentation for comparison.
    - The catalyst study demonstrated good Pt/Co alloy formation in situ. However, a better scientific rationale should be included to support such an observation.
    - The catalyst demonstrated good stability under a relatively mild cycling condition (0.6–1 V). For the Pt-based catalyst, a more severe aging condition should have been used.
- The 40% doped Pt/A-CCC catalyst was able to meet targets for the 1.2 V hold test. This is an improvement over the Pt-only results last year when the mass activity loss was 47%. Furthermore, the hydrogen/air iR-corrected performance at only 0.1 mg/cm^2 Pt appeared good (it would be helpful to see a Pt/C baseline). The team achieved 0.17 A/mg Pt for Pt/A-CCC after 400 hours at 1.2 V, and 0.23 A/mg Pt for Co-doped Pt/A-CCC after 30,000 0.6–1.0 V cycles. The end-of-test target is 60% of 0.44 A/mg Pt or 0.264 A/mg Pt. Therefore, the doped catalyst is just below the end-of-test targets after these cycles. However, there was improvement in mass activity versus last year (0.38 A/mg at the beginning of the test and 0.20 A/mg at the end of the test for 0.6–1.0 V cycling). The doped catalyst had 79 mV of loss at 0.8 A/cm^2 after 30,000 0.6–1.0 V cycles—this is beyond the targeted 30 mV loss. One point of confusion exists: the doped catalyst is described as having 0.408 A/mg Pt on slide 16, and then a doped catalyst is said to have 0.35 A/mg Pt on slide 19. This discrepancy should be sorted out. Catalysts are progressing toward end-of-test targets through incremental improvements in doping and leaching.
- The presenter could not explain the whole presentation in the time allotted, so the following comments are based on presentation material only. Catalyst durability—as indicated in the previous 2013 comment, the support should be evaluated based on new DOE protocols instead of a 1.2 V hold. Milestone—according to the 2013 presentation, the down-selected catalyst scale-up and short-stack testing were scheduled in Phase II. These two milestones could be valued points to evaluate the practicability and manufacturability of any type of new catalysts.
- It appears that a substantial amount of work has been accomplished with good progress toward meeting the project's assigned targets on performance and durability. It is confusing, however, that the polarization curves shown to declare progress toward the inverse specific power density target of 0.44 A/mg (PGM) all show an iR-corrected voltage scale. The inverse specific power density should be determined from as-measured polarization curves, not iR-corrected curves. The MEA anode and cathode loadings are not always stated, either.
- The activities of the best catalyst show insignificant improvements over commercial Pt/C, with a few metrics meeting the DOE 2017 target. No comparison is shown to the state-of-the-art Pt/C or the best Pt alloy catalyst. Even though the project reports on multiple characterization venues, no durability studies of aged materials are reported. The progress since 2012 and 2013 is very limited.
- From the presentation and the PI's remarks, it is unclear whether any meaningful advances have come in the past year. Much of the issue comes from the presentation itself, where the PI took 10 minutes to go through the first five slides and had less than half of his allotted time to discuss any data or findings from the project. He was unable to get through the last 10 slides of his presentation. From the table on slide 21, it

seems that the researchers have been able to approach the performance of state-of-the-art PtCo/C catalysts but are still far behind other advanced alloy nanoparticle approaches.

- This was very unclear from the presentation. Too much 2013 data was shown. The presenter indicated changes in the targets, but not why they were made or the impact on approach. The PI spent too much time railing against earlier presenter comments.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.3** for its collaboration and coordination.

- There is good collaboration between universities involved in this work. If funding allows, it would be helpful to have an industrial partner as a collaborator to evaluate catalysts' performance in fuel cells.
- The list of collaborators is very strong and has promise, but the majority of the results reported come from the lead institution, with very little information available from all the subcontractors. It is not clear what company/institution out of the team is leading a scale-up effort, if any.
- There is mostly strong collaboration with academia, but not with an automotive original equipment manufacturer (OEM) (Hyundai Motors). OEM testing of these catalysts is recommended.
- University collaboration is strong. The program needs collaboration with catalyst makers and stack integrators.
- The project has a reasonable set of collaborators. However, it is unclear what the key contribution is for each collaborator. Dr. Popov's group seems to be doing (as shown in the presentation) the majority of the work, while other collaborators are providing some support in analysis. The synergistic interaction of these various collaborators so this project can achieve the targets is unclear.
- The collaboration and coordination appeared to be acceptable. A slide with collaborations was shown. Whether these are partners or not was unclear. The technical aspects they contributed to were indicated, but not during the presentation. It seemed more like single investigator work, which perhaps it predominantly is.
- The level and type of collaborators are reasonable. It would be useful to have some collaboration with an established MEA integrator (industry or national laboratory) to validate performance at a second site.
- The collaborations throughout this project are little noted in the slides. It appears that the project had characterization done by three parties outside the University of South Carolina (USC): University of Illinois, Chicago; University of Illinois, Urbana-Champaign; and Clemson. These collaborators did ICP, TEM, HRTEM, and XEDS. Some other organizations designed test cells to USC specifications. Essentially all "collaboration" was realized through tasks appointed by USC. No automotive OEM or stack OEM collaboration existed. Other catalyst projects have included a partner for scaling up manufacturing quantities (e.g., Johnson-Matthey Fuel Cells Inc. [JMFC] in the General Motors catalyst project). That does not exist in this project.
- It is unclear if any of the presented work was performed outside of South Carolina or what the listed subcontractors specifically contributed to the project. The project would benefit from the inclusion of a catalyst company and an OEM, and it might benefit from other groups with enhanced characterization or MEA fabrication/testing capabilities.
- Most of the work appears to be centered at USC, with no national laboratory or industrial partners. What other work is done is restricted to materials characterization.
- The collaboration in this project is weak. The PI's collaborators are limited to microscopic imaging or analysis. The vendors should not be listed as collaborators. The PI should include other institutions, particularly industrial OEMs, in catalyst and MEA evaluations.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The project is relevant to DOE goals on improving catalyst activity, durability, and stability.
- Durable and high-performance catalyst/support systems are needed with low-PGM loading.

- This project is well aligned with DOE targets on cost, activity, and durability.
- The project is well aligned with the DOE Fuel Cell Technologies Office's goal.
- The project, with the data presented, shows promise in improving the stability of the Pt catalyst. The key is understanding how to improve the high air performance and document improvement against the state-of-the-art Pt/C catalysts.
- The project is focused on low-loaded Pt catalysts with required durability. This is perhaps the top need of the community at this time. The project lacks a defining advantage over other approaches being pursued.
- The approach and progress aside, this project is working on the most relevant topic to DOE's fuel cell effort, which is the development of a durable, high-activity oxygen reduction catalyst. If such a thing exists, the entire stack could be decreased in size, reducing cost for Pt, membranes, gas diffusion media, and plates. The cost of the air handling system may also be decreased if catalyst activity could be leveraged for lower pressure. The project has combined concepts for non-PGM materials that are active for oxygen reduction, and Pt modified with base metals for higher activity. Both streams have been identified as relevant in other projects. The only difference in this project is that the investigators have combined the two concepts.
- If successful, this project would provide an alternative approach to generating Pt-alloy catalysts dispersed on graphitized carbon-type supports with some potential additional support benefits. However, it would still have the disadvantages of carbon corrosion at the high potentials expected in start-stopping; the costs associated with the high-temperature processing steps of dispersed catalysts; and loadings not much different from other methods being pursued that do not have these limitations, such as the 3M electrode approach. The long time (12 hours) of leaching and annealing for the best catalyst preparation will significantly add to the manufacturing costs of this catalyst on any reasonable scale. One question is whether a faster process can be found with the same effectiveness.
- The project is certainly progressing in the direction of DOE and DOE Hydrogen and Fuel Cells Program goals, but it is not going to contribute strongly.
- The project is relevant to DOE's objectives in creating a durable, active, low-Pt-content catalyst. As a result of very insignificant progress, no significant impact is expected, considering this is the last year of funding.

Question 5: Proposed future work

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

- The future work is logical and rational and has quantitative performance and durability goals. Durability with updated protocols should be checked.
- Overall, the future work advances each of the tasks. However, a question arises from the timeline slide: it is unclear whether there is any mechanism or time allotted for "circling back" from any newly understood result to insert a new approach or modify the plan of any given task. The tasks seem to be laid out very linearly. The target catalyst performance characteristic (ii) listed for Task 4 (slide 22), according to the revised SOPO dated 1/23/2014, says "initial high current density performance of at least 1.5 A/cm^2 at 0.6 V iR-free." This should be a goal stated in terms of as-measured cell voltage, not iR-free, because the electrode impedance and how it changes with current density and operating conditions need to be understood. This is a critical property of any new catalyst. The durability testing should also include voltage cycling to 1.5 V, not just 1 V, because this will evaluate the most likely serious flaw of this approach—carbon corrosion at very high potentials generated by stop/start effects. This should be done sooner rather than later, because it will likely be the most serious problem to be addressed.
- There is little time remaining in the project, but for the most part, the tasks identified are ones that are needed. The doped catalyst may only be a few optimization steps from meeting both end-of-test mass activity and 0.8 A/cm^2 performance targets. The one thing that needs to change involves "optimization of high volume production procedures." The partners, as listed, do not have the experience to complete this task, because nobody listed is a high-volume catalyst producer. It would be good if someone at DOE could manage to help the investigators work with a partner like JMFC/TKK/Umicore/BASF, or some party with that expertise. A cost analysis of the final process would also be helpful.
- The proposed future work sounds good. It would be good if the project can clearly document whether there is any change in specific activity (microA/cm^2 Pt) and any loss of Co from the catalyst during the durability cycling.

- The proposed work is adequate, considering the duration of the project. Because no obvious progress has been made with the current best material, efforts should be put into durability and scalability studies.
- The future work appears appropriate, based on the stage of the project.
- The future work is focused on overcoming barriers identified during the previous year. Identifying the reasons for enhanced mass activity of the Co-leached Pt catalyst should be a focus of the future work.
- It will be beneficial to have the industry-led evaluation of the technology in addition to the DOE deliverables.
- Future tasks such as the scale up of synthesis and MEA fabrication and testing to meet the 0.3 A/mg Pt target are not in line with the DOE out-year targets that speculative materials should be targeting. It is unclear how to best refocus effort in this project, but the key issue should be to focus on higher catalyst performance and improved activity before worrying about either scale-up or fuel cell testing.
- There was no time remaining for discussion of future work and rationale. The speaker went way beyond the time limits.
- The proposed future work is weak. At this stage of the project development, the PI should be able to elaborate on one or two topics to focus the effort based on the lessons learned. Describing the future work by reiterating the initial proposed tasks without including new scientific insight renders it generic.

Project strengths:

- The project features a good concept in combining two different oxygen reduction activity ideas into one: non-PGM activated carbon composite and base-metal-doped Pt. The project team understands well how to manipulate on a small scale the process parameters needed to increase the mass activity of the materials, both for beginning-of-test and end-of-test. There has been year-after-year improvement. The project ably used surface modification of C to improve dispersion of Pt, which is not an easy thing to do.
- If experimentally demonstrable, the synergy of the ORR active support combined with the Pt core-shell catalysts deposited on it is a real strength.
- The project has a strong team, as well as strong synthetic and characterization capabilities.
- This project proposes a new, very interesting approach for doping Pt with a transition metal.
- This project closely meets the DOE targets on activity and support durability.
- The project features a good approach and a strong team.
- Strengths of this project include the reported performance and the strong relevance to DOE's goal.
- The project focuses on the key barriers of performance, durability, and cost.

Project weaknesses:

- It is unclear whether the approach offers anything above or beyond state-of-the-art PtCo/C catalysts. A major concern for the project is the PI's lack of concern for guidelines and his inability to present his work in an adequately reviewable format. The presentation packet has 59 slides, while the guidelines stated a maximum of 28. The inclusion of such significant but unorganized data in the supplemental slides constitutes either an inability to determine the relative importance of the work presented or a disregard for reviewers' time and effort. The PI was unable to present the majority of the most important slides regarding results during the allotted presentation time and spent undue time either preaching or going through background information of low value. The slides themselves left much to be interpreted, and despite spending more than twice as much time on this review than on any other, this reviewer still has twice as many unanswered questions. The PI's behavior with the session moderator also was inappropriate.
- Weaknesses include the lack of automotive OEM collaboration and the lack of catalyst supplier collaboration for scale-up. All collaboration was directed by the USC. Numerous processes appear to be involved in making the catalyst, including pyrolysis for the A-CCC, chemical leaching to remove metals from the A-CCC, and the doping of Pt, presumably followed by some form of leaching to remove excess metals. No economic analysis has been presented to show whether all these processes add significant cost that would offset the lowering of precious metal loading.
- The presenter and the presentation material are the biggest weakness for the project. It was hard to follow the speaker. To further complicate the matter, the catalyst types and activities were not clearly mentioned. A simple table of all catalysts prepared and tested with electrochemical surface area, mass activity, specific

activity particle size, and other key parameters would help. It is almost impossible to get these data without digging through various parts and pieces of the slides.
- The project pursues very difficult and convoluted synthetic routes with an unclear purpose. No comparison is presented with the state-of-the-art Pt-alloy material. The Pt-C interaction and the stability of carbon support have not been carefully studied. Very incremental progress has been made in the last two years.
- Cobalt leaching, both from the support and the core of the core-shell nanoparticles, may compromise fuel cell durability. It is another approach to modify the catalytic activity of Pt by introducing a transition metal.
- There is weak collaboration with OEMs and catalyst suppliers.
- The targets are not rigorous enough to reveal the real potential issues with this catalyst/support approach. They should involve more severe testing to determine sooner the extent of the materials issues.
- The project has much more data than its interpretation. The project is also lacking the industry evaluation/verification.
- Weaknesses of the project include the scientific rationale and limited collaboration.

Recommendations for additions/deletions to project scope:

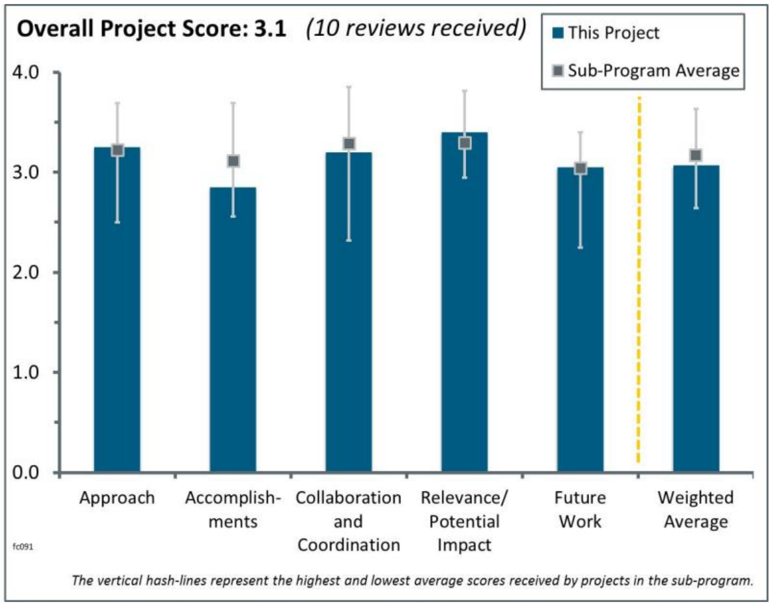
- The researchers should add voltage cycling durability tests to 1.5 V for the durability as soon as possible. They should also add tasks to evaluate the MEA performance as a function of PGM loading, covering the range of 0.125 (total from anode and cathode) to 0.35, to demonstrate the impact of electrode thickness effects, water management, and impedances on high current density. The researchers should be sure to show the as-measured polarization curves, not just the iR-free voltages.
- Mass-activity evaluations need to be complemented by specific activities and Pt surface-area calculations. Otherwise, it is unclear why Co leaching from the Co-modified catalyst leads to improvement in mass activity. It is unclear if it is just surface area effect. Durability tests need to be complemented by testing the fuel cell in “start-stop” conditions (i.e., between 1 and 1.5 V).
- The project could benefit from an assessment of actual current status and a management review of the proposed plans to achieve targets. This should be a go/no-go meeting, and some technical catalyst experts (especially with regard to commercialization) should make the assessment.
- The project is always reporting hydrogen-air performance as iR-free voltage; it is unclear why the researchers cannot report the as-measured cell voltage. The reviewer wants to know if they can work with the newer material set for membranes and gas diffusion layers to improve performance rather than trying to correct for it.
- One important addition needs to be made: an industrial catalyst partner needs to be added to scale up the catalyst manufacturing and to provide perspective on whether the processes being prescribed are suitable for high-volume manufacturing. Stack or automotive OEM collaboration would be helpful and should be added in the last stage of this project.
- The project team should focus on one or two promising systems and demonstrate stability at more demanding conditions.
- The team has to focus on the durability and scalability of the best material to-date.
- The team should broaden collaborations.
- This project should be discontinued. Outside of this, any further effort should be focused just on catalyst development and characterization without scale-up or large-scale fuel cell testing. In the future, the material presented has to be presented in a cleaner, more cohesive manner.

Project # FC-091: Advanced Materials and Concepts for Portable Power Fuel Cells

Piotr Zelenay; Los Alamos National Laboratory

Brief Summary of Project:

The objective of this project is to develop advanced materials (catalysts, membranes, electrode structures, membrane electrode assemblies [MEAs]) and fuel cell operating concepts capable of fulfilling cost, performance, and durability requirements established by the U.S. Department of Energy (DOE) for portable fuel cell systems. The project will ensure a path to large-scale fabrication of successful materials.



Question 1: Approach to performing the work

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

- This project involves a variety of activities related to fuel cells using alcohols or other C-containing molecules as fuels for portable power systems. Specific focus areas include improved PtRu anode catalysts and hydrocarbon membranes for direct methanol fuel cells (DMFCs) as well as improved catalysts for direct ethanol fuel cells (DEFCs) and dimethyl ether fuel cells (DMEFCs). The project also includes materials characterization and MEA performance testing. The project is near the end of its term. The project is well designed, focusing on technical milestones relating to barriers from the Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The specific work tasks are a bit diffuse; for example, alternative fuel research is to a large degree separate from DMFC research, but all of the work helps support the DOE goal of improved portable power generation.
- The combination of catalyst, membrane, and electrode development and the investigation of three fuels toward the targets is strong.
- The project was well designed. DOE made good choice. The barriers are clear.
- The overall approach is sound. All critical aspects of catalysis, membrane, and MEA for alternative fuel fuel cells are included. One thing that could be improved is to clarify the purpose and interactions of the multiple directions/institutions on catalyst development. For example, in methanol oxidation catalysts, Johnson-Matthey Fuel Cells Inc. (JMFC), Los Alamos National Laboratory (LANL), Brookhaven National Laboratory (BNL), and the University of Delaware (UDel) all have seemingly isolated projects. The interconnection of each direction is not clear. BNL and UDel catalysts have remained on the model catalyst scale and have never been benchmarked against the newly developed JMFC or LANL catalysts, so it is very difficult to judge the merits of the UDel and BNL catalysts.
- The project has a wide scope, including DMFC anode research, electrode structure development, hydrocarbon membrane development, and alternative fuels for portable fuel cells. This wide scope may prevent the project from making significant progress. Reduction in catalyst loading and cost would be an important area of focus for the portable power field. Expensive catalysts and high loading are still used for DMFCs and DME fuel cells. PtRu nanotubes and PtRu nanowires are good pathways to reduce the catalyst loading, but improvement in durability is not known. Last year, the project showed high DMFC performance with ultra-thrifted anode PtRuSn/C with only 0.3 $mgPt/cm^2$ loading using 2 M MeOH. It is unclear why a different anode catalyst (advanced anode catalyst [AAC] 1:4 PtRu/C) was the focus of this year's DMFC work and used for stack testing by SFC instead. It seems that the lower-loading PtRuSn/C would have been a better choice. LANL should strive for a low-loading catalyst that is durable and performs well at higher methanol concentration. These criteria are important to aid in the

commercialization of fuel cells. The advantage of fuel cells is the longer operating time; therefore, the DMFC system must be developed for operations at higher MeOH concentration to increase the energy density of DMFCs. An impressive set of characterization tools is available and has been implemented in the project.

- The approach to adapt improvements in catalysts for hydrogen fuel cells using nanostructured catalysts for DMFCs and DEFCs focuses on a key barrier to commercialization: cost. The bifunctional catalyst approach is logical; the use of oxides such as CeO_2 to enhance oxidation is a good approach. Block copolymer work is a good approach to try to reduce methanol crossover and improve performance. The work on alternative fuels (i.e., ethanol and DME) is appropriate. The researchers need to look at fuel utilization, anode recycle schemes, and exhaust/emissions for DMEFCs.
- The multidirectional approaches taken by the team for the completion of all tasks are adequate. All the technical barriers have been addressed appropriately. The responsibilities for anode, membrane, alternative fuel development, and performance/durability tests were given to the research teams with significant experience and strength in the respective areas of research.
- LANL is targeting the main challenges of DMFCs: reducing the high amounts of expensive catalysts currently needed, reducing methanol permeation, increasing overall efficiency, and increasing durability. These aims are achieved mainly by developing new anode catalysts and new membranes. For direct DME fuel cells and direct ethanol fuel cells, improved catalysts are developed to improve performance.
- There appears to be a lot of work being reproduced—Pt:Ru ratio optimization has been carried out numerous times. One of the most interesting things with EtOH fuel cells should be the extent of conversion to CO_2, but not much emphasis or work has been done here. It was good to see some long-term testing, but of most interest would be a post-mortem analysis of these materials (—determining where the decrease in performance came from, how much catalyst was lost, or how much the mass transport resistance increased). Justification for the different membrane chemistries is not given; it is unclear how block chain length affects the conductivity/permeability ratio. LANL was criticized last year for a lack of long-term testing; the rationalization that it will tie up precious resources is unsatisfactory for a $10 million four-year project Choosing only one system to test provides little insight to the community. The durability of these systems is one of the biggest open questions, and it is given minimal importance.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- LANL is making consistent progress toward DOE goals. The DMFC anode work with JMFC is providing optimized formulations with high activity at low precious metal loadings. The performance target is very nearly met. The nanotube/nanowire catalyst microscopy was very well done, and the catalysts appear to have high activity. All the approaches discussed for improving catalysts appeared to provide incremental improvements. The hydrocarbon membrane work also provided incremental but significant improvements, with favorable combinations of methanol blocking and ion transport demonstrated. The ethanol and DME oxidation work was interesting, but it needs a tighter focus on mass balance to account for all the various pathways that may be followed for fuel oxidation. If ethanol oxidation occurs with only 40% efficiency, it will be important to consider why ethanol might be desirable as a replacement for methanol.
- The team has achieved the stack milestone using the AAC. It will be good to see the use of a low-cost tetramethyl bisphenol A (TM) membrane in stack testing. With N115 in the stack, it is hard to understand whether the low-cost membrane will be able to function in the stack. The team should also determine the stability of these new anode catalysts under anode starvation, which is important to understand.
- Good progress has been made on the JMFC DMFC catalysts, LANL DME catalysts, and Virginia Tech hydrocarbon membrane thrusts. The BNL and UDel catalyst projects each tried some seemingly new combinations, but it is difficult to judge their impacts on the overall project based on the limited data presented. The potential poison effect by Cu in the UDel catalyst project was raised last year, but this issue was ignored by the team.
- The new PtRu nanowire catalysts look promising, but scale-up to 9 mg per batch is not a demonstration that multigram quantities could be produced if the technology were to be commercialized. A nice suite of new multiblock copolymers was prepared, some of which showed all three desirable properties of water uptake,

ionic conductivity, and lower crossover. Because it is known that breaking the C-C bond in ethanol is extremely challenging at low temperatures, the electrochemical data must be combined with product analysis for meaningful evaluations of these new catalysts. The progress with the DDMEFCs was outstanding.

- The researchers have achieved good advancement in DME activity over the past year and over the life of the project. Good improvement in DMFC performance is indicated for the TM membrane (slide 12). However, this membrane has high resistance, which raises questions because others have found membrane resistance is much more important than methanol crossover for determining performance. It is not clear whether cell voltage is iR-corrected in this figure. (If it is iR-corrected, then cell performance for the TM membrane would not be improved, due to the resistive losses from membrane). If it is not iR-corrected, the improvement from decreased crossover is larger than others have observed. It is not clear what has been done in the past year, but overall progress has been good. Improvement in DMFC performance does not appear to have been made over the last year; it is still 40 mV from the target. It is not clear what new work has been done with membranes since last year.
- Significant progress has been made in all fields of work. In particular, the high DMFC performance with low anode catalyst loading is very impressive, even if the milestone has not quite been reached yet. It would be desirable to have methanol permeation data for these measurements. A very high methanol utilization of 0.96 was assumed without any proof. The newly developed membranes look very promising, but measurements comparable to the measurements with the new anode catalysts and methanol permeation should be performed to show the effect of the membrane under fuel cell operation conditions. Progress in DEFC catalyst research is being made, but this is shown on a model system only. Fuel cell measurements should be done here as soon as possible. Progress on DDMEFC catalysts has been made and performance similar to a DMFC has been achieved. Measurements with DME ware made under significant pressure (anode 26 psig, cathode 20 psig); the effect of this on balance of plant (BOP) efficiency should be evaluated to assess if the BOP efficiency of 0.9 stated as an aim at the beginning can still be reached.
- Overall, the project team has made good progress in all areas, but not necessarily toward DOE goals. Durability testing in a stack by a DMFC developer is good. However, a 1 mg/cm^2 AAC (PtRu/C) was used for the testing instead of ultra-thrifted PtRuSn/C (0.3 mg/cm^2). It is not clear what the wt.% of metal was in AAC and what type of carbon was used in the support. It was also not clear how it was determined that the optimum Pt:Ru ratio was 1:4 for fast dehydrogenation and efficient CO removal. It has been reported in the literature that 1:1 PtRu was the optimum ratio. Also, a relatively low methanol concentration was used for testing; a higher methanol concentration needs to be used to increase the energy density of DMFCs and make them competitive to batteries. The work on PtRu nanotubes and PtRuCu nanowires is very interesting. It was unclear whether the synthesis method was galvanic displacement, how the ECSA for these catalysts was determined, whether there was any evidence of Cu leaching from the catalyst, and how this affects methanol oxidation over time. The scale-up of PtRuCu nanowires was notable. Catalyst development for ethanol oxidation is interesting, but the mechanism of ethanol oxidation on these oxide catalysts is unclear. Also unclear is the role of CeO_2 and why it enhances the activity of PtML/Pd/C. The hydrocarbon membrane appears to have good properties, but it does not seem that the membrane has much better methanol crossover than the industry standard. It would be nice to see data for higher methanol crossover and electro-osmotic drag. Furthermore, the fact that HFR increased with time during life testing does not indicate a stable, durable membrane. It is unclear whether this was a membrane issue or an interfacial resistance issue, and whether the ionomer was used in the durability testing of the membrane.
- Improvements in DMFC performance are incremental. Project goals are not very ambitious. LANL has done a nice job of showing the feasibility of DMEFCs, but there is little innovation here; the MEA that worked the best was essentially the same as the DMFC MEA. How to use DME with a high utilization seems like the most important question now, and investigators have given it little thought. A stoich of even 1.3 would be disastrous; it is unclear whether one would just vent this to the atmosphere or oxidize it in a reactor—that negates system simplifications. The essential membrane performance metric is the ratio of conductivity/permeability; then original equipment manufacturers (OEMs) can choose the thickness that optimizes for their application. This ratio is never given and the two parameters are always given separately, making it hard to evaluate their progress.
- It is unclear whether the 0.6 V at 150 mA/cm^2 MeOH-oxidation is reachable before the project ends in September. Temperature or loading does not seem to provide the necessary boost, and the increase in temperature would increase the performance boost at the cost of durability. There does not appear to be a temperature or pressure set point in this performance target—there should be. The mass activity target has

been achieved according to the principal investigator (PI), but it is not clear whether the mass activity is based only on the anode platinum group metal (PGM). The cathode loading was very high. LANL showed good stack and membrane results. The nanowire looks promising, but at 19 mg/batch, scale-up seems far away (despite claims of demonstrated scale-up from 5 to 19; this is not nearly manufacturing scale). The quantities necessary for commercialization are not clear.

- The assumed fuel efficiency of 0.96 is way too high—the project team should explain the basis for this assumption. The choice of ethanol oxidation catalyst does not make sense. The product of the ethanol oxidation is not clear. There is no way to break the C-C bond at low temperature. The decay rate of 19 µV/h is too high. For the fuel cell decay rate, 3 µV/h is normally acceptable. DOE has high expectations of talented national laboratory researchers; however, the progress of the project was not too positive.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The team consists of a good mix of university, national laboratory, and industrial partners. Collaboration with SFC and Oorja Protonics is very advantageous to the team because SFC has expertise in portable DMFC systems and Oorja has expertise in high-power DMFC systems. The team can obtain valuable information on the material/performance requirements for portable power and high-power DMFC systems.
- There is good collaboration with university, national laboratory, and industry partners. It is nice to see a DMFC developer as part of the team to integrate the MEA into its DMFC system and independently test it and validate the performance.
- There are very good collaboration with industry, national laboratories, and universities as well as with international collaborators; it would be nice to see some more industrial collaborators, now that good power densities are being achieved.
- This project features good collaboration with partners. The involvement of SFC and JMFC is beneficial and ensures that any advancements make the transition to industry.
- The project includes many partners who contribute a wide range of expertise. At times it appeared that the project might be operating as several autonomous units; in future iterations, it may be helpful to include a tighter focus so all partners stay fully engaged.
- The lead PI assembled a great research team. The collaboration with industrial and systems partners such as SFC and Oorja Protonics is an excellent addition. However, the interaction between anode catalysts by each research group needs to be improved because they seem isolated.
- Presently, all institutions are working in their own field of expertise, which leads to good progress in the individual fields. An integration of the individual results, however, is missing. It would be interesting to see a combination of the new membranes with low methanol permeation and the high-activity anode catalysts. Also, the work on DDMEFCs and DEFCs is not connected to the work on DMFCs. For example, the results on DDMEFCs are compared to the results on DMFCs with commercial catalysts only.
- A clear responsibility for each party was not seen.
- There are lots of partners, each focused on its own little box. It is unclear why one of the developed membranes was not used in the performance test. The HFR appears to be too high compared to Nafion® 212. It is unclear why the results were plotted in the bar graphs versus Nafion® 115. A lot of work was done, but the partner interactions were unclear.
- The OEM appears just to have tested one MEA; it is not clear how feedback is working with regard to the importance in performance. It is not clear what is most important, i.e., current efficiency, durability, or peak performance? How were MEA components selected accordingly?

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The project is relevant to the objectives of FCTO's MYRDDP. All activities are aligned to DOE's goal. The focus of the project is on the development of advanced materials, such as catalysts, membranes, electrodes, and MEAs for DMFC application, and it is expected to fulfill cost, performance, and durability requirements established by DOE. Meeting these requirements is very important for the commercialization of DMFC technology.

- The project is very well matched to DOE and FCTO goals for portable power. It addresses the critical limitations that are holding back widespread adoption of fuel cells for portable power. The progress from the project has been mostly incremental, but it is real and important.
- The project objectives are in line with the DOE Hydrogen and Fuel Cells Program (the Program); more specifically, the project addressed key issues in direct methanol and other liquid-fuel-based fuel cells for portable applications.
- Fuel cells with liquid fuel are ideal for portable applications. The research toward this application is a must.
- This project covers all aspects of DOE's goals for portable fuel cells.
- The work on DMFC and DDMEFC, focusing on highly active catalysts and membranes with low crossover, is highly relevant to achieving DOE goals. The relevance of the DEFC work will depend on achieving a stable catalyst with a high selectivity for the 12-electron process as proposed in the future work.
- It is not clear how the project is going to achieve the system cost target of $3/W. The catalyst loading is still relatively high. The performance target is a system target—it is unclear whether this is taken into consideration when determining the project's progress toward this target. It is unclear whether certain assumptions about the system (e.g., BOP, power controls, and fueling container) are made to calculate the performance target for the project, and, if so, what the assumptions are. The project is more research and development focused, with an emphasis on advanced materials development; therefore, system considerations are likely not taken into consideration. The durability of the advanced materials in a system is relevant and important. The path to large-scale fabrication of successful materials is a good objective.
- DMFC catalyst loadings are very high compared to hydrogen fuel cells, and power densities of the fuel cells are much lower. It is not clear how DMFC, DEFC, or DMEFCs help DOE reach its overall goals of reducing petroleum imports and CO_2 emissions. The costs are high due to the high PGM loadings needed and low power densities, and these fuel cell systems are unlikely to move into anything other than niche applications where impacts on petroleum usage and emissions will be negligible. In addition, the technology is sufficiently different that their adoption will not significantly impact production volumes for hydrogen fuel cell materials or impact costs. The potential benefit is getting users familiar with fuel cells, but this is a limited benefit.
- DOE made great efforts to promote fuel cells and hydrogen; however, every year, good stories always come from national laboratories without any promising applications.
- There appears to be poor correlation between DOE goals for portable power and project goals. LANL was criticized last year for not considering DME systems, and that pattern continues this year. Just optimizing the catalyst for these systems does not answer system questions—the OEM partner should be able to help here.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The future work described is aligned with the proposed work of the project.
- The proposed work plan is a reasonable extension of the existing work. Because this project is near the end, some practical metrics need to be addressed: the scalability of the new catalysts and membranes, cost/performance analysis, etc. The viability of the BNL catalysts needs to be demonstrated on at least the MEA level in a real fuel cell.
- In general, the proposed future work is good. It is recommended that the PI examine a higher concentration of fuel and/or develop a membrane that lowers methanol/fuel crossover. The PI should focus on testing lower catalyst loading and on developing a new catalyst that is non-PGM, if possible, to lower costs.
- Future DMFC work is focused on further increasing the cell voltage, on catalyst stability, and on demonstrating a 500 W DMFC system with reduced catalyst loading. This will make the DMFC results suitable for industrial application. Further work on catalysts and implementation of multiblock copolymer membranes will lift the DDMEFC to a level the DMFC has already reached. DME crossover studies are

suggested and should be compared to similar studies on DMFCs. For DEMCs, the proposed work on catalyst stability and 12-electron selectivity is crucial for the usability in a real-world cell.
- The PI has identified critical issues and a realistic path to success in methanol, DME, and ethanol work.
- The project has almost ended so there is relatively little proposed future work. That which is proposed is sensible and appropriate.
- LANL is approaching four years on the project and is still looking at new materials; this part of the project should certainly be demonstrating long-term durability, relevance to system targets, and cost analysis.
- Ethanol oxidation may be premature. It is suggested that the PI first get the methanol and DME to work properly. Integration of the developed components into a functioning MEA (the nanowires/AAC/membrane) also shows better partner collaboration.
- This reviewer does not believe this project will have value for industrial applications.
- The project is scheduled to end this fiscal year.

Project strengths:

- The team is well organized and capable of developing DMFC membranes and MEAs. The team is composed of respectable research organizations with adequate expertise. Overall, the team is equipped with the knowledge base, resources, and industry/academia/national laboratory mix that is required for the success of this project.
- The team is strong and highly capable. Good progress has been made on JMFC DMFC catalysts, LANL DME catalysts, and Virginia Tech hydrocarbon membrane thrusts. The collaboration with industrial and systems partners such as SFC and Oorja Protonics is an excellent addition.
- A strength of this project is the integration of research into catalysis, electrode design, and membrane synthesis.
- Incremental but significant progress has been made on many fronts related to use of liquid alcohol and ether fuels in portable fuel cells.
- The catalyst work is the major strength of this project. The inclusion of industrial partners such as JMFC and SFC is also a strength.
- This project features excellent research by individual partners in their respective fields of expertise.
- This project features strong research capabilities, especially in generating new materials.
- This project features good individual efforts from each partner in its own portion of the project.
- The project's strength is the team and its expertise.

Project weaknesses:

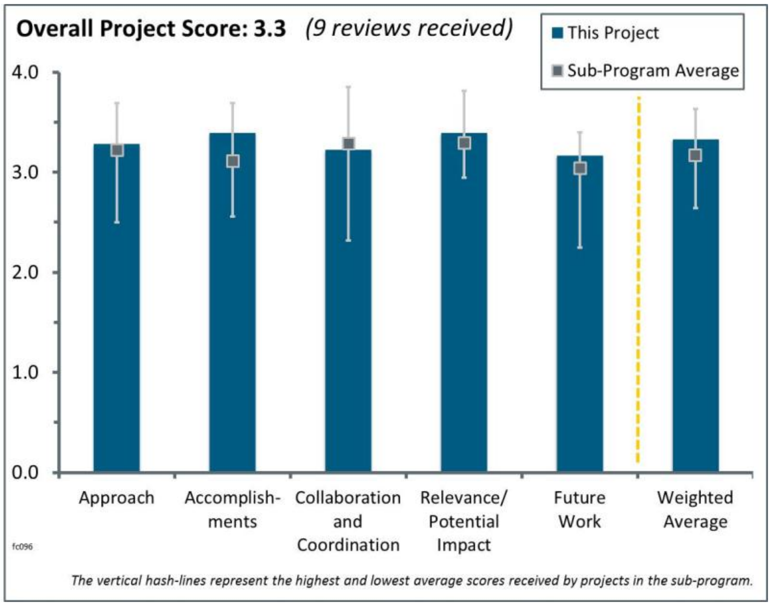
- Project direction is the biggest weakness; there appears to be a lot of work over different areas without much coordination or steady progression toward Program goals. The strength of the team is also its weakness—the team members are very good at generating new materials and have spent most of the project resources on this task. As the project progressed, this task should have been narrowed.
- The coordination and interaction between research groups need to be improved because they seem isolated. The catalyst development projects at UDel and BNL need to be gauged against catalysts developed by JMFC and LANL, preferably in actual fuel cells, to help judge their efficacy.
- The project needs more scientific input. Much of this project seems to be empirical. There are no real theories about where to go in terms of catalyst formulation and no ability to screen catalysts for product specificity.
- Project weaknesses include the lack of appearance of partner cooperation, insistence on very high Pt loadings, and possible problems in scale-up of the nanowires to commercial levels.
- A weakness of this project is the combination of a large number of research organizations, which may be a management challenge for the prime organization.
- The project is a bit diffuse, with many parts operating, but not always with great synergy.
- There is limited cooperation between partners.

Recommendations for additions/deletions to project scope:

- It is unclear why so many fuels are included in this project. Improvements in DMFC performance, durability, and reduction in cost are what is needed to bring portable fuel cells to commercialization. Methanol cartridges are already approved for onboard passenger airplanes. Therefore, it is recommended that an ethanol oxidation catalyst not be added to the project because it will be not provide as much power as methanol. Toxicity is the only reason for choosing an alternative fuel to methanol, but methanol toxicity is not an issue for commercialization.
- Because this project is near the end, some practical metrics need to be addressed: the scalability of the new catalysts and membranes, cost/performance analysis, etc. The viability of BNL catalysts needs to be demonstrated on at least the MEA level in a real fuel cell.
- Because the project is finishing up, all efforts on new materials should end and the focus should be on performance, durability, and cost.
- LANL should remove ethanol research from the scope; this is premature to be funded by DOE. The team should also add system integration of all the individually developed components.
- LANL should screen ethanol catalysts for product specificity.

Project # FC-096: Power Generation from an Integrated Biomass Reformer and Solid Oxide Fuel Cell (SBIR Phase III Xlerator Program)

Patricia Irving; InnovaTek, Inc.



Brief Summary of Project:

The 2013/2014 objective of this project is to demonstrate the technical and commercial potential of power generation from an integrated biomass reformer and solid oxide fuel cell (SOFC) for energy production, emissions reduction, and process economics. InnovaTek's steam reforming fuel processing technology has multifuel capability, using natural gas as the bridge to renewable nonfood biomass. Effective thermal integration (with combined heat and power [CHP]) and off-gas recycling enable high system efficiency. Additive manufacturing (three-dimensional [3-D] printing) is used to reduce the fuel processor equipment cost.

Question 1: Approach to performing the work

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

- The project clearly defines the barriers and how it plans to overcome those barriers. There is excellent integration with the Mid-Columbia Energy Initiative. The project has a logical flow that clearly documents the successes throughout.
- The project seems to be going well. The researchers have achieved an impressive reduction in size and part count for the fuel processor.
- The approach is very good—adding multifuel capability makes a lot of sense.
- This is a great project. It is well planned and executed with a laser-like focus.
- The project was well designed, feasible, and well integrated with other efforts.
- It addresses cost, performance, and durability for small distributed generation systems. The approach is reasonable and direct: develop and demonstrate a fuel cell distributed energy system that operates with second-generation biofuel. Several important aspects of the approach are as follows:
    - Use of a system based on InnovaTek's steam reforming process and SOFC (major emphasis).
    - Nonfood biofuels that include pyrolysis oil and bio-kerosene processed locally.
    - System to be demonstrated in Richland's renewable energy park and tied to grid.
- The project uses biofuel for SOFCs. It is a good concept. The cost of the biofuel is unclear. If the biofuel costs a lot, the whole system can run on natural gas and other fuels.
- The project addresses the burner system but no references were listed for S impurities. This could be critical. The sulfur scrubber was shown in the process flow diagram, but not much information was presented on it.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The incremental improvements of the system design and overall simplification of the system are excellent. The use of additive manufacturing for the innovative components pushes the technology further than expected. Performance gains of the system are very close to DOE targets and above many other systems targeting similar applications.

- The project seems to be on track to achieving all or most of its goals. Impressive results have been achieved thus far, although it is early in the long-term testing. It will be good to see the results of the long-term test. The project is slightly behind on schedule but within reason for a project of this complexity.
- The project achieved its goal of demonstrating the use of reformed biofuels in a fuel cell system.
- The technical results and economic analysis are very sound.
- The cost numbers quoted for the SOFC are outstanding. This unit should be cost comparable if not better than other fuel cell technologies. Even at 1,000 units per year, these numbers are very good; they should be crosschecked with the 5 kW numbers from Battelle.
- The project's technology has been improved from last year due to the following:
    - Addition of ejectors and nozzles for the reformer.
    - Reduced part count and complexity.
    - Improved stack efficiency.
    - Addition of a heat exchanger for CHP.
    - Production of two gen-3 prototypes.
    - Came close to 2015 goals.
- The project could check the potential lifetime of the system first. Any good system or catalyst without lifetime is useless. DOE should think about lifetime and performance together.
- Laboratory demonstration of the performance and durability is important and not yet clearly presented. Some of the progress from year to year appears to be better than expected and the reviewer is concerned about the reality of this progress.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- This project features excellent collaboration with the following:
    - Subcontractors: Topsoe – SOFC; manufacturing partners – 3-D Printing (Direct Metal Laser Sintering)
    - Strategic partners: Impact Washington – commercialization strategy support; Pacific Northwest National Laboratory – provided upgraded bio-oil made from non-food biomass (within the DOE Hydrogen and Fuel Cells Program); Honeywell UOP – provides bio-kerosene; Systems Integrators – working with several commercialization partners for identified markets; and Mid-Columbia Energy Initiative – collaboration for demonstration.
    - Education: Supported one graduate student from Washington State University and one Delta High School intern in mechanical engineering and chemistry.
- The project seems to be collaborating with partners very well. InnovaTek is supporting students at high schools and universities and collaborating with manufacturers and a national laboratory. The addition of Topsoe fuel cells appears to be good.
- The support for the graduate student and the high school intern was good. The project enjoys excellent partners and collaborators spanning a cross-section of academia, private industry, and potential customers.
- The project has a good range of collaborators—all the way from a stack manufacturer to end users.
- Subcontractors may or may not be providing strategic input to help the project—this is not clearly defined. More involvement from the national laboratory level may be useful with respect to modeling.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- Operating fuel cell systems on biofuels and natural gas and utilizing them for CHP is an excellent nonautomotive application that needs more attention. This project is nearly meeting DOE goals for hydrogen production and CHP.

- The work is very relevant to the Program's research and development goals. Biofuel is a renewable energy with a CO_2 reduction effect, and the work with natural gas is in line with commercializing CHP systems. Completing this project is a way to advance technologies and generate jobs.
- The project aligns well with the Program's and DOE's research, development, and demonstration (RD&D) objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- The cost and performance values reported by InnovaTek should open the market for CHP and replace the market left by ClearEdge Power.
- The technology developed has great potential and broad and diverse market opportunities.
- Focusing on longer-term renewable fuel sources is important for the DOE mission.
- Biofuel for hydrogen is a good concept; however, the cost is too high.
- While the project was successful in demonstrating the technical feasibility of operating fuel cells on reformed biofuels, there is no business case for it. Thus, the project will have little impact in the real world.

Question 5: Proposed future work

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

- The future work that is right around the corner—the continuation of the long-term performance test—is of great interest. The remaining future work is consistent with, and a natural extension of, the progress to date.
- The integrated system appears to be a technology leader for natural gas. Cost numbers are very good. Performance numbers are excellent.
- The future work is important to providing evidence of performance, durability, and viability.
- The proposed future work is a reasonable and logical continuation; if high-temperature polymer electrolyte membrane (PEM) is a real additional option, it should be evaluated thoroughly. A small phosphoric acid fuel cell might be better.
- The future work, which involves the following, is well planned:
    - Continue long-term performance tests of the fuel processor with both biofuel and natural gas to obtain durability data.
    - Obtain performance data for multiple fuel processors.
    - Integrate balance of plant and the fuel cell.
    - Verify performance and durability with long-term and accelerated stress testing.
    - Further analyze process economics and market strategy.
    - Continue collaborations and establish additional relationships with fuel cell partners and systems integrators for the markets identified.
    - Conduct additional Phase III projects for auxiliary power unit applications with HT-PEM (beyond this project).
- It would be great to see a 1,000 hour test with an integrated system. The team should track CHP efficiency because components lose performance.
- The researchers need to think about the market.

Project strengths:

- First, this was a very well presented project. The slides were awesome. They effectively presented a good picture of the project and were very easy to follow. Well done! This project has been well planned and executed. It is meeting its technical goals. It involves an innovative use of new technologies such as 3-D printing of parts. It is meeting the DOE technical targets.
- Reductions in size and part count are a strength of this project. Additionally, the economic analysis showing estimated costs of $1,722/kW at 50,000 units per year is impressive, and the technology should penetrate the CHP market if long-term testing is successful and these costs are achieved.
- A strength of the project is the incremental improvements in the system design to reduce part count and simplify the system.
- All of the data are very positive. InnovaTek is reporting a very successful project, and performance is much better than polymer electrolyte membrane fuel cells. An electrical efficiency of 42% was demonstrated on reformed fuel.
- This project features solid work with a lot of in-house competence and valuable collaborators.

Project weaknesses:

- There are no weaknesses in this project.
- Challenges remain, but there are no glaring weaknesses in this project.
- There is concern that once this project ends, DOE will not continue working on biofuel fuel cells and reformed fuel systems.
- The long start-up time for SOFCs reduces the market. They are not competitive on a cost of energy basis with natural gas. Only 300 hours of durability performance was demonstrated. Even at 1,000 hours, durability remains an issue.
- More data on durability and start/stop performance would be helpful. The biofuel appears to be a dead end.

Recommendations for additions/deletions to project scope:

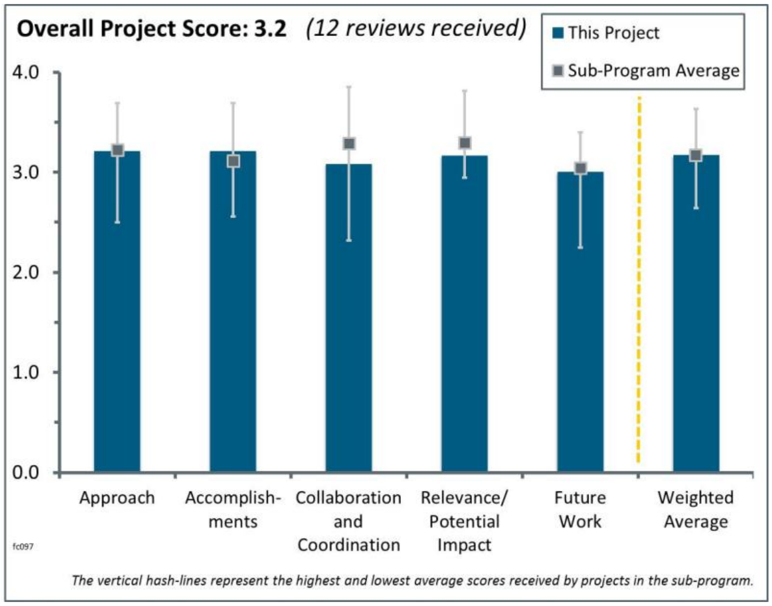
- The data reported by InnovaTek should be used to advance this power system for commercialization. This effort should be moved to the Technical Validation sub-program based on InnovaTek's reported data.

Project # FC-097: Stationary and Emerging Market Fuel Cell System Cost Analysis—Auxiliary Power Units

Vincent Contini; Battelle

Brief Summary of Project:

This five-year project develops independent models and cost estimates of fuel cell systems for stationary and emerging markets. Goals include: (1) identifying major contributors to fuel cell system component, materials, and manufacturing cost; (2) quantifying potential cost reduction based on technological improvements; (3) identifying areas for manufacturing research and development (R&D) to improve quality and/or throughput; (4) developing a basis for consideration of transition from other industries; and (5) developing accurate cost projections that can be used to evaluate total cost of ownership and facilitate early market adoption.



Question 1: Approach to performing the work

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

- This is a well-designed project. The cost estimates seem reasonable. It clearly shows that the balance of plant (BOP) is the cost driver in solid oxide fuel cell (SOFC) systems and why SOFC systems may be unsuited for transportation applications where the heat is wasted.
- The modeling is important to provide a breakdown for status and projections. It would be helpful to know how this project is similar to and different from the sister cost reporting and modeling projections, or perhaps the team could add a collaboration step for status checks of assumptions and results with these similar projects.
- The cost estimate methodologies are logical and appropriate. Key assumptions need to be clearly defined, and comparisons with previous cost estimates are suggested.
- Real-world estimates of cost for components, industry-based design information, and life cycle cost are all critical to a strong understanding of the relevant issues.
- Using the design for manufacture and assembly (DFMA) approach for analysis of a generic fuel cell system design is appropriate methodology.
- The approach is good and thorough.
- This project is broadly focused on cost analyses for both stationary and emerging-market fuel cell systems, with a specific focus on auxiliary power units (APUs) and material handling units. The approach is structured around activities that address three major barriers—cost reduction of fuel cell components and materials, manufacturing capability, and customer acceptance. The approach results in cost estimates derived from detailed models of manufacturing components. For the most part, the activities undertaken address these major barriers. The approach does provide a useful sensitivity analysis to identify significant cost contributors but does not yet address details on the manufacturing sub elements that are major cost contributors. There are also limited efforts to address key uncertainties.
- Analysis starts with a market assessment, which provides a good foundation for system design and costing.
- Initial cost and manufacturing data for the APU application can be obtained from non-U.S. sources that are producing high numbers of SOFC stacks. The data used and system designs represented in this study need

to be updated; Delphi is no longer developing APU systems and changed its design prior to exiting the market.
- The researchers make reasonable estimates for most of the fuel cell system costs. This period, they have focused on SOFCs and performed a cost analysis versus units made. Their conclusions are not very surprising—namely, that materials processing limits cost in SOFCs.
- It is not clear what information is sourced from literature and what—if any—is relevant input from industry.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The details in the cost estimates for manufacturing analysis are strong and are helping to refine the overall life cycle cost of the systems. While many pieces of data were not readily available, the data presented is reasonable and helps to move the project along and bring it to the group for discussion.
- Excellent progress has been made in estimating SOFC system costs.
- The researchers have made very good progress with regard to both the specific project goals and the broader goals of the DOE Hydrogen and Fuel Cells Program. They have completed the manufacturing model and applied it to the APU case study. The models are currently in Excel format, and it is not clear to what extent the models are available to DOE or other collaborators. There was some brief mention of model validation, but few details were shared on how effective this effort was in establishing model performance goals.
- The results published seemed to satisfy what the project team aimed to do. It would have been helpful to see the similarities and differences of the 1 kW and 5 kW systems. Some of the tables were assumptions/inputs and perhaps more time could have been spent on the results, with the assumptions moved to backup.
- Key findings include the following:
    - Production volume has negligible effect on stack costs.
        - Ceramic material and commodity costs are constant across all volumes.
        - Material processing requirements limit throughput.
    - The Manufacturing Readiness Level (MRL) for many BOP components indicates they are not ready for mass production, and this is a significant cost driver.
        - DMFA performed on specific components (reformer, desulfurizer, and stack) assumes technology with an MRL >9.
- Manufacturing cost analysis was completed for SOFC APUs and polymer electrolyte membrane (PEM) material handling equipment (MHE).
- The team is adequately working toward all its goals. It seems that input from cutting-edge innovations, which might lead to dramatic changes in cost, are not included in the team’s analysis, suggesting an inadequate literature review. Such a review should be included in the future.
- The project should be reviewed by people who actually build similar systems (especially SOFC). For example, it is surprising that insulation was not a top line item. SOFC has multiple very high temperature components, which require expensive refractory insulation. This cost is not trivial. In addition, costs to retrofit the system into a vehicle are substantial—and not shown as a top line item. The learning curve on slide 20 seems off. At a volume of 100 units, items would be built and assembled by hand because tooling costs would be too expensive relative to manual labor. However, at a volume of 50,000 units, tooling would be used. The reviewer would expect the cost differential to be at least threefold, while the cost drops only 1.7 times.
- The MHE PEM analysis of cost is the best database available to the public. A critique from Ballard and Plug Power would be helpful. The costs may be excessive if design dependence on S removal with three expensive heat exchangers after the reformer is assumed. Battelle should review the InnovaTek design, which is simpler. The Battelle 5 kW SOFC system cost is almost twice that of InnovaTek. Battelle should work with InnovaTek.
- It is hard to judge if these results are of real help to manufacturers.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- Excellent collaboration was demonstrated. The following companies provided support for the costing effort:
  - NexTech Materials: System design review/feedback, SOFC technology assessment.
  - Precision Combustion, Inc.: Fuel processing technology review/feedback.
  - Delphi: System design review/feedback, BOP design comments.
  - AVL: System design review and application feedback.
- This reviewer appreciates that collaborators were clearly identified this year.
- Broad collaboration is evident, but more involvement from truck original equipment manufacturers might be beneficial.
- There was only a brief discussion of collaborations, and the presentation did not provide much detail on these collaborations. There was limited discussion of what value was added as a result of the collaborations. In particular, there could be more effort to use collaborations with manufacturers to get better insight on the likely ranges of parameters used to model manufacturing processes, as well as collaborations with other national laboratories to have consistency in the metrics used for life cycle cost.
- The researchers should add international partners with a well-defined and accurate manufacturing plan in place.
- Battelle should expand cooperation to include InnovaTek. DOE should facilitate the collaboration.
- Collaboration should be intensified and extended to more partners.
- Additional developers would strengthen this project by providing additional input.
- Input from developers of stationary SOFCs (in addition to Delphi and NexTech) are desirable.
- The collaborators might not be the optimal. For example, Precision Combustion, Inc. is not a mainstream catalyst supplier. While Precision Combustion, Inc. has an interesting product, automotive-style catalysts should be considered (e.g., SudChemie). Such catalysts are canned and toughened for automotive applications already. NexTech materials might also be a bit far upstream to consider for a system-level analysis. The researchers should bring in companies that are leading similar system integrations. Delphi's level of involvement and quality of interaction were unclear.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- This project provides background information, telling DOE whether the efforts on the development of fuel cells are working toward a commercial, energy-saving, and low-pollution product.
- This is important for DOE's status and progress related to fuel cell system costs.
- The project is well targeted and beneficial to the research goals of the Program. The relevance of this work to DOE goals could be improved with more focus on the impacts of assumptions about manufacturing on cost results and with more attention to time frame in presenting life cycle cost estimates.
- The work is addressing technical barriers that are important to the Program:
  - Cost reduction of fuel cell components and materials.
    - Identify major contributors to fuel cell system cost.
    - Quantify potential cost reduction based on technological improvements.
  - Manufacturing capability.
    - Identify major contributors to fuel cell system manufacturing costs.
    - Identify areas for manufacturing R&D to improve quality and/or throughput.
    - Provide basis for consideration of the transition from other industries.
  - Customer acceptance.
    - Develop accurate cost projections that can be used to evaluate total cost of ownership and facilitate early market adoption.
- Impacts of BOP components on SOFC system costs have been identified.

- This is potentially a great market for early adoption of SOFC systems.
- Cost analysis offers insight into important high-component-cost R&D areas and market potential.
- The elaborated cost projections give some indication of costs; whether they are really useful is not clear.
- In a negative way, this honest and fact-based assessment shows what does not work for transportation.
- There appears to be little work outside of the hydrogen focus for the DOE Fuel Cell Technologies Office; this project is very important to meeting the goals for nonautomotive applications. This project can help strengthen this area with a little more refinement.
- It is difficult to assess the impact of this project because the conclusions are somewhat obvious. The analysis was neither terribly exhaustive nor insightful.

Question 5: Proposed future work

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

- The future work holds promise because the lessons learned from the earlier project periods are planned to be expanded into many more different technologies.
- The focus for the next budget period turns to primary power and combined heat and power (CHP) from 1 kW to 25 kW for appropriate fuel cell types.
- The future work is focused appropriately on primary power and CHP applications (e.g., polymer electrolyte membrane fuel cells [PEMFCs], high-temperature PEMFCs, and SOFCs).
- The researchers have organized the work to meet the project goals in a sequential and logical manner. During the next budget period, they will focus on primary power and CHP applications. In planning their future work, it will be important to address an analysis of the sensitivity of cost analyses to assumptions about manufacturing and to consider appropriate time scales for cost estimate comparisons.
- Continuing to refine the work and move onto CHP applications is important to meeting the DOE goals. Systems from NexTech and Precision Combustion, Inc. are both capable of operation on ultra-low sulfur diesel without S removal. The reviewer is curious about what impact removing S cleanup, heat exchangers, and the material associated with that new process flow has on the total system cost and maintenance.
- The proposed work could be done, but it would be better to solidify the results obtained so far to come to viable judgments.
- PEMFCs have been proven repeatedly as being impractical and uneconomical for CHP. There is a long road scattered with the bones of those who have trekked it. Japan still has some activity, which is highly subsidized, and the heating systems applied in Japan are different than the United States. This reviewer has experience with PEMFCs and HT-PEMFCs as a systems engineer. The focus on SOFC may be prudent—especially for larger systems (100 kW and larger). Heat recovery ability has not been clearly demonstrated for such systems, and work to produce such benefits may be of benefit for the Program.
- Questions remain about the validity of the HT PEM, whether someone is taking ClearEdge Power's place, and whether the HT PEM actually works.
- A more detailed description of the future work is recommended.

Project strengths:

- The researchers are very effective in both meeting their project goals and providing results that are relevant to Program goals. Overall, the life cycle cost and sensitivity analysis is good. The determination and discussion of dominant cost drivers is useful and informative.
- This project features excellent detail and data gathering to develop the system models and start the costing process. There are good collaborations that will help the project if the partners stay engaged.
- This project is well planned and is producing interesting results. It clearly shows that the basic fuel cell stack is no longer the cost driver. The research community needs to go after BOP.
- Strengths of this project are its extensive industry contacts in the United States and rich experience in cost analysis.
- The cost estimation methodologies are a strength of this project.

Project weaknesses:

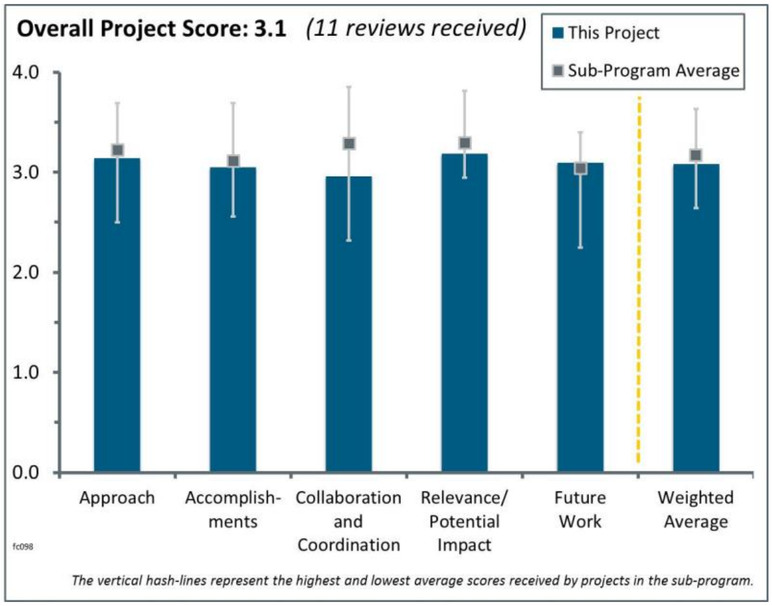
- No weaknesses can be identified.
- A project weakness is the apparent lack of coordination or comparison with similar DOE cost modeling projects. This is not to say this is not a relevant project in all the DOE cost modeling projects; it would be helpful to know more about how this project supports the DOE cost reporting mission with the other projects. Adding additional partners would be important to get more than one data point for a category.
- It is not clear that there are any collaborators who are currently or will be manufacturing the units being analyzed. The cost of ownership analysis was fixed to three years. Results in the cost comparison table may be quite sensitive to this and other assumptions.
- It was not evident that Battelle has actual experience in manufacturing components for fuel cell systems. The project is dependent on interviews with industry; it is unclear what will happen if industry says no to interviews.
- The project needs a better system analysis; this current design is outdated and inefficient. The researchers should collect data from foreign SOFC manufacturers.
- It is early in the process—the researchers need to add CHP and primary power.
- The work as presented seems to be a bit “hypothetical,” with little relevance to the “real world.”
- There is a lack of certain details (e.g., assumptions and rationales).

Recommendations for additions/deletions to project scope:

- There should be more efforts to determine sensitivity model results for the cost to manufacture methods choices. The team should work to address model quality control and quality assurance. It should also include more details on manufacturing sub elements—an effort that will be important for overall sensitivity analysis.
- The researchers should pursue more collaboration to achieve more relevant information, which will lead to more realistic results.
- The team should delete HT PEM and add collaboration with InnovaTek for 5 kW SOFC cost analysis.
- The team should make sure end users are involved.
- Comparison with previous cost estimates is strongly recommended.

Project # FC-098: A Total Cost of Ownership Model for Design and Manufacturing Optimization of Fuel Cells in Stationary and Emerging Market Applications

Max Wei; Lawrence Berkeley National Laboratory



Brief Summary of Project:

This project develops a total-cost-of-ownership (TCO) modeling tool for the design and manufacturing of fuel cells in stationary and materials handling systems in emerging markets. The framework has been expanded to include life cycle analysis (LCA) and possible ancillary financial benefits, including carbon credits, health/environmental externalities, end-of-life recycling, and reduced costs for building operation. System designs are identified that meet the lowest manufacturing cost and TCO goals as a function of application requirements, power capacity, and production volume.

Question 1: Approach to performing the work

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

- This is a well-established and good process for system evaluation. This project presents a detailed cost analysis that identifies key contributors to cost and determines realistic process-based costs. The project considers realistic production rates for combined heat and power (CHP) systems. Progress to date has been very good.
- The general objective of the project is of great interest to promote the commercialization of fuel cells. Actually, end users need information on the technical and economic viability of the fuel cell technology. The proposed approach—including the design for manufacturing and assembly (DFMA) analysis cost model and integrated LCA impacts, including life cycle costs, carbon credits, and health and environmental benefits—is correct.
- The project features a good approach and detailed investigations. It is a good complement to the work of Strategic Analysis, Inc. (SA).
- Addressing the monetized impacts of health and environmental impacts gives a much better appreciation for how closely the fuel cell systems compete with the grid. It would be interesting to see how the latest U.S. Environmental Protection Agency rulings will impact the grid levelized cost of energy (LCOE). These additional costs will be good for fuel cells. In addition, perhaps demand charges should be addressed—these can be considerable.
- Incorporating other models to minimize redundant modeling work demonstrates good collaboration.
- The approach is good.
- The work approach is designed to address fuel cell costs: expansion of the cost envelope to TCO, including full life cycle costs and externalities (Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan technical barrier 3.4.5B).
- The project has been conducted using appropriate estimating methodologies. However, information on key assumptions and the basis for assumptions is lacking.
- The approach used to get data for the manufacturing model is not clear. Cost areas were identified, but it is unclear where or from whom the project gets the cost data. Because industry has reached high production, it is unclear how confident the project team is in the ability of DFMA to yield correct costs.
- The general approach could be explained better. Including environmental and health externalities is insightful.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- The project features a very thorough analysis of the systems and components. The presentation has some great graphs that show the effects of externality components on the LCOE for fuel cells versus grid power.
- The researchers have done a great job of updating and completing the TCO model for low-temperature polymer electrolyte membrane (PEM) CHP systems (e.g., manufacturing cost model, lifecycle cost model, and externality valuation). In particular, monetizing of health and greenhouse gas (GHG) has been done. A direct cost model for a high-temperature PEM CHP system has also been realized; however, the reliability of the study is unclear because today neither HT-PEM vendors nor a stabilized technology exist, as performance and durability have to be strongly improved.
- The team completed detailed design plans and a bill-of materials and balance-of-plant (BOP) inventory for HT-PEM systems in co-generation and stationary power applications.
- The project features valuable results; the sensitivity analyses for manufacturing volume are very good.
- Progress was presented over a broad spectrum of fuel cell types and applications. The cost breakdown provides insight into research and development (R&D) needs.
- Progress has been made on estimating costs for LT and HT systems.
- The analysis or its articulation could be more consistent. For example, power electronics are spelled out for LT-PEM, but not for HT-PEM. It is also unclear if the backup power systems are alternating current or direct current, or if they have power electronics at all. There are also questions about whether hydrogen storage is present and the number of hours of operation for the systems—those assumptions need to be provided. It is strange that externalities are monetized. The reviewer was unaware of such mechanisms, other than sporadic incentives from different states. This may not be relevant in the analysis.
- For some of the CHP costs, it was unclear what was new and what was a repeat from last year. Take-aways on all the accomplishment slides would have been helpful in the review. The charts set at 100% (e.g., slide 15) were interesting to study how the different segments changed with production volume.
- SA is using DFMA—it is unclear what other technique Lawrence Berkeley National Laboratory (LBNL) could use to estimate higher production cost. SA started before LBNL, so it is unclear why DOE would have the method used by both teams. Perhaps DOE wanted the teams to crosscheck each other.
- Dependence on the grid for load following does not seem like a reasonable assumption. The project would be well served to add energy storage into the system.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- The project features a good cross-section of national laboratories and private industry. The leveraging of other models was effective, particularly for health and environmental costs.
- This project features good advising partnerships, including with the University of California, Berkeley; Ballard Power Systems; and Altergy Systems.
- There seems to be improvement in the area of partnerships and collaboration from previous years. Continued efforts to try to get information from key stakeholders are encouraged.
- The collaboration is correct, but it should be expanded to other end users and system providers. Coordination with the other principal investigator conducting the DOE projects dealing with cost estimation might be helpful for the community.
- Each scenario should have more collaboration from stack integrators and end users with current, firsthand knowledge of component costs.
- Formal collaboration could be expanded to more partners; the informal contacts are very good.
- Additional collaborations would strengthen this project.
- More inputs from fuel cell developers are desirable.
- Only Ballard was consulted; it has very limited experience with systems integration, especially considering that the majority of this work is on CHP systems.
- It is unclear who the collaborator is for the polybenzimidazole (PBI) membranes.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- The project fully supports the goals of the DOE Hydrogen and Fuel Cells Program. The significance of the project will be high because the project delivers publicly available, unbiased data, which is important for economic considerations and decisions of suppliers.
- This reviewer concurs with the expanded framework to include LCA. Ancillary financial benefits are very relevant in lowering the TCO and should be addressed in other projects that address TCO and capital expenditures (CAPEX).
- The project aligns well with the Program's and DOE's research, development, and demonstration (RD&D) objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- The project is complementing the work from SA and leads to more trust in the results for both projects.
- The cost analysis provides some guidance to R&D needs for cost reduction.
- This project is relevant to support DOE's objective of cost reporting and projections.
- The potential impact may be high because it might convince end users to buy and install fuel cell systems. However, the relevance to study the HT-PEM system, which is not ready for commercialization, is not clear.
- The impacts of BOP components have been identified.
- It is important to keep an eye on the state of the technology and its implication on potential system designs. However, it may be prudent to be frugal on technologies that have technical (not cost) challenges—namely, CHP—as LT-PEM systems have consistently been proven inadequate for heating purposes. The emphasis on this system type should be reduced.
- It is important to generate cost information and TCO. The impact may be low because of ongoing work by the National Renewable Energy Laboratory and SA.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The future work is properly focused on the go/no-go decision to see if the TCO model is satisfactorily completed for HT-PEM systems in CHP and stationary power applications.
- The proposed future work is in line with the Program and appropriate for this project.
- The proposed future work is a logical, straightforward continuation of the project.
- The proposed plan is correct, except for the continuation of the HT-PEM study. The associated resources should be put into solid oxide fuel cell (SOFC) modeling and into updating the LT-PEM model.
- There seems to be a large difference (by a factor of 5 or so) between LBNL's cost projections and those developed by SA. The future work should address this difference and account for it in terms of different assumptions, different methodologies, etc.
- The future work addresses some of this reviewer's concerns.
- A more detailed description of the future work is recommended.
- It is unclear why HT PEM is emphasized. It is not clear whether it really works or whether it is an expensive replacement for SiC in phosphoric acid fuel cells (PAFCs). The researchers should work with InnovaTek on SOFCs.

Project strengths:

- The model takes into account the total installed system, unlike the automotive systems. It also takes into account health and GHG impacts.
- Project strengths include the creation of publicly available, unbiased cost data, as well as coordination between industry, national laboratories, and consulting companies.
- The project features a very detailed capture of the system components and cost information. The analysis and resulting graphs were easy to understand and informative.

- This project is important for supporting DOE's objectives for fuel cell cost reporting.
- The estimation methodologies are a strength of this project.
- The determination of cost as a function of volume is very good.
- The project features experience in fuel cells from an academic viewpoint. There is no industrial experience.

Project weaknesses:

- The project does not do enough to make a connection between its cost estimates and the costs from actual fuel cell manufacturers. The community would like to see the cost estimates extended to lower manufacturing volumes and compared with actual, real-world fuel cell prices.
- There is a lack of HT-PEM vendors, and the technology is not ready for deployment (lifetime and performance). It is therefore difficult to see the added value of this study.
- It is hard to cover all aspects of this detailed cost analysis in a short presentation. A better job of explaining the assumptions would be useful.
- This project would be helped by having more coordination/comparison with the other projects that support DOE's cost reporting objectives.
- This project is repeating the DFMA of SA; the researchers should develop a new method. There is no industrial experience. The researchers need to depend on industry giving them the cost data.
- The project team has been able to determine with the methodology that CHP can meet the 2015 target at 10,000 systems per year.
- The lack of the full array of technical collaborators is a weakness of this project.
- A wider range of collaborators would be nice.
- There is a lack of information on the basis for assumptions.

Recommendations for additions/deletions to project scope:

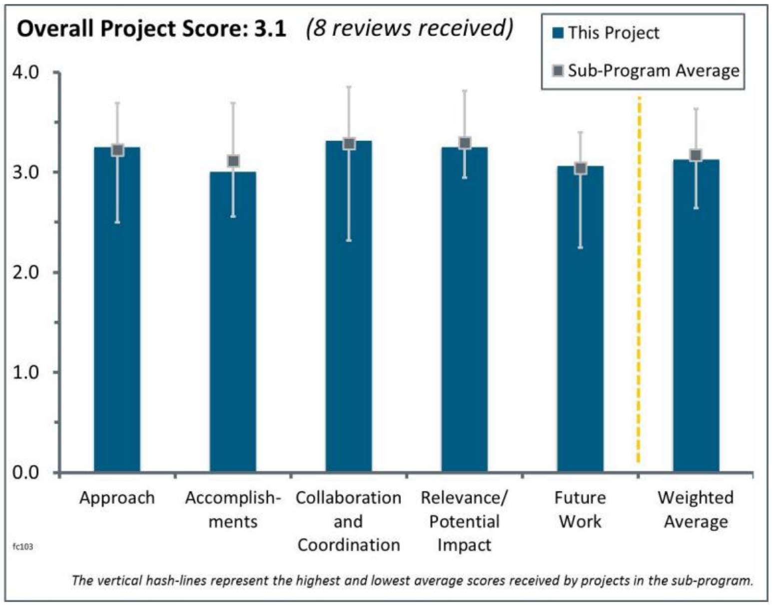
- The researchers should not put a lot of effort into HT PEM, because the technology is not ready. They should also ensure that the cost assumptions among the different cost analyses are consistent (e.g., FC-018, FC-083, FC-097, and FC-098).
- In addition to the capture of ancillary costs, it would be interesting to research the various federal and state tax incentives—such as renewable identification number credits, accelerated depreciation, and peak demand charges—and determine their effect on CAPEX and LCOE.
- The work needs more statistics so confidence levels can be established. There is a lack of HT-PEM vendors and original equipment manufacturer contacts, but the team has started discussions with Advent Technologies and PAFC contacts.
- The team should eliminate the HT-PEM effort until some industry entity makes it work. The team should also find an alternative to DFMA.
- The researchers might consider a detailed cost analysis of high-impact BOP components.
- A comparison with previous cost estimates is strongly recommended.

Project # FC-103: Roots Air Management System with Integrated Expander

Dale Stretch; Eaton Corporation

Brief Summary of Project:

The primary objective of this project is to demonstrate key improvements to compressor/expander efficiency, including compressor/expander efficiency at 25% flow, combined motor/controller efficiency at full flow, and compressor/expander input power at full flow. Secondary objectives include conducting a cost reduction analysis and fully testing and validating air management system hardware capable of meeting project targets.



Question 1: Approach to performing the work

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

- The approach is excellent because it incorporates upfront analytical work to improve the device-critical parameters before a concept is down-selected. In addition, the project includes both performance validation testing and endurance testing in an application.
- The general approach of applying computational fluid dynamics (CFD) to guide the design of the expander/compressor and an integrated motor, followed by subsystem integration validation and full 80 kW system validation, is sound.
- There are clear goals with well-defined primary and secondary objectives. The barriers are accurate and clearly presented.
- This project features good and clear approaches based on a real-world problem/issue.
- The approach is good. This project will overcome cost and reliability barriers by leveraging recent advancements to further develop the Roots blower by accomplishing the following:
  - Leveraging the broad efficiency map of Eaton’s Twin Vortices Series (TVS) compressor to improve the overall drive-cycle fuel economy.
  - Integrating the expander, compressor, and motor to reduce system cost and increase system efficiency (this is a new approach, similar to a traditional turbocharger).
  - Reducing part count, and thus cost, by incorporating overhung expander and motor rotors such that four bearings and two shafts are used.
  - Operating at a lower speed to leverage lower-cost bearings and improve system reliability.
  - Developing a net shape plastic expander to lower manufacturing costs.
- The project features an alternative approach to the compressor, eliminating the turndown weakness of air bearings.
- The project features a relevant approach with a somewhat known process map optimized for a fuel cell component. Cost remains an issue, and there does not appear to be much further opportunity to reduce the component cost to the target.
- The approach is classical analysis, design, build, and test. For the most part, it looks as if Eaton is selecting existing subcomponents and assembling them into a compressor/expander module. There seems to be little innovation.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- Compressor and expander CFD work has been impressive. Work in analyzing tip clearance and improving fidelity is impressive. Seven compressor studies and 10 expander geometries provide excellent data and demonstrate clear understanding of how to achieve performance and meet DOE goals at the system level.
- Kettering University's progress is highly relevant and shows good progress with respect to overall project progress.
- The team has made good progress on its goals. The go/no-go metrics are not clear. DOE approved of the project moving forward even though some of the targets are not met by the proposed solution. It would be useful to see the trade-off analysis on the critical parameters and understand why specific decisions were made.
- Eaton has made the following good progress:
    - CFD modeling: Eaton has made strides in modeling three- and four-lobe Roots expanders and compressors. A tool was used to optimize designs.
    - Expander plastic rotor: Plastic rotor hardware has been successfully demonstrated through testing.
    - Develop compressor/expander with integrated motor: Improvements have been made to the expander and compressor design. Compressor/expander matching has been improved as well. The design has been completed, with detailing in process.
    - Hardware procurement: The Ballard Fuel Cell Module Test Specifications, Procedures, and Acceptance Criteria were defined, and a purchase order was issued. Some hardware was ordered.
- Applying STAR-CCM+ enabled a 50% improvement in modeling tip clearances. However, this implies that the modeled tip clearances are still 3.5 times greater than production clearances. The reviewer wonders whether this is a limitation to using CFD to guide compressor/expander design. The reviewer also wonders whether the use of "special software" that can handle production clearances is computationally efficient and viable for exploring a wide variety of design options. It is stated "to improve the expander flow performance, clearances have to be tightly controlled." It is unclear what the absolute dimensions of these clearances are, and whether they can be reasonably maintained with lower-cost plastic components. Also relative to tip clearances, it is unclear whether the effect of thermal expansion has been accounted for over the entire range of expected operating temperatures.
- The progress toward building and testing is okay. Slide 3's projected performance status does not meet some key efficiency targets. Some originally proposed concepts have been abandoned, such as common shafts, and it is not clear whether plastic molding will be successful. Design and build tasks are on track.
- It is too early to tell if Eaton's compressor/expander design is working. The costs are high and similar to commercial turbochargers.
- According to the summary slides, some elements of the project, such as cost, are quite far from the target values. The future work does not quite explain how to achieve the targets.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The amount of collaboration is great, and it is in the correct areas, from the analytical modeling work to the final product testing.
- There is good support from Kettering University. The project partners are highly relevant to both furthering technical understanding and gathering original equipment manufacturer experience.
- The project has a good collaboration team.
- The project features fairly reasonable collaborations.
- Collaboration areas include systems and cost analyses, CFD, and a stack/system integrator.
- The upstream partnerships (subsystem and system level) are strong, but the project could benefit from a motor/controller partner because these components seem to be the primary barriers to meeting the cost target.

- Collaboration is evident; however, all listed entities are subcontractors. It is unclear whether there are any relationships developed for mutual benefit, and whether that is an option.
- The prime is Eaton. The subcontractors are Ballard Power Systems, Kettering University, and Electricore, Inc. Technical support is provided by Argonne National Laboratory and Strategic Analysis, Inc.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The project aligns well with the DOE Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- The project has broad relevance to balance-of-plant (BOP) components and the long-term durability of the fuel cell stack/system.
- There is good progress toward the goals, and more emphasis on BOP equipment is needed; so it is great to see this project supported by DOE. It is not clear how the proposed technology plans to achieve the DOE targets.
- BOP components are responsible for a significant portion of costs and for most system failures.
- Compressor/expander performance is essential to meeting the DOE targets for system performance.
- If successful, the project should have an important impact. There is not enough data yet.
- There are many projects focused on fuel cell catalyst/support material, membrane electrode assemblies, and stacks. However, fuel-cell-focused component development is very important for successfully launching new fuel cell electric vehicles.
- The project needs more focus on the motor to achieve the efficiency and cost targets. Although the work on low-cost plastic components is important, it was stated in the presentation that the total cost impact of plastic versus aluminum is small.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The project has a good plan to perform the endurance validation testing for one year with a partner. This plan could be improved to include additional validation testing under simulated conditions at Eaton that would expand the learning with multiple test units. In addition, accelerated testing should be considered in order to determine any potential failure modes quicker and possibly mitigate them before the end of the project.
- It will be good to see the hardware built and tested to validate models. Integration into a Ballard fuel cell module is also critical to overcoming barriers.
- The project team has done a good job of developing plans for 2014 work:
    - Prototype compressor/expander with integrated motor: Fabricate and qualify prototype components, and determine production cost estimates.
    - Test compressor/expander with integrated motor at Eaton.
    - Undergo project review with DOE, and report progress against go/no-go criteria.
- Testing and evaluation of the integrated air management component (with go/no-go decision), followed by full-system testing and evaluation, seems adequate.
- The project is on the right track for testing.
- The future work entails a continuation of the analysis, design, build, and test progression.
- There do not appear to be any further means of reducing cost or increasing efficiency.

Project strengths:

- Strengths of the project are its strong product knowledge and collaboration with significant fuel cell partners. The academic partner provides solid computational support of in-house testing activities. There is a clear pathway from component development to subsystem evaluation to full system evaluation.
- This project features great collaboration. The project targets an area that needs more development to meet the DOE targets.
- The project excels with CFD models and system configuration and analysis.
- Eaton has a good reputation for BOP components such as compressors.
- This project has a good team.

Project weaknesses:

- The project needs to involve partners/collaborations that provide the skills and knowledge required to close the significant cost gap versus the target.
- The project may need an independent test facility when the compressor prototype is ready.
- The project is not very innovative.
- The project has limited testing.
- The progress seems slow.

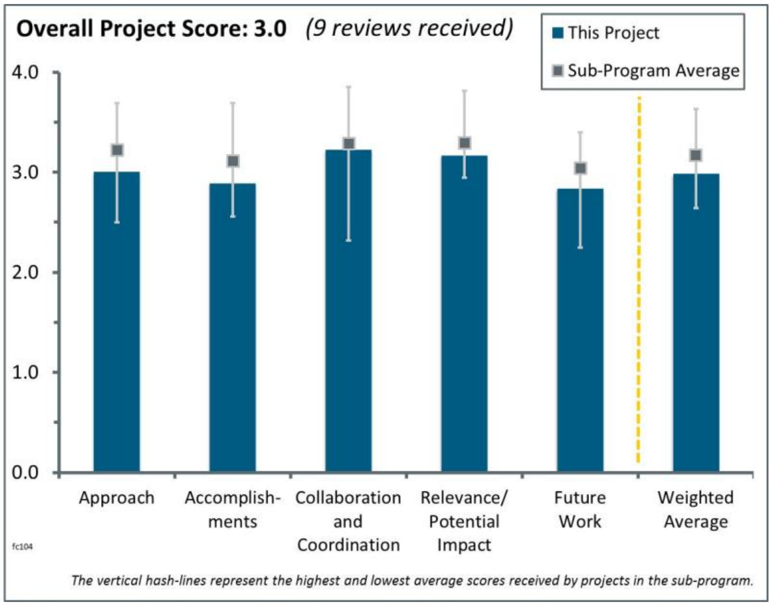
Recommendations for additions/deletions to project scope:

- DOE should support this project.
- The research team may be able to scale back the plastic rotor development work because this does not seem to significantly benefit either the cost or efficiency gaps.

Project # FC-104: High-Performance, Durable, Low-Cost Membrane Electrode Assemblies for Transportation Applications

Andrew Steinbach; 3M

Brief Summary of Project:



This project's overall objective is the development of a durable, low-cost, robust, and high-performance membrane electrode assembly (MEA) for transportation applications that is able to meet or exceed the U.S. Department of Energy (DOE) MEA targets. Primary objectives and approaches this year include: (1) improving MEA robustness for cold start-up and load transients via materials optimization, characterization, and modeling; (2) evaluating candidate MEAs and component durability to identify gaps; (3) improving durability through material optimization and diagnostic studies; (4) improving activity, durability, and rated power of MEAs based on Pt_3Ni_7/nanostructured thin-film (NSTF) cathodes via post-processing optimization and characterization; and (5) integrating MEAs with high activity, rated power, and durability with reduced cost.

Question 1: Approach to performing the work

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

- The project is focused on improving the performance of 3M NSTF MEAs in wet conditions and during transients. These MEAs have some of the lowest platinum group metal (PGM) loadings and highest activities demonstrated. Addressing the shortcomings under transient and cold/wet conditions will address key barriers to adoption of this low-loading MEA.
- The approach is to improve the performance and durability of the high-performing NSTF catalysts in MEAs and to integrate the MEAs in fuel cell stack testing. One element of the approach is to improve the MEA robustness for cold start-up and load transient via materials optimization, characterization, and modeling. This is one of the most important and needed aspects of the project.
- The objective of this project is to meet the MEA performance and stability targets, and the approaches to meet these targets are clear. There appears to be a good balance of fundamental studies and operational MEA work to try to elucidate, and counteract, challenges relating to performance and durability. While the objectives are sound, the project seeks to overcome some considerable technical hurdles without making significant changes to the electrocatalyst layers (per the original project plan). In this regard, the scope of work is very ambitious, relying on a finite number of possible parameters to counteract the MEA performance sensitivities.
- The principal investigator (PI) employed a traditional 3M approach that incorporates the NSTF catalyst technology platform. The applied strategy is excellent, and very few details need to be added in order for it to be outstanding. One missing part is the in situ characterization of morphological changes during dealloying and/or during the oxygen reduction reaction (ORR). Considering that J. Erlebacher is a part of the 3M team, the PI should consider utilizing his expertise in understanding the dealloying process.
- The 3M group has set the standard for low-PGM MEA development. The approach for this project is based on further optimizing the NSTF catalysts' platform and initiating testing in short stack with General Motors (GM). The researchers' approach addresses MEA durability, stack material and manufacturing costs, and MEA performance. MEA durability and performance are the focus of the present work.

- The approach is good, considering the constraints of the NSTF catalysts. There has been good effort to perform tests under a range of conditions. The major deficit is that the researchers are bound to the NSTF catalysts, forcing them into certain directions specific to the geometry. The catalysts have typically been prone to flooding at high power, but for some reason this does not show up in the tests. The ORR activity appears to fall off rapidly (see slide 10) by a factor of two over time at 1 A/cm^2. The time is not clear. It seems like the researchers are doing many tests, but perhaps the most relevant is the short stack testing.
- The project addresses DOE barriers related to cost, durability, and performance.
- The Pt/C interlayer is of limited value because there are additional process costs and durability issues with inclusion of such a layer. The approach appears to have a significant chance of meeting cost and performance targets. Although the different anode gas diffusion layer (GDL) designs have demonstrated improvements, MEA temperature performance is still significant and would be difficult to incorporate "as-is" in an automotive application.
- NSTF has serious issues that must be addressed if this novel catalyst architecture is ever going to be adopted as bill of material by any of the polymer electrolyte membrane fuel cell (PEMFC) developers. These issues include its difficulty to break in, high sensitivity to contaminants, extreme sensitivity to low temperatures, and durability. The only way to address these serious issues is to make some serious changes to the electrode configuration. Instead, this project is focused on minor changes that are having only a minor impact. The PI stated that component development is not allowed under the topic for which the project was selected. This reviewer's experience with DOE's Fuel Cell Technologies Office has been that there is considerable flexibility when it is obvious that something different must be done than what was the intent of the original proposal or solicitation. It is highly unfortunate that the PI wants to just continue pursuing work that will not have a major impact.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- There has been a tremendous amount of technical progress and accomplishments—it is almost overwhelming. Almost all of the metrics have been achieved. All of the MEA components were studied using many different and relevant characterization techniques. 3M tested four anode GDLs and down-selected one GDL that has higher water removal capability for MEA integration. The model agreed with the experiments well. It was unclear which of the 4–5 GDL characteristics would have the most impact on cold start-up and water removal; in other words, whether it would be the pore size distribution, contact angle, injection pressure, wetted area (it is unclear if this is measured with water or some other solvent), or thickness that would have the most significant impact, or whether it would be the interaction(s) among the different parameters that have the most impact. This understanding would help one design better GDLs. It is unclear whether the cathode GDL has also been studied for cold start-up, water transport, and transient operation. It is also unclear whether the team plans to use a similar approach to study the effectiveness of the catalyst layer in cold start-up and water transport. An interlayer development was discussed and shown to improve the robustness of the cathode for cold start-up and load transient, but one does not know what this interlayer is and how it improved the performances indicated. Without this knowledge, it would be difficult to design better catalyst layers for this purpose. Factors affecting membrane durability were studied: load cycle temperature, polymer electrolyte membrane (PEM) equivalent weight, and PEM additive. The presenter did not mention what additive or ionomer was used and why these factors resulted in the improvements shown. Also, these fuel cell tests were conducted at higher cell temperatures (80°C, 90°C, and 100°C). It is unclear whether these parameters would have much effect on the cold start-up and transient operations. Annealing and dealloying of NSTF catalysts have shown to improve the mass activity of Pt_3Ni_7/NSTF catalysts.
- Significant progress has been made toward improving anode performance during cold start. Introducing the Pt/C interlayer to address low-temperature steady state and load transient performance does not look like a promising solution. It leads both to an increase in Pt loading and well-known durability problems with carbon supports that NSTF was designed to address. No evidence is provided that performance loss in the presence of the PEM additive is due to anionic contamination. The features in the hydrogen underpotential deposition region before and after the durability tests on slide 58 look almost identical in the presence and

absence of additives in the membrane. The researchers made fast progress in obtaining modeling insights on the effect of GDLs on the performance loss during cold start. Progress toward improving cold-start performance needs to be accelerated in order to start short stack testing next year.

- The PI presented convincing results and confirmed that the thermal annealing protocol is indeed important in improving the catalytic activity (and stability) of NSTF catalysts. As in the past, activity and stability are not an issue for the NSTF catalyst, but water management is still a bottleneck for fully implementing this technology in fuel cell systems. Regarding the active surface area, it is not clear that the project team developed a method that would be able to unambiguously determine the specific surface area of the NSTF catalysts. Rather than using modeling to "capture many experimentally observed trends," it would be very important to see if modeling can be utilized to predict how, or even if, the issue of water management can be overcome in these systems. It is unclear what has been learned from modeling.
- The technical work presented is extremely detailed, and substantial effort is clearly being applied to overcome the challenges of the NSTF layers. These challenges do, however, seem to be increasing in number and relate not only to the known issues of water handling and variable temperature operation, but also to the fundamental activity and stability of the electrocatalyst. Attempts to deal with the latter have offered some success, although it is acknowledged by the PI that these may be insufficient. A range of approaches are being explored to improve the overall performance of the MEA in the diverse range of conditions in which it is required to operate. Clearly, some are proving advantageous, but not to a degree that provides encouragement that the performance targets are attainable.
- The progress is good. The researchers have achieved much advancement with the dealloyed catalysts, although the results are not as striking as those reported by the National Renewable Energy Laboratory and Argonne National Laboratory (ANL). The researchers still need to do a lot more work in order to reach the DOE targets. It was not clear from the data presented if they are on a successful trajectory. Short stack testing is the most critical. The short stack testing is only 10% complete, however.
- The researchers have increased performance at rated power by 60 mV since last year, up to 932 mW/cm^2 (7% increase). They have also decreased PGM total content to 0.138 g/kW and loading to 0.129 mg/cm^2 (12% and 6% decrease since last year). They have moved backwards on performance at 0.8 V while increasing performance at rated power. Load transient operation has been improved. They have met their go/no-go targets for mass activity and high power activity.
- The project team has shown progress in meeting/exceeding the DOE fuel cell goals for catalyst mass activity and durability.
- It would be beneficial to state the bounds of error of the measurements of PGM/cm^2 and potential at high current density as they relate to the go/no-go decision. The researchers made excellent progress on Task 5 toward the targets, as well as in fundamental understanding of exhibited performance.
- To date, this project has focused on "Band-Aids," such as these during the past year:
    - Using different anode GDLs, which have minimal impact because performance is still inadequate at 30°C.
    - Adding complexity and cost such as "interlayers" (the reviewer thought a major advantage of NSTF was supposed to be the elimination of Pt/C.
    - Optimization of the alloy composition (the value is not clear if the electrode is not robust).
    - The PI is changing both the GDL and the catalyst composition, as well as adding an extra layer, which may be perceived as "component development." The reviewer questions why changing the catalyst-layer architecture is not allowed.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- There are good collaborations with Oak Ridge National Laboratory (ORNL) and Johns Hopkins University (JHU), resulting in improved activity through dealloying and improved understanding of structural changes. Collaborations with Lawrence Berkeley National Laboratory (LBNL) and Michigan Technological University (MTU) seem to be leading to improved transient response.
- Good correlation has been observed between the modeling work and experimental tasks relating to the GDL selection, with obvious learning through this collaborative effort. Effect utilization is also evident of ex situ characterization capabilities of the project partners. The interaction with JHU appears to have been

particularly fruitful, with very clear benefits in stability observed by the annealing of the metallic components. The results observed are consistent with those reported in other projects.
- The teaming and collaboration are very strong, involving national laboratories, industry, and universities. The project features a strong team with very relevant capabilities.
- The 3M group has collaborated well with other institutions. The group has worked particularly well with MTU in evaluating water distribution in advanced GDLs.
- The collaboration is good and the partners are well coordinated with the leading PI.
- There is close collaboration with ORNL, MTU, and LBNL
- This project features well-selected partners, good progress, and highly relevant results from partner organizations. A greater emphasis on GM stack data, once available, would be beneficial to understand original equipment manufacturer (OEM) experience with the latest solutions developed under this project.
- The project features a good team, and the members appear to all be used effectively, with the exception of GM.
- Although the list of project "partners" is impressive, the PI does not actually appear to be responsive to the reviewers' comments. The partners are generally asked to help with 3M's "Band-Aids," such as measuring the water permeability of GDLs, instead of actually striving to understand and address the root causes for the major issues with NSTF-based electrodes. For example, if 3M was really interested in taking maximum advantage of collaboration, then results from LBNL's project on low-temperature sensitivity would be seriously considered and addressed.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- The research being led by the 3M group is critical for developing an advanced MEA that can meet the DOE Hydrogen and Fuel Cells Program's goals. Testing advanced MEAs in hardware developed by system integrators such as GM is an important step to meeting fuel cell system development goals.
- The project has the potential to significantly advance progress toward DOE goals and objectives. The project has significant durability issues to overcome.
- The main cost reduction pathway in this project is reducing the catalyst loading. This is relevant. It is not clear whether there are efforts in reducing the cost of other fuel cell materials and components as well—this would be a good path forward. It would be interesting to see how durable the lower-loading catalysts would be, especially with the contamination issues discussed.
- The project's potential impact is high, if the robustness and cold-temperature operation of NSTF MEAs can be improved to meet automotive requirements. NSTF MEAs have shown high activity and performance under their optimum conditions, but operational robustness has limited adoption of this technology.
- The project could potentially have a high impact if barriers to performance can be addressed. Addressing performance under load transients, low-temperature operation, and demonstration of some fraction of the reported performance in OEM testing would vastly impact the state-of-the-art PEMFC performance toward DOE goals.
- The development of cathode materials for successful implementation of fuel cells in transportation is of paramount importance. If successful, the NSTF technology may play a key role in the future of alternative energy systems in acidic environments.
- The potential impact of a successful NSTF project has been apparent for some time, and this has not lessened. However, the materials still remain a long way from practical application despite many years of research.
- It is hard to evaluate the impact of the MEAs if they are not broadly available or being tested in a relevant format.
- Unfortunately, after much investment by DOE due to a promising start over a decade ago, it appears that NSTF is becoming irrelevant to the PEMFC community. This does not necessarily have to be the case, but unless serious changes are made, no serious improvements will result and NSTF will not become part of the bill of material for PEMFCs.

Question 5: Proposed future work

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

- The project has an effective approach to future work. The key feature is the integration of advanced MEAs into short stacks for testing. Durability would best be assessed in the short stack testing.
- The proposed future work is focused on overcoming performance and durability issues.
- With the short time left on the project, the proposed future work is reasonable and a good path forward. Durability and stack testing of the best-of-class MEA components and low catalyst loading would be most critical (Task 5 and 6).
- The project is scheduled to be completed this fiscal year. The proposed work addresses the key needs to finish the project. The work appears to be more than can be done in the time remaining.
- The proposed future work is reasonable; however, the PI did not demonstrate how feasible it is to expect any further improvement in the development of stable cathode materials and efficient water management.
- The proposed future work is consistent with the project scope and with the outstanding challenges for the project. However, there is little project time remaining to bring into effect the numerous tasks suggested.
- The researchers should develop a root-cause understanding of what causes these major problems (especially relative to Pt/C-based CCMs): difficulty to break in, high sensitivity to contaminants, extreme sensitivity to low temperatures, and durability issues.
- There is clearly a lot of work to be done to get this type of MEA to work. The benefit for the government is unclear.
- If contaminants, etc. are the cause for performance decay, improving activity will not help to increase the performance in MEA applications. The development of recovery methods, although possible as a means of negating lasting performance decay, is not preferred, because of the difficulty of implementation on-vehicle.

Project strengths:

- The project team, which now includes GM, has a strong mix of both research and system development. With 3M spearheading MEA development, the group has made impressive progress toward achieving DOE fuel cell technical targets.
- This project is starting with a high-performing and highly durable cathode catalyst. The project team has shown progress toward DOE targets and since last year. The inclusion of an OEM partner is beneficial to furthering the understanding and observed performance of developed materials.
- A strength of the project is the well-integrated team that can take the information from characterization and basic studies and apply it to improve results.
- The project heavily relies on the previous 3M approach. It is encouraging that the PIs are focusing on the real problem and that they are trying to combine fundamental knowledge with real applications.
- NSTF is a promising catalyst architecture—with many potential benefits—that might be fully realized in a new type of electrode architecture.
- Having a strong modeling team onboard helps to get insights on cold-start performance issues.
- 3M's expertise in NSTF catalysts and the team are project strengths.
- The continued work on NSTF catalysts is showing slow but steady improvement.

Project weaknesses:

- It appears necessary to take a number of steps outside of the electrocatalyst layers directly (i.e., the substrate choice or use of pressure differentials) to obtain the best performance from NSTF layers, imposing atypical demands of the end user. A number of the challenges appear to relate to the absolute limitations imposed by the material set and that appear unlikely to be overcome with the current electrocatalyst layer structure. The sensitivity of the electrocatalyst layer to the suggested ionomer contaminants is a concern; not least because it suggests the probability of some significant issues in real-world operation in which balance of plant and air contaminants will also come into play.

- Transient conditions and operation at non-ideal temperatures (including start-up) are still issues for the NSTF catalyst. Extra interlayers of Pt/C add complexity and cost to the MEA. The inclusion of a PtNi-type catalyst has issues with respect to Ni leaching and contamination of other stack/system components.
- It is not clear whether 3M is ever going to get to the 2017 goals with the NSTF catalyst structures. The researchers should be doing the hard tests earlier instead of doing tests that emphasize the benefits of their catalysts.
- It seems that the project has too many problems to address. In order to solve one problem, a solution is proposed that brings another problem.
- There are no real breakthroughs with this type of NSTF catalysts. It is disappointing that there is no attempt to develop a new class of materials that may surpass the stability of PtNi-based alloys.
- Although it is out of the scope of research, MEA comparisons using commercially available Nafion® would be beneficial and particularly useful for the durability studies.
- There is a serious lack of root-cause analysis to address major issues.

Recommendations for additions/deletions to project scope:

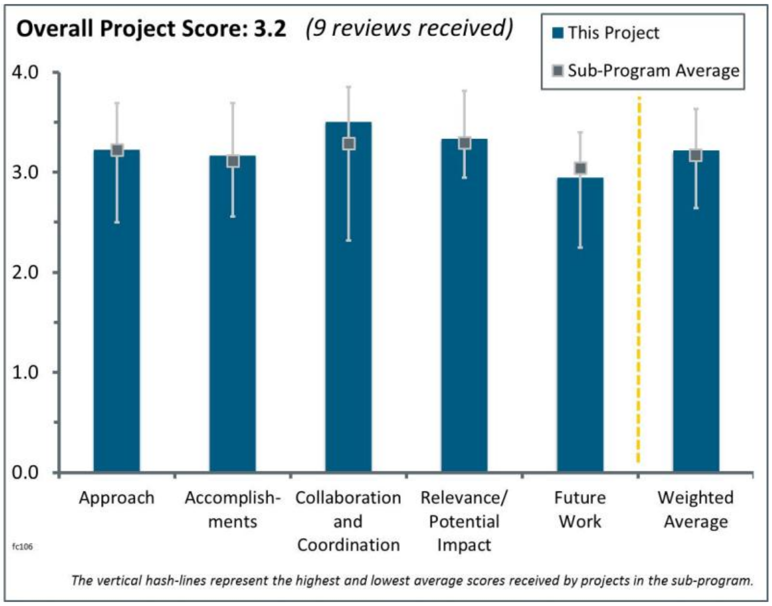
- Catalyst development should be deleted from the project scope. Dealloyed and annealed Pt_3Ni developed by ANL performs well at NSTF support, according to the 2013 DOE Hydrogen and Fuel Cells Program Annual Merit Review report. It was also demonstrated that three Pt monolayers are required in order to protect the alloy core while maintaining high activity.
- Some analysis should be done on the impact of moving water out of the anode on the system and system efficiency. Hydrogen utilization, anode recycling, and purge frequencies during operation may all be impacted, and there should be a check to see if there are negative impacts, and if so, how severe they may be.
- The PI should motivate subcontractors to contribute even more.
- DOE should tell the PI that modification of the catalyst-layer architecture is not only allowed, but required.

Project # FC-106: Rationally Designed Catalyst Layers for Polymer Electrolyte Membrane Fuel Cell Performance Optimization

Deborah Myers; Argonne National Laboratory

Brief Summary of Project:

The overall objective of this project is to realize the oxygen reduction reaction (ORR) mass activity benefits of advanced Pt-based cathode electrocatalysts in membrane electrode assemblies (MEAs) and stacks operating at high current densities and on air and at low platinum group metal (PGM) loading. Objectives in the current year include: (1) determining the catalyst and cathode layer properties responsible for the decline in advanced Pt-based cathode air performance at >1 A/cm², (2) developing a cathode catalyst layer model for an advanced Pt-based catalyst, and (3) developing a method to impart proton conductivity to high-surface-area carbon supports.



Question 1: Approach to performing the work

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

- The project focuses on pinning down the structural and morphological properties of dealloyed-PtNi (d-PtNi) cathode layers that limit high current density performance. The team is using extensive in situ, ex situ, and in-cell characterization of electrodes. An excellent experimental design matrix on catalyst inks of 60 compositions is utilized for analytical characterization. The strong modeling effort of mass transport losses complements the characterization results.
- The approach is focused on overcoming performance barriers for advanced catalysts at high current densities. Although modification of the structure of the catalyst layer by changing the ionomer-functionalizing support might help to prevent Ni dissolution in d-PtNi catalysts, it might also create new mass-transport problems related to changing hydrophobic and hydrophilic properties of the catalyst layer. Overall, the approach seems a little bit strange because the structure of the catalyst layer is intended to be drastically changed due to durability problems with the dealloyed catalyst.
- The multidirectional approaches taken by the team for the completion of all tasks are adequate. All the analytical techniques have been thought through appropriately. The study of advanced Pt-based catalyst, developed by Johnson-Matthey Fuel Cells Inc. (JMFC), is a good way to understand the behavior of nanoparticulate catalyst materials under DOE cycling conditions.
- Making a catalyst based on performance-limiting properties appears to be feasible based on the project progress.
- The use of d-PtNi is justified by the fact that it has demonstrated the ability to meet both beginning-of-test and end-of-test mass activity targets for Pt dissolution cycling. The project should be open to other catalysts such as the nanoframe catalysts, mesostructured thin films, and other catalysts with high activity. These catalysts will also need to be integrated into catalyst layers while facilitating high proton conductivity and proper catalyst layer structure. It is good to see that a comparably high-surface-area Pt/C is being used as a baseline. That is the right approach. Characterization of both inks and catalyst layers will be necessary to understand how processing, structure, properties, and performance are all related. There may also need to be characterization of inks and catalyst layers at various stages. It would be good to see whether both wet and dry mapping of catalyst layer components will happen.

- The team has focused on integrating d-PtNi nanoparticles into MEAs, focusing particularly on small, non-porous materials. Fuel cell performance is used as a baseline, which is important, and the team has been focusing on the problems of Ni leaching during use and/or synthesis and its impact. There is no evidence for the team's critical assumption that high current density performance is limited by mass transport through the ionomer. This seems to result from modeling that shows a lower surface area enhancement factor. However, in d-PtNi, it is well known that the hydrogen adsorption is intrinsically smaller (by more than one half) than on Pt, which the researchers show. It would be useful to probe the dealloyed catalysts for surface Ni, which should show up as hydroxide peaks while scanning in base.
- The approach to optimize the cathode catalyst layer specifically for alloy catalysts at low PGM loading does merit some investigation; however, it is unclear whether a detailed investigation of the matter is needed. The approach of modifying the ionomer-to-carbon ratio and the solvent composition in the electrode design has been studied for decades and is more of a development approach than a new idea that could dramatically improve cell performance.
- Gaps to the ultimate targets are not shown. It is not clear what the path is to meet the barriers listed at the beginning of the presentation and whether the project has taken these as targets. The impact table provides the status against targets, but there is a gap because the table contains no high current density targets, whereas the project objective is focused on high current density performance. The durability barrier is not addressed to date, but there has been good work on looking at Ni dissolution. The project has a well-designed approach, with a good balance of fundamental and empirical studies to understand the effects observed, such as the agglomerate size/solvent studies looking at interactions with catalyst type and ionomer concentration, the Ni oxidation effects of solvent concentrations, and modeling approaches. There are good, detailed characterization studies to support the observed effects.
- The reason for the catalyst performance loss at high current densities may already be known based on mass transfer coefficient measurements at low catalyst loadings, non-PGM catalysts' behavior at large current densities, and contaminant effects effectively decreasing active surface area: the decrease in active centers implies a larger oxygen flux and mass transport impact through the ionomer covering the catalyst (slide 27). From that standpoint, the work should be refocused to the design and fabrication of catalyst layer structures that minimize that transport loss. Other approaches should be considered to reduce the risk (e.g., thinner ionomer coating, new catalyst layer structures). The model's usefulness cannot be assessed because no information is provided. The team could use the results of the FC-049 project rather than creating a new model. It is not clear what the stability of the catalyst support proton conductivity (slide 17) is. In addition, it is unclear whether the proposed approaches consider this requirement. Anecdotal evidence from long-term degradation suggests that mass transfer changes in electrodes may be linked to surface functionality modifications. Therefore, the reviewer wonders if it is reasonable to expect stability of the carbon support modifications under long-term conditions.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- Addressing the mass activity loss at high current density of state-of-the-art PtNi electrocatalysts impacts critical limitations of dealloyed catalysts. The composition of PtNi was optimized, which resulted in a highly active catalyst with controlled particle size and the smallest Ni amount. All project milestones were exceeded. The model demonstrated that interaction between the ionomer and the catalyst may be directly related to mass transport losses.
- The team has exceeded the DOE 2020 target for ORR mass activity. The team has also demonstrated minimizing carbon agglomerates through annealing of Pt/C inks. Promising proton conductivity has been demonstrated by functionalized carbon black, which addressed the low performance of SEF catalysts. The mass transport losses related to lower surface area enhancement factors (ECSA/electrode area) of d-PtNi were also demonstrated by modeling.
- Good progress has been made in meeting and exceeding DOE targets for catalyst performance.
- The early milestones have all been exceeded, but they were based on performance improvements only, and the link to the ultimate targets is not shown. Improvements in voltage due to cell compression should not be considered improvements in technology, but rather indicate an improper setting of the baseline. Therefore,

exceeding the goals is not a significant achievement. The project team is wisely resetting the targets; however, the targets should be set with a clear path to ultimate objectives. It is suggested that the researchers set a goal where the expected kinetic benefits of the PtNi are realized at the low current density, and the high performance is maintained out to at least 1.5 A/cm^2 (better than Pt) and also meets >1,000 mW/cm^2 at this current density with <0.125 mg/cm^2 PGM.

- Good progress has been made toward improving the activity of the version 1 dealloyed catalyst, which did not perform well. Not too much progress has been made with respect to improving the durability of the dealloyed catalyst.
- The mass activity of the catalyst exceeds the DOE 2020 target, but voltage loss remains high at high current densities due to high O_2 transport resistance local to the Pt surface. There is no clear path to reduced local resistance.
- It is not clear what is causing the gain in performance observed with successive generations (JMFC results on slide 7). It is unclear if these performance changes are only variations due to manufacturing variability, or whether they are the result of modifications based on a specific hypothesis. This is an important question because confidence needs to be established that these results are reproducible and scalable. An improved catalyst formulation given on slide 8 is an insufficiently clear explanation. It is unclear if the thickness of the ionomer layer covering the catalyst is being measured, because it is expected to play a significant role in the mass transfer loss. For example, the ink solvent may affect the ink rheology and the thickness of the ionomer layer. The other parameters mentioned on slide 9 are not considered as important from a mass transport point of view. From that standpoint, the reviewer wonders if the propanol d-PtNi ink leads to a smaller mass transport loss. Presumably for an equivalent ionomer content, the ionomer film is expected to be thinner, considering the smaller ink particle size and higher surface area (slide 11). It is not clear whether the presence of Nafion® in the ink decreases the free Ni ion concentration by ion exchange favoring Ni dissolution with a lower Nernst potential for Ni (slide 12). The Ni dissolution mechanism of d-PtNi catalysts during cell operation (slide 13) is not clear. The model description is insufficient (slide 14) and does not allow any criticism of the assumptions made. For instance, it is not clear why the anode overpotential is neglected, especially for high current densities, and whether the mass transfer loss correlates with surface activity (slide 16).
- It is difficult to understand how the mass transport losses are associated with the d-PtNi because mass transport losses for it and the annealed Pt are the same. The project team achieved a very interesting result on the lack of ionomer content effect on d-PtNi. Baseline polarizations should begin with optimized compression. Project milestones should preferably begin with the first catalyst (which appears to be the d-PtNi coded “12/409”) and with optimized compression. Sulfonic acid group functionalization of catalyst powders has already been completed and could result in a reduction of ionomer content. This could prove to be important for eliminating a poorly engineered catalyst layer component.
- The accomplishments of this high-quality team are surprisingly poor. The team claims improved cathode electrode performance by an optimization of cell compression; this optimization is needed either because of an initial improper cell design or a behavior that is unique to the alloy catalysts. If it is the latter, a detailed investigation of why alloy catalysts need an optimized cell compression should have been provided. If it is the former, then this performance improvement is simply a cell design improvement and not something that contributes to the fuel cell community. The polarization curve shown on slide 7 looks almost identical to the starting point polarization curve shown on slide 3. The shape is remarkably the same, indicating that no fundamental electrode improvement has been made.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- Collaborations are clearly shown throughout the project. JMFC is heavily engaged in catalyst preparation, ink preparation, and post-mortem characterization. United Technologies Research Center (UTRC) is engaged in cell testing. The University of Texas (UT) is characterizing individual nanoparticles, while Indiana University – Purdue University Indianapolis (IUPUI) is involved in the surface functionalization of catalyst powders. In general, the collaboration appears well coordinated and each partner is more than just a service provider. JMFC, UTRC, UT, and IUPUI all have the ability to heavily influence the project. The project benefits from having a catalyst/MEA supplier involved, JMFC. JMFC should be able to provide feedback about whether new integration techniques for structuring catalyst layers are industrially feasible. The project also benefits from having General Motors (GM) involved as an in-kind partner, to provide cell and gas diffusion layer context. This is particularly true given that some of the targets are related to high current density performance.

- The team consists of a good mix of university, national laboratory, and industrial partners. The collaboration with UTRC and JMFC is advantageous to the team.
- The team interactions and collaborations are very strong. There seems to be good communication and materials transfer between the different groups.
- The results shown in the presentation indicate excellent collaboration among national laboratory, university, and industry partners.
- Good collaboration is demonstrated, with a large number of partners, and the results from a number of partners show the good integration between project members.
- This project features an effective mixture of different organizations with significant experience.
- The project features a very well-coordinated effort between different institutions.
- The project features a strong team involving contributors to all components of the project, including catalyst manufacturing, fabrication and testing of catalyst coated membranes, extensive characterization, and modeling contributions. Purdue University does not seem to fit into the project at this stage.
- Collaboration with the partners could be improved. The carbon surface treatment by IUPUI, although successful, never made it any further. It is unclear whether it improves the alloy performance in a fuel cell.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- Achieving high mass activity of advanced Pt electrocatalysts at high current density is of great significance to the commercialization of these materials. This project brings in fundamental understanding of limiting factors in dealloyed Pt catalysts through extensive characterization, modeling, and testing. Determining the interaction between ionomer and catalyst and its potential link to mass transport losses is a first step toward developing strategies for addressing activity loss at high current density.
- The project is relevant to the objectives of the DOE Fuel Cell Technologies Office's Multi-Year Research, Development, and Demonstration Plan. All activities are aligned with DOE's goal. The focus of the project is to realize the ORR mass activity benefits of advanced Pt-based cathode electrocatalysts in MEAs and stacks operating at high current densities and on air and at low PGM loading ($\leq$0.1 mg Pt/cm^2 on cathode).
- The project is well aligned with DOE Hydrogen and Fuel Cell Program (the Program) goals and with the need to achieve high performance at high current density with low Pt loadings and for alloy catalysts. The approach of detailed characterization studies, empirical studies, and modeling should lead to significant improvement in understanding and the ability to design optimized structures.
- Focusing on improving performance at high current densities, the project will impact low-Pt-loaded catalyst layer fabrication that could facilitate cell cost reduction.
- High-performance and durable catalysts are essential.
- While it seems clear that dealloyed catalysts will play an important role in next-generation catalyst materials, it is unclear whether this team's approach has advantages over other approaches (e.g., 3M nanostructured thin film). It would have been useful to have an assessment of the synthesis methodology and its scalability.
- The project's relevance is sustained by the fact that the ANL team is investigating how the ink solutions are affecting the dealloyed catalyst and providing characterization. If the project was just an MEA integration project, there would be some question as to whether this effort was simply relevant to the catalyst being used, or to the processing methods that have been selected. However, the researchers have instead directed the project to a basic level. Decline in high current density performance (and beginning-of-life low performance at high current density) has been a problem for Pt alloy oxygen reduction catalysts. Studies of proton conductivity in the catalyst layer are highly relevant so long as state-of-the-art materials are being used or there is some way to translate the findings to the use of state-of-the-art materials.

- This project has the potential to advance progress toward DOE goals and objectives, if the relationship between the structure of the catalyst layer and mass-transport overpotential will be established.
- This project is looking for only a very limited improvement in cathode catalyst layer performance—just the part of the polarization curve beyond 1.2 A/cm^2 is targeted. Some fundamental issues with the alloy catalyst were identified, such as movement of the Ni in the membrane, which is quite valuable information about the stability of the alloy catalysts.

Question 5: Proposed future work

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

- The future work described is aligned with the proposed work of the project.
- Overall, the future work is well planned.
- The project has a well-thought-out approach, and the future work is appropriate. An increased emphasis on in-cell diagnostics to understand losses is required, and this is planned. An area to consider for high current density effects is the effect of water saturation levels with thin catalyst layers.
- The four major categories of what the project proposes to do could be very simply broken down as the following: (1) studying where dealloyed Ni goes throughout processing, (2) processing parameters versus performance, (3) structural characterization and mapping, and (4) collection of layer transport and kinetic properties. All four categories of elements to study in a catalyst layer appear to be covered: processing, properties, structure, and performance. It is not clear whether, and how, all of these elements of the study will be coordinated with each other. It may be interesting to include some awareness of durability as all the other parts of the study come together. The development of catalyst cracks should also be kept in mind because processing parameters are varied.
- Further work should be done to understand the stability of the alloy catalysts; if Ni does move in the MEA, then this could be a show stopper for this type of alloy. The impact of this Ni movement should be investigated prior to doing more optimization of the catalyst layers.
- Because local O_2 transport resistance is responsible for the high voltage loss at high current densities, future work should include identifying and quantifying the local resistance and developing a method to mitigate it.
- The future work focuses on finding ways to improve interactions between the ionomer and the catalyst surface; the exact path for doing this is not very clear. The role of functionalized carbon is not clear. The effect of Ni leaching will be addressed through the addition of Ni^{2+} into electrode layers, but strategies to mitigate Ni leaching should also be considered. Strategies for addressing mass transport losses are also not very clear.
- The objective of the Monte Carlo model is unclear. This was not discussed at all! Based on the information given, this model serves no useful purpose and likely duplicates activities of project FC-049. It would be much more useful to measure the ionomer layer thickness and stability of the functionalized carbon support than performing impedance measurements to separate mass transport contributions and characteristics of the functionalized carbon. It is unclear how the mass transfer contributions can be separated because only a single loop is generally observed at low frequencies (Nyquist plot). The functionalized carbon characteristics are not useful if the treatment is not stable.
- The motivations for the next steps of this project are unclear:
    - It is not clear how adding Ni ions to the ink will help. Presumably, one would have to add a salt, such as nickel sulfate, but then sulfate ions will also be affecting things. In addition, it is unclear why any of the leached Ni would remain during electrode fabrication.
    - The next three tasks seem predicated on the notion that high current limits are due to poor ionomer/catalyst contact. Working on optimizing the inks is useful, but there are no tasks that will address possibilities for low high-current performance.

Project strengths:

- Organizations involved in the collaboration include those that are very strong in their fuel cell depth (e.g., ANL, GM, and JMFC). ANL seeks to take advantage of recent good work in other projects developing alloy catalysts. ANL seeks to address major questions that could result in considerable benefits if answered correctly. Those questions include: (1) why an alloy catalyst performs worse at high current density, and

(2) how a catalyst layer structure/properties can be influenced by processing parameters in order to achieve higher performance. ANL has already successfully accomplished the modification of catalyst powder with proton conducting groups, which is impressive.
- The project has a well-thought-out approach with good baseline and catalyst systems selected; a range of solvent and ink properties; and good characterization of the interaction effects of the solvent with catalyst agglomerates, Ni dissolution, modeling, in-cell diagnostics, and concepts for improved performance. The linkage of these approaches should produce strong project results.
- The project has exceeded all of its targets in the first year. The team used an excellent catalyst layer design matrix of a large number of catalysts. Very strong modeling efforts complement the very strong characterization component.
- The team is composed of respectable research organizations with adequate expertise. Overall, the team is equipped with the knowledge base, resources, and industry/academia/national laboratory mix that is required for the success of this project.
- The team has access to sophisticated analytical resources; these should be used more heavily in this project.
- The project has a good team covering advanced catalyst materials and fabrication, catalyst layer characterization, and modeling and diagnostics.
- The project features good team composition and expertise. This project addresses an essential topic for successful fuel cell commercialization.
- The technical expertise and collaborations are strengths of this project.

Project weaknesses:

- ANL will have to deal with hazards inherent in any project where materials are being integrated into layers, including using irrelevant solvents and inking parameters; using irrelevant ink application techniques; and generally focusing on processing parameters that may only be relevant to one particular process, or only on a small scale. The project may not have the right characterization methods for mapping the ionomer, the catalyst, and the pores, so as to understand the structure.
- Modeling must be done in better correspondence with characterization and testing in terms of parameters used. The very important effect of morphology is overlooked. There is no clear strategy for addressing the poor interactions between the ionomer and the catalyst. There is a lack of durability results.
- The project should have started with a well-designed durable catalyst, such as one developed at the end of the GM project. It takes too much effort to develop a working catalyst that is unlikely to be employed in practical fuel cells. ANL should not report modeling results that have been known for a long time, such as "mass transport overpotential not being related to mass activity."
- The model cannot be evaluated, because details are not given. The model duplicates efforts of another project to create a catalyst layer model. The source of the mass transport loss is sufficiently clear. Additional mitigation strategies would be more beneficial to reduce risks.
- Early in-cell testing was impacted by poor cell compression.
- The project will have a very limited impact with the current target and approach.
- Within the defined scope of work, the project does not seem to have a weakness.

Recommendations for additions/deletions to project scope:

- The project team may want to add other successful catalysts developed under the Hydrogen and Fuel Cells Program, including nanoframe catalysts and mesostructured thin-film catalysts. The team may also wish to add characterization that allows catalyst layer mapping under both dry and wet conditions.
- Porosity/morphology control should be part of addressing mass transport losses. Durability studies must be performed.
- The catalyst with so many problems needs to be replaced by a more stable, highly active catalyst. The optimization of hydrophilic and hydrophobic properties of the catalyst layer should be one of the priorities of this project.
- The project team should change the scope to investigate the stability of the alloy catalysts in operating fuel cells. Good progress was made in this area, and it is very significant if the Ni is moving about the MEA as shown in the data.

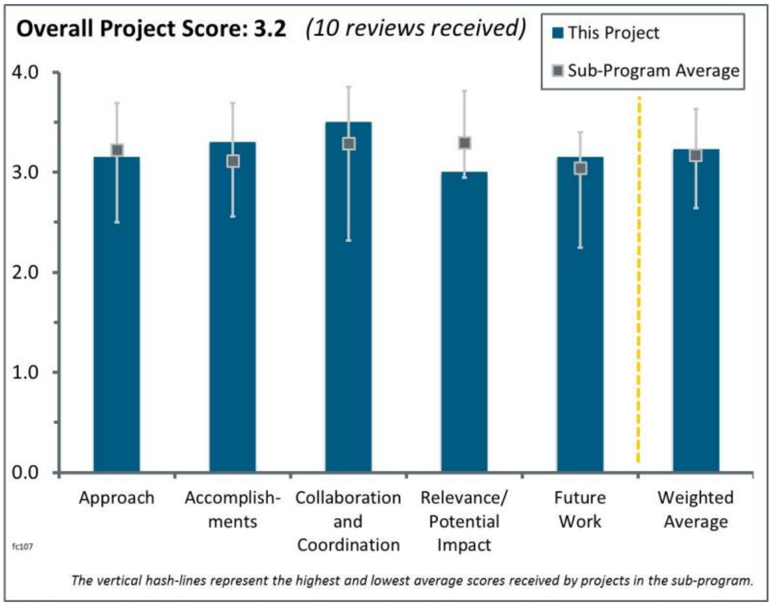
- ANL should eliminate planned modeling activities and integrate the work completed in project FC-049. ANL should eliminate efforts to understand the source of the mass transport loss and increase activities to address it.
- It is necessary to conduct a durability study of the rationally designed catalyst layer even if it is not in the scope of work.
- The team should look at water management effects on the thin catalyst layer and include catalyst layer thickness effects in the study.

Project # FC-107: Non-Precious-Metal Fuel Cell Cathodes: Catalyst Development and Electrode Structure Design

Piotr Zelenay; Los Alamos National Laboratory

Brief Summary of Project:

The objective of this project is to advance non-platinum-group-metal (PGM) cathode technology through the development and implementation of novel materials and concepts for cathode catalysts and electrodes with (1) oxygen reduction reaction (ORR) activity viable for practical fuel cell systems; (2) improved durability; (3) high ionic/electronic conductivity within the catalyst layer; (4) adequate O_2 mass transport; and (5) effective removal of the product water.



Question 1: Approach to performing the work

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

- Non-PGM catalysts have improved very significantly, and the progress this team has made is very impressive. This is probably a direct result of creative science, proper modeling approaches, and good collaborations.
- This project is off to a very good start. The results demonstrate excellent performance for a non-PGM catalyst. The approach for developing these catalysts is well founded and is leading to excellent results.
- The strategy used in this project is excellent, incorporating synthesis, fundamental understanding, and implementation of knowledge in the real system; very few details need to be added for this project to be outstanding. One missing element is the in situ characterization of catalysts and determination of the possible active sites. The principal investigator (PI) should develop an in situ protocol to really understand activity and stability relationships during the ORR.
- The approach for improving catalytic activity of non-PGM catalysts and developing optimized electrode structures is well planned out. The approach includes significant characterization to improve understanding of active sites and electrode structures, as well as associated modeling approaches of transport properties within the electrode structure. The approach to addressing durability is less defined, and it is unclear whether the approaches listed to mitigate degradation are consistent with degradation mechanisms.
- The project team explained the potential problems and difficulty of this technology but did not show an effective approach to solve them. It is good to be sticking to hydrogen/air cell performance rather than just RDE. The reaction mechanism needs to be investigated to develop effective approaches.
- The project is a synergetic effort of non-PGM ORR catalyst development, active site determination, and electrode design and optimization. Several routes toward the design of materials are currently being evaluated. The selection of precursors and synthetic routes at this stage of the project seems quite random. The rationale behind selecting the precursors, number of heat treatments, temperatures, etc. is unclear. It is also unclear how the information from three-dimensional (3-D) structure and surface probe analysis is/will be fed back to the catalyst synthesis. The membrane electrode assembly (MEA) testing—both in-house and with industrial partners—is of great importance.
- Transition metal inclusion is not preferable. Nano-XRT appears to be very useful for determining pore structure and unravelling the performance implications of the microstructure and nanostructure of the synthesized catalysts. This project is highly relevant to the commercialization of automotive fuel cell technology—a demonstration of useful and durable non-PGM catalysts would significantly impact catalyst cost.

- This project purports to address the key barriers of activity, durability, and power density, but not cost specifically. The plan and approach are well thought out but fundamentally limited by the focus on non-PGM materials as a means to meet the activity and power density needed for automotive applications. LANL appears to have already exceeded some of its activity targets, but with total PGM loadings (on the anode) that are still far too high. The types of durability tests indicated to be done in the future may not be extensive enough to really explore the limits of these catalysts.
- The main weakness here is the complete lack of cost analysis. A large portion of the driving force toward non-PGM MEAs is their supposedly lower cost. The cost of a full system (from feedstock, including processing steps and added hardware/system cost) should be compared to that of an equivalent power system with PGM-containing MEAs. This cost analysis has to be included, especially in the light of the PI's assertion that non-PGM catalysis work has outgrown U.S. Department of Energy (DOE) Basic Energy Sciences (BES) funding. Non-metal catalysts are so poor in activity that the system cost to get the equivalent power output as PGM-containing MEAs could more than offset the cheaper catalyst cost. This technology is nowhere near maturity and should be excluded from the Program and perhaps funded by BES. The use of super-high-loaded anodes (2.0 mg Pt/cm^2) is a concern. The crossover of Pt may skew the results. Transport property research as performed is a plus. Durability needs to be proven.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- LANL shows one of the highly active non-PGM catalysts. There has been an excellent analytical effort to understand the active sites and optimize the electrode.
- The project has shown excellent progress to date, achieving results that exceed milestones.
- Excellent fuel cell activities are demonstrated, exceeding the milestones for two sets of catalysts. Excellent air performance is accomplished as well. The achievement of a metal-free catalyst is of doubtful importance and relevance. Morphological information is of high importance to the optimization of performance, and the effort the team has made toward extracting morphological information deserves praise. It is not clear how this information will be used to affect the catalyst and electrode structure design. The strategies of altering the morphology of electrocatalysts and catalyst layers are unclear. With the project finishing in September 2014, the lack of durability work is disappointing.
- LANL has made excellent progress; however, some of the characterization results are surprising. Slide 7 shows single atom Fe species (see inset), but the elemental maps in slide 16 show significant segregation of the Fe. If the precursor contains molecular Fe species, it is unclear what causes the large-scale agglomeration of the Fe seen in the mesoscale structure.
- The team has made good progress in meeting its volumetric performance targets in a short time. However, the results are clouded by the exceptionally high anode Pt loadings (0.2–2 mg/cm^2) under which the cathode non-PGM catalyst was evaluated. The anode loading for non-PGM cathode MEAs should be as low as required for PGM cathodes if automotive applications are being targeted (e.g., 0.05 mg/cm^2). The exceptionally high 2 mg/cm^2 Pt loading used in many of the tested MEAs can affect the apparent cathode performance in multiple ways, especially at high power densities. In response to a question from a reviewer, it appears the researchers have not explored anode loadings below 0.2 mg/cm^2. In PGM MEAs, going to ultra-low anode loadings can have a deleterious effect on performance. This range of anode loadings from 0.03 to 0.2 mg/cm^2 Pt should be explored for its impact on the non-PGM cathodes for more honest comparison to PGM MEAs. This will be even more important once high-voltage cycling durability testing of these materials is carried out. Such start-stop tests will cause Pt from the anode to migrate to the cathode and give false impressions of the cathode durability.
- The team has achieved significant improvements in performance versus the previous state-of-the-art technology. The team has made good progress toward the September 2014 volumetric activity milestone and demonstrated continuous improvement of the catalyst and layer. Performance gaps versus DOE targets remain.
- The project's accomplishments have been primarily qualitative; because it appears that the researchers do not really understand the mechanism, it is hard to pose the next-step problems. However, the materials

developed so far finally seem at a stage where the performance is high enough that detailed mechanism studies are plausible, and the team should be congratulated for that.
- CM-PANI-Fe-C performs well in air. It is necessary to investigate the interaction of Fe with other materials in the MEA, such as membrane degradation.
- Considering that this is a new project, a good amount of progress has been made, especially in characterizing the synthesized materials. Although the PIs presented some nice TEM images, there is not much value in them—these are nice pictures without substance. The ORR is a surface process and the PI should focus on that—rather than on defending the possible active sites—and try to uncover the still-puzzling role of 3d cations, rather than focusing on bulk imaging. The "graphene approach" is good, and hopefully the utilization of Raman spectroscopy will be a big help in these experiments.
- The current density targets have been achieved, but the crossover of Pt needs to be excluded to affirm that this activity is not due to Pt crossed over from the anode. A combination of much lower anode loadings (an order of magnitude lower) and post-mortem microscopy has to be performed to validate that the measured activity is indeed purely due to the non-PGM cathode. Furthermore, the iR-corrected targets are misleading unless it can be shown that the added thickness of the catalyst layer does not add cell resistance (then iR-correction would artificially make the catalyst appear active). Non-metal catalysts perform poorly; it is doubtful that they will reach the go/no-go target (more than 250 mV off in $E_{1/2}$). This research should be excluded from this project. Good progress has been made on porosity imaging and modeling. New targets should be coming soon, as per the presenter's comments. Hopefully the targets will be aligned with what non-PGM cathodes need to achieve to be competitive with PGM MEAs.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- A strong degree of collaboration is evident between the project partners. The selected university and industry partners are performing highly relevant work to further the understanding of catalyst activity and catalyst layer structure property-performance relationships.
- Good collaboration is demonstrated, with a large number of partners, and the results from a number of partners demonstrate good integration between project members.
- Collaboration is good, and partners are well coordinated with the leading PI.
- The partners seem to work together very well to help make the project successful.
- There is good collaboration with industry partners.
- This project has a broad and large collaborator list, covering issues from catalyst synthesis to fundamental characterization to original equipment manufacturer stack testing. It has good project management potential, with a highly experienced PI. The project is quite new, so it is hard to see the degree of collaboration of all the collaborators.
- This project has a very strong team, ranging from academia to national laboratories to industry. The only thing lacking is a material supplier. To be a successful candidate to replace a Pt-based catalyst, the potential limitations of scaling up, as well as the cost projection, must be studied in the early stages of development.
- An excellent team is working on the project with clearly defined tasks. The synergism is not seen yet in reality.
- Team interactions between modelers, experimentalists, and microscopists are very strong, except with the collaborators at Waterloo, who seem to be working independently on the metal-free catalysts.
- While each partner seems to have his/her own clear project within the project, they supplement each other well.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- The development of highly active and durable non-PGM electrocatalysts has become a focus of the Hydrogen and Fuel Cells Program, with several projects funded by DOE. The progress made in recent years by the community is exponential, and the current project builds up from the project team's own expertise in this area and has lots of potential for contributing to continued development.

- The development of non-PGM cathode materials is of paramount importance for the successful implementation of fuel cells in transportation. The challenges are huge, but the seed of knowledge already exists.
- The development of non-PGM catalysts is of great potential impact for lowering the costs of fuel cells.
- The project has the potential to significantly further state-of-the-art non-PGM catalysts and close the gap between Pt-based and non-PGM catalysts. Granted, there are still significant performance gains that must occur for the non-PGM catalyst; however, LANL seeks to understand the entirety of the catalyst synthesis and catalyst layer structure. The understanding of the non-PGM catalyst layer structure is still critical to enabling the high-activity catalyst, but significant progress has been made.
- Progress is certainly being made toward DOE goals, but it remains to be seen whether these materials might rival Pt-based cathode catalysts, which also keep getting better.
- The non-PGM catalyst area is generally relevant if the catalyst can achieve comparable performance with a low-Pt-loading catalyst for both high potential and low potential (high current density area).
- Although good progress has been made, it is still unlikely that the technology will be introduced into automotive applications. The project team should consider non-automotive applications and conduct a feasibility study to determine if there may be an earlier path to a commercial application.
- In this reviewer's opinion, non-PGM catalysts are only going to be a success if their activity ensures that the cost of the entire system is lower than PGM-containing fuel cells. At current estimation (based on the plenary session at the DOE Hydrogen and Fuel Cells Program Annual Merit Review), the catalyst cost is <50% of the system, and is likely to drop lower with development of lower PGM-loaded catalysts. Therefore, this reviewer contends that non-PGM catalysts can only be a success if their cost (including processing steps such as high-temperature pyrolysis) is effectively zero *and* their activity is >50% of that of PGM-loaded MEA's.
- The basic premise of the project—the development of non-PGM cathode catalysts that overcome activity, durability, and power density barriers—seems flawed. It is interesting that the PI did not cite cost as one of the barriers that the non-PGM development project addresses. This is actually good, because the total PGM loading targets of <0.125 mg PGM/cm^2 are sufficiently low that replacing the cathode PGM with a non-PGM would have minimal cost impact. Therefore, the remaining potential impact of the project would have to come in its ability to address the activity and durability barriers, compared to a PGM-based system. It is highly unlikely that non-PGM site densities and site ORR turnover rates will ever approach those of metals. The non-PGM focus on hydrocarbon carbon based materials will not eliminate the carbon corrosion issues at the high voltages that start-stop cycling will produce. Therefore, it is difficult to see what the long-term advantages will be, given the higher risk involved.

Question 5: Proposed future work

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

- The proposed future work is reasonable. More in situ spectroscopies are needed to understand the limits and the potential of these types of materials.
- The team has identified all the relevant barriers and challenges.
- The proposed future work is reasonable and enhances the approaches in this project.
- The plans are appropriate and well thought out. The exception may be in the area of durability, where insufficient justification is provided on a path forward having a reasonable probability of success.
- In addition to the proposed future work, the following tasks are recommended:
    - Proposed Task A: Have system-level modeling, including a fairly precise balance of plant (BOP) especially designed for the non-PGM catalyst.
    - Proposed Task B: Identify some potential limitations/difficulties to scale-up.
    - Proposed Task C: Start the cost modeling based on the process issues identified by Proposed Task B in manufacturing and additional BOP systems identified by Proposed Task A.
- Metal-free non-PGM catalysts are preferred because of potential issues with component and membrane contamination. It will be interesting to see the results of the molecular probe approach.

- Optimization work may shed light on the attainability of non-PGM MEAs; the modeling work seems on track. Cost analysis needs to be added, as well as durability studies.
- LANL has a large amount of proposed work focusing on mechanism studies and modeling. More effort should be spent in the initial period on experimentally determining the performance and durability gaps in working MEAs that have membrane and anode characteristics acceptable for automotive applications. This will better define the scope of the cathode catalyst limitations and produce a more reliable set of data for the modeling and mechanism studies. For example, LANL should take the best-of-class CM-PANI catalyst at this time and experimentally determine how its performance and durability (under the most severe kind of DOE-defined CV cycling in MEAs) depends on the Pt anode loading over the range of 0.2–0.03 mg/cm^2, membrane thickness from 20 µ to 50 µ, under a range of wet to dry conditions and cold to hot temperatures. This will provide key information for modeling and identify the factors that will need the most work. As the catalyst development proceeds to achieve higher activities, catalysts can be inserted into the defined MEA structure as a new best of class and the array of tests can be repeated.
- With the project in an early stage, the proposed future work perhaps encompasses too many goals. The team has to focus on down-selecting a promising catalyst, optimizing its morphology in catalyst layers, and performing extensive durability studies of electrode structures.
- It is not clear that a technique such as Mössbauer spectroscopy, which yields average information from the entire sample, will be useful to discriminate between the three kinds of Fe species present. It would be difficult to infer the characteristics of each of the species. Furthermore, Mössbauer spectroscopy may not be very useful if atomically dispersed Fe species are responsible.

Project strengths:

- LANL made strong technical improvements over the state of the art. It has also made significant progress in understanding non-PGM catalysts and electrode layer fabrication to enable the use of non-PGM catalysts in hydrogen/air PEM fuel cells. Very-high-activity of the non-PGM catalyst was observed even before catalyst layer optimization.
- The work on electrode structure imaging and linking to modeling for transport properties provides a good approach to better understand opportunities for optimization of the electrode. The characterization of the active sites should support essential mechanistic studies.
- The project heavily relies on the previous PI's accomplishments, and this project has the potential to provide many answers to a complex electrochemistry on non-PGM catalysts. It is encouraging that the PIs are focusing on a real problem and are trying to combine fundamental knowledge with real applications.
- This project features an excellent team with clear separation of tasks. It also features a great industrial component, very strong characterization efforts, and excellent achievements in activities.
- Strengths of the project are its great breadth of collaborators and the vast experience of the PI and the PI's institution.
- New PGM-free catalysts with high performance are a great platform to study the ORR in non-PGM contexts, and examination of these materials is being done systematically with a good team.
- The porosity studies are a strength of the project—perhaps providing knowledge transferable to carbon support work. Each partner seems to supplement the others well.
- LANL has established analytical methodology for non-PGM catalysts (materials and performance).
- LANL has excellent in-house expertise to deal with all aspects of catalyst synthesis and testing.

Project weaknesses:

- There is a lack of cost analysis to prove that this technology is competitive to PGM-containing MEAs. If non-PGMs are not financially competitive, then optimization studies are premature and non-PGM research may have to go back to basic research. LANL should reduce the anode loading in the MEA tests by at least an order of magnitude.
- There is no clear path in synthetic routes. Durability must be a priority for candidate electrocatalysts. The purpose of making metal-free catalysts is not clear, unless they are to be used as a reference to study the active site. There is no clear understanding of how morphological 3-D information and other characterization outcomes will be used as input to alter synthetic routes and the choice of precursor.

- The explanation of results still heavily relies on a competition of "my explanation versus your explanation." It is time to demonstrate how stable these materials are and to find a way to quantify the dissolution of active components during the ORR.
- The project's weakness is trying to optimize the catalyst activity without simultaneously gaining reliable capability for fabrication of electrodes and MEAs that use realistic anodes and membranes, and that only partially test performance and durability.
- Longer-term durability tests are needed to demonstrate the utility of the catalysts being developed. Also, the heterogeneous nature of the catalyst may make it difficult to scale up.
- The metal-free catalyst work seems separate from the other activities, and the performance of these materials is far behind the Fe-containing materials.
- The feasibility for automotive applications is unclear. Durability is a major issue that needs to be addressed. Increased mechanistic studies are required.

Recommendations for additions/deletions to project scope:

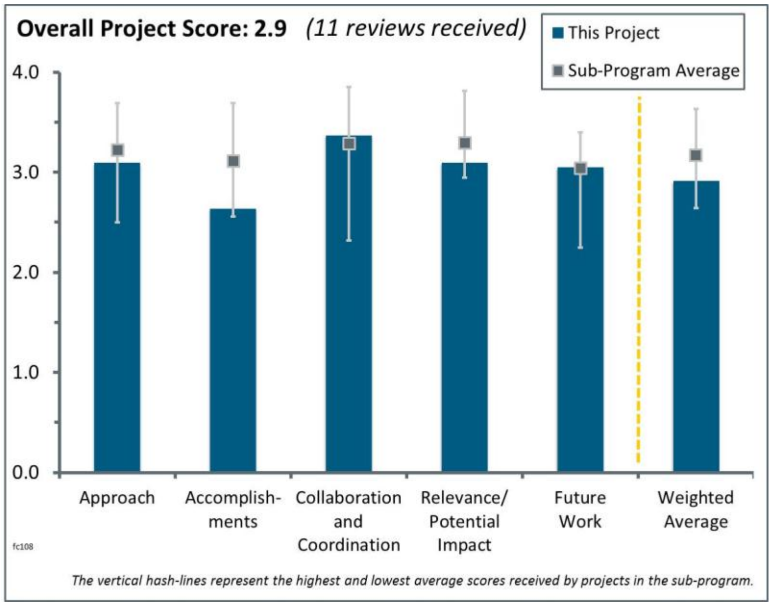
- At periodic points, LANL should put a stake in the ground and define a "best-of-class" MEA with the best cathode non-PGM to that point that uses anode loadings and membranes that meet the PGM required targets. The team should do more extensive fuel cell testing with that best-of-class MEA to understand the performance and durability issues that will have to be addressed for competitive performance with PGM MEAs.
- Non-metal ORR catalysts perform too poorly to be funded in an applied research program. The PI should go back to basic research funding (e.g., BES). A full cost analysis (including processing steps, and compared to a PGM-containing stack of equivalent power output) is a must-have.
- From the application side, a purely non-PGM catalyst is not valued. Reducing the use of expensive materials is more important; therefore, opportunities in which these non-PGM catalyst materials can enhance the ORR activity of low-PGM-loading catalysts are of interest.
- If this technology is targeting the automotive application, the competition is the PGM-based catalyst. Without achieving PGM-level activities and high current density performance in air, it will be difficult to use in a product simply because of space issues.
- The team should eliminate the secondary focus on the metal-free catalysts and instead focus on the mechanisms of oxygen reduction in the PANI-based materials.
- The team should pursue better characterization of the active sites in the working catalyst.
- LANL should address anode MEA loadings.

Project # FC-108: Advanced Ionomers and Membrane Electrode Assemblies for Alkaline Membrane Fuel Cells

Bryan Pivovar; National Renewable Energy Laboratory

Brief Summary of Project:

The overall objective of this project is to develop advanced perfluoro (PF) anion exchange membranes (AEMs) for improved-performance alkaline membrane fuel cells. Goals include: (1) synthesizing novel PF AEMs with high temperature stability and high water permeability; (2) employing novel PF AEM materials in electrodes and as membranes in alkaline membrane fuel cells; and (3) demonstrating high performance, durability, and tolerance to ambient CO_2.



Question 1: Approach to performing the work

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

- This is excellent, groundbreaking work that has a chance to dramatically change the incumbent acid polymer fuel cell industry.
- The project looks to attack the two major problems with AEMs; it is looking at different chemistries to avoid the Hoffman elimination, and increasing water transport. The approaches, in terms of chemistry and fundamentals, are sound.
- The approach for the development of a novel AEM outlined by the project team is focused and addresses the barriers of durability, cost, and performance. The primary focus of the work is on membrane durability.
- The multidirectional approaches taken by the team for the completion of all tasks are adequate. All the analytical techniques had been thought through appropriately. The study on Grignard reagents as a type of “clip on” chemistry amenable to several different well-defined end groups onto PF-sulfonyl fluoride precursor (SFP) side chains is a reasonable strategy.
- This project builds on the principal investigator’s (PI’s) prior and ongoing work, which has U.S. Department of Energy (DOE) Office of Basic Energy Sciences (BES) support, involving new hydroxide-ion conductors. The present project seeks to use knowledge gained from the BES-sponsored work to guide new work making fluoropolymer alkaline ionomer membranes for use in AEM fuel cells (AEMFCs). The project is well designed and addresses the barriers identified by DOE. There are, in fact, only two such barriers in the DOE Fuel Cell Technologies Office’s Multi-Year Research, Development, and Demonstration Plan (MYRDDP), but the project is working toward addressing those barriers and is providing new materials that could help to enable further adoption of alkaline chemistries for fuel cell applications. The approach is well reasoned and sensible.
- The project approach is to address temperature limitations and water permeability with new membranes, which should address the major concerns. The membrane design seeks to separate the electron-withdrawing group from the ammonium group with a benzyl ring, which is good. The perfluorinated design is there for water permeance. The original approach was to use Grignard chemistry, which appears to now be abandoned due to an inability to find a suitable solvent. The project did well to develop sulfonamide linkages in parallel with the higher-risk Grignard route. There is a slight flaw in the approach in that hydroxide conductivity measurements were not yet required within the first year of the project. Instead, the go/no-go decision was allowed to be passed based on IR data on S-F conversion. At this point in the project (especially given that the project is only for two years), hydroxide conductivity data should exist.

- This project is properly focused on facilitating water transport in alkaline membranes. In alkaline fuel cells, the water is produced on the hydrogen side, which does not have a substantial exit flow of inert N_2 to help flush out product water, as there is for the cathode of acid-membrane fuel cells. Because hydrogen recirculation pumps have proven to be difficult to implement, this issue remains quite critical. Claims of alkaline cells being a solution to high noble-metal catalyst costs should be tempered by the fact that the hydrogen evolution reaction is slower on platinum group metals (PGMs) in alkaline than in acid. There is still a catalyst problem; it is just shifted to the anode.
- The project has a sound rationale and scientific approach toward addressing the CO_2 poisoning and water transport issues with alkaline membranes. However, the approach and project timeline should have been much more aggressive to have an impact on reaching the DOE goals for alkaline membranes in just two years.
- The main theme of this project is to use PF polymer as the backbone for AEMs to achieve better transport properties, mainly OH- conductivity and water permeation, and better chemical stability over hydrocarbon-based AEMs. While the PIs are correct that the PF-based electrolyte was proved superior in cation exchange membranes, this does not directly translate into AEMs. In fact, as the PIs pointed out, PF can cause chemical stability issues in AEM due to strong electron-withdrawing PF backbones. It is also debatable whether to expect higher OH- conductivity by using PF backbone in AEMs. Again, the strong electron-withdrawing backbone helps to form stronger acid in proton exchange membranes, but in AEMs, the same PF backbone may cause a weaker base. More direct evidence/data is needed to support the claims of higher conductivity and water transport by using PF backbone in AEMs. Using PF backbone will most likely increase the cost of the AEM itself. Cost/performance benefit analysis is needed at the system level.
- This is almost a basic research project that perhaps should have been funded by BES, except for the drive toward applied goals. The major deficit of the approach is that the membranes are not tested in fuel cells at the National Renewable Energy Laboratory (NREL), and the researchers are relying on a collaborator for all fuel cell performance results. In addition, that collaborator was not responsive as of review time.
- The base for the project is questionable. It is unclear why fluorinated polymer is needed with the hydrocarbon polymer. Amide alkyl has C-H bonds. The whole idea does not make sense.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.6** for its accomplishments and progress.

- The work to date has focused mostly on getting the synthetic chemistry to work well. The researchers have worked hard and had some success, although they also encountered some difficulty. They have demonstrated that coupling to sulfonyl fluoride groups can be achieved, but they do not yet have a good handle on the extent or yield of reactions for the Grignard coupling, the sulfonamide coupling, or the quaternization reactions on sulfonyl fluoride groups in polymers. They have said that future work will include a more quantitative focus on the extent of the reaction; for example, using titration or some other analytical measurement to address how much of the sulfonyl fluoride groups have been converted into sulfoxides/sulfonamides, and ultimately into fixed cationic sites. This focus is good. Studies of conductivity and water uptake may be premature on materials of unknown ion-exchange capacity, so the team should make such studies a high priority in the second year of the project. The team should also start working with hydroxide-exchanged materials as soon as possible. Hydroxide-form membranes have presumably been made, because that is what CellEra will be testing, so the researchers should work on them, too. Studies on chloride/iodide/bromide/PF_6 membranes are fine for a start, but they will not reveal much of what needs to be known to evaluate potential utility in AEMFC technology. Decisions about alkaline stability for ionomer molecular fragments are so far being made just from density functional theory (DFT) calculations; it will be good for the team to start generating experimental results in this area.
- The researchers have successfully demonstrated the Grignard chemistry synthetic route. They have also developed a novel processing scheme to allow Grignard chemistry on PF-SFP. They have successfully used modeling to understand the limitations of Grignard chemistry. They need to find an alternative way to prepare PF-based AEMs.

- Good progress has been made, but many significant challenges remain. Conductivity is still an order of magnitude lower than polymer electrolyte membrane fuel cells (PEMFCs). This looks to be a high-risk project.
- The project team has met its initial goals and shown progress in developing AEMs that can be fabricated into membrane electrode assemblies (MEAs). The conductivities look promising. This reviewer did expect to see OH- conductivities reported at the time of the review.
- Although the idea has flaws, the NREL team made good efforts for the research. The value of the project for commercialization is unclear.
- Two PF AEM chemistries were synthesized. Key characterization data such as OH conductivity and water transport properties has not been obtained, yet the project timeline has passed 50%. It raises a concern about whether the final objectives can be achieved by April 2015. The membrane obtained so far suffers from poor mechanical properties.
- Because this is only a two-year project and the project is 50% complete, it is troubling that the hydroxide ion conductivities of the films have not yet been measured. The MYRDDP has a goal of demonstrating an AEM that retains 99% of original ion exchange capacity for 1,000 hours in hydroxide form at >80°C for the second quarter of fiscal year 2013. The project is far from that goal and has not yet demonstrated hydroxide ion conductivity, let alone durability. Given that it is only a two-year project, the go/no-go decision criteria should have been much more aggressive if progress toward the DOE goals is to be demonstrated.
- So far, the new membrane materials are having numerous difficulties—they are brittle and not yet suitable for hydroxyl ion conductivity testing. There could be issues with water contamination and sulfonyl groups causing crosslinking (and therefore brittleness). The project has less than one year to go and there are still difficulties getting the chemistry to work. It is fair to say that within the next year, it is unlikely that the project will contribute to overcoming the identified barriers. There has been some good work from a polymer synthesis perspective. Materials were made from sulfonamide linkages and some conductivity measurements were made, albeit for halogen anions. DFT modeling has been used to quickly identify alternative synthesis pathways, which has been a highlight. The main issue is the compressed time frame for this project. Investigators are attempting to do about four years' worth of work in two.
- A year into the project, one would expect the researchers to be synthesizing robust films, especially with the competency of the partners. Particularly disappointing is the lack of characterization capabilities at this point in the project. Using other anions as a proxy is OK; however, eventually these characterizations have to be done in the -OH form. The group should have learned how to solve the CO_2 problem by now with commercially available membranes and standardized its testing.
- Membranes have been made in significant quantities, but with only limited testing. More information on the thermal melt process would have been helpful. It seems like that process will affect the performance. DFT seems to be better at explaining observables than giving predictions. Not enough information was given to determine whether the DFT predictions were correct.
- The Grignard-reagent synthetic approach was given a good try and then dropped in favor of using more conventional chemistry to add the innovative spacer sections between the fluorosulfonate and the amine functionality. Conductivity testing of the OH- form of the membrane materials is central to this project and should have been worked out by now—carbonate issues need to be properly dealt with or this project is not doing valuable work.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- The team consists of a good mix of national laboratory and industrial partners. The collaboration with CellEra and 3M is very advantageous to the team.
- The collaborations between the Pivovar group and other institutions are well outlined. Working with CellEra will be of particular benefit because testing the novel membrane in a fuel cell system will best address durability.
- CellEra's insights into the remaining technical problems with alkaline membrane fuel cells have provided an unusually strong level of appropriate focus for this alkaline membrane project that is often lacking in

other such projects. The project appears to be making good use of some of 3M's insights into fluorocarbon membrane chemistry.

- The team is extremely well qualified and the best imaginable to carry out this work. Both a commercial manufacturer and an end user will keep the focus on processability and usability.
- This project features an excellent team that has world-class capabilities in this area.
- This project features good collaboration and a good team.
- Contributions are evident from both NREL and the Colorado School of Mines (CSM). NREL worked on the Grignard route and DFT efforts, while CSM provided other syntheses and conductivity measurements. The project has done well to align itself with both a membrane producer (3M) and an AEM stack original equipment manufacturer (OEM) (CellEra). In the world of AEMs, there are very few players, and this is about as good as it gets. It will be interesting in the future to see more explicitly how 3M is able to contribute toward improving the membrane synthesis routes.
- The collaboration with 3M is important as a source of sulfonyl fluoride polymer materials and should be beneficial in sorting out how to do the reaction chemistry on sulfonyl fluoride polymers. It is not clear what 3M has done so far to help, although this may be due to 3M not being onboard as a partner until relatively recently. The collaboration with CellEra will be very valuable, given its experience with AEM technology.
- The collaborators are clearly experts in their fields and appropriate. Because Los Alamos National Laboratory (LANL) has a long history in this area, which was mentioned in the 2013 presentation for this project, incorporating LANL activities and expertise in the future may speed up progress in materials chemistry and development. It is unclear from the presentation what contributions to materials synthesis and characterization are being made by NREL, and what are being made by CSM. The only clear contribution of NREL is the DFT modeling. The PI should indicate on each slide, where appropriate, the material and information coming from each organization. The roles of the participants in the project are unclear. In the 2013 presentation, CSM's role was listed as AEM MEAs, electrocatalysis, and cell/system testing. However, under the Work Plan, CSM is not listed under MEA testing, but NREL, 3M, and CellEra are.
- The CellEra results are not yet in. No schedule has been given for CellEra to provide results.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The research being led by the Pivovar group is crucial to the development of AEM technology. AEM technology may surpass polymer electrolyte membrane (PEM) technology in performance and cost, and thus may have a great impact on DOE Hydrogen and Fuel Cells Program goals.
- The project is relevant to the objectives of the MYRDDP. All activities are aligned with DOE's goal. The focus of the project is alkaline membrane development as an enabling aspect of nonprecious catalysis in fuel cells without the concerns of liquid alkaline electrolytes.
- While the future of alkaline membrane fuel cells remains doubtful, this project is focused in determining whether the water transport properties of alkaline membranes can be improved to the point that the disadvantages of water generation on the anode can be tolerated. Office of Energy Efficiency and Renewable Energy funding for this project thus makes some sense, even in light of the significant Advanced Research Projects Agency – Energy (ARPA-E) money that is being expended (with less expert guidance) in this high-risk area. Even if this membrane project meets all its goals, significant additional catalyst work (particularly on the anode) and electrode and cell engineering will need to be carried out before claims of enabling significant reductions of PGM costs in practical fuel cells can be justified.
- There is still a long way to go to demonstrate AEM feasibility. By attacking the key issues with AEMs, this project is of great value to the research community. Successful completion will most certainly guide future research, e.g., on whether perfluorinated backbones should be pursued and what functionalities are suitable for performance and durability.
- Alkaline membrane fuel cell development has been identified in the MYRDDP as an important activity toward reducing the cost of fuel cell systems by enabling the use of non-precious metal electrocatalysts. It may be appropriate at this early stage for DOE, through its Systems Analysis sub-program activities, to

evaluate the feasibility and cost of an alkaline system, because the system considerations are vastly different than for PEMFC, and any materials advantages of alkaline may be negated by system complexity and cost. The project is tackling the relevant issues for developing a feasible alkaline fuel cell, but the progress needs to be accelerated to have an impact on meeting DOE goals.

- This is a high-risk, high-gain project. If this project were to provide a significantly improved AEM that meets the goals of the current MYRDDP, it could enable significant and potentially very important improvements in cost and probably other areas for alkaline fuel cell systems by enabling the use of non-PGM catalysts. It is premature to tell if this end result is likely, but it is surely a goal worth pursuing.
- This is a high-risk game changer approach, so the near-term relevance and potential impact will be low to none on the incumbent PEMFC systems.
- A new AEM is important—it is too early to tell if this is the right approach.
- The project relevance is fairly low because this is a moonshot effort to remove precious metal catalysts from low-temperature, solid-state electrolyte fuel cells. If there were surer routes toward doing this and the development of just one component were the only issue (as is the case for non-PGM O_2 reduction catalysts), the relevance would be higher. As it is, because of the freeze issues entailed in having a stoichiometric feed to the cathode, because of the historic low durability of AEMs, and because of the fairly low power densities associated with AEMFCs, the project has an uphill battle on a variety of fronts. The relevance may still be acceptable for a few LT stationary/portable applications (e.g., backup power, forklifts), but the relevance is low for automotive applications because of the need for water feed on the cathode.

Question 5: Proposed future work

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

- The proposed future work is sensible and well chosen. A focus on the extent of the reaction, including careful measurements of ion exchange capacity, is important at this stage and should have high priority.
- The future work described is aligned with the overall proposed work direction of the project.
- The future plans are well focused and very ambitious. It is unclear whether the remaining synthetic pathway (after the others have been abandoned) has the flexibility needed to realize the project's goal of testing the addition of spaces to improve performance and durability. The intended fuel cell testing will probably require catalyst and cell development in addition to the membrane work described in the presentation.
- The list shown for future work shows that the investigators know what they need to do. Unfortunately, the workload is tremendous for just one year. It would be acceptable to not do fuel cell testing if it means that more time could be spent getting a more durable, higher-conductivity membrane chemistry. Hopefully water permeability measurements will also be included in the future work, especially if time does not permit fuel cell testing.
- The project has an effective approach to the future work. Similar to catalyst development in PEM research, the key feature is the integration of advanced MEAs into short stacks for testing. Durability would best be assessed in the short stack testing.
- In its future work, the team should include characterization of the water transport property, and benchmark mechanical properties against the state-of-the-art AEMs. The chemical stability of PF AEM should be benchmarked against the state-of-the-art AEMs. Cost/performance benefit analysis is also needed.
- The most important focus needs to be generating viable membranes; a support could greatly help the researchers' issues with mechanical properties. Characterization in the -OH form should be addressed in short order.
- The researchers are focusing on improved fabrication, but they are not clear about how the fabrication will be improved. It seems like NREL needs an in-house evaluation effort instead of waiting for CellEra to look at the AEMs and then give feedback.
- The proposed future work is appropriate, but there was little in the way of details on how the future goals would be met, especially in terms of creating dispersions of the ionomers to incorporate into the electrodes.
- Scale-up of any of the chemistries may be a bit premature. The team should do more work on the fundamental materials needed to achieve better conductivity.
- The basic idea has flaws.

Project strengths:

- The project has a good concept that entails separation of electron-withdrawing groups from the anion conductive groups. It also has a good collection of collaborators, including a membrane producer and a company trying to commercialize AEMFCs. Another strength is the use of DFT modeling to direct alternative synthesis pathways. The investigators are experienced with the issues associated with AEMs—these are not people who just heard about this technology last week.
- The team has the experience and will to focus on the critical issues in the development of alkaline membranes for practical fuel cells. A successful down-select of preparative chemistries has been made. This project’s plans make more sense than those of other projects in the alkaline membrane area, which is receiving perhaps a bit too much hype in this era of ARPA-E and other programs in which novelty trumps careful reasoning about what realistically can be considered likely to work in practical applications.
- The team is composed of respectable research organizations with adequate expertise. Overall, the team is equipped with the knowledge base, resources, and industry/academia/national laboratory mix that is required for the success of this project.
- The PI proposes a sensible and well-reasoned approach to realizing the substantial cost gains that could be realized if alkaline systems could be used with non-precious-metal catalysts. His approach is based on solid foundational work with fluoropolymer systems and on computer modeling of alkali stability.
- The strength of the team and approach is very good. The approach is fundamental and attacking the right questions.
- The project features a diverse team of experts covering all aspects of chemistry, dispersions, film formation, characterization, MEA fabrication, and testing.
- Strengths of the project are the enthusiasm toward basic research on AEMs and the fundamental reaction chemistry.
- Strengths of this project are its strong team and high-risk approach.
- The approach involves experimentation and modeling guidance.
- The project team is very strong.

Project weaknesses:

- The claim that using PF in AEM can lead to improved conductivity, water transport, and chemical stability needs more direct supporting evidence. The progress is a bit slow; the project could use better planning so that the final objective could be achieved. In the future work, the researchers should include characterization of water transport property, mechanical property, and chemical stability benchmarking against the state-of-the-art AEMs. Cost/performance benefit analysis is also needed.
- “Relevance” statements oversimplify the overall catalytic gains associated with using alkaline-based membrane chemistries. A more detailed description of the need for better water transport in alkaline-membrane systems would have improved the presentation. The project team should not have gotten this far into the project without being able to do reliable conductivity testing of the OH- forms of membranes—halide ions are likely not adequate analogs, at least at higher humidification levels.
- NREL focused on very limited fabrication of AEMs and delving into basic research of polymer synthesis. No progress was made toward evaluating AEMs in an actual fuel cell. The project is not set up so that the researchers can evaluate their products effectively.
- The technical risks associated with the approach are better suited for a project with a four-year duration, not two years. The project has not yet produced hydroxyl anion conductivity data. The chemistries selected have had mechanical difficulties.
- Weaknesses include the results to date, inability to form robust films, and lack of characterization in the alkaline form. Thermal stability (using thermal gravimetric analysis) is of little, if any, value, especially with a different counter-ion.
- Weaknesses include the limited duration of the project and the lack of sufficient funding/activity to accomplish project goals and make an impact on the Program in this challenging area.
- Membrane properties (conductivity) have yet to be assessed in a hydroxide environment. Issues with carbonation can be mitigated with conventional test fixtures.
- The team is having some trouble doing reaction chemistry on polymers—the researchers will have to work hard to overcome these issues.
- The high-risk approach is an area of weakness.

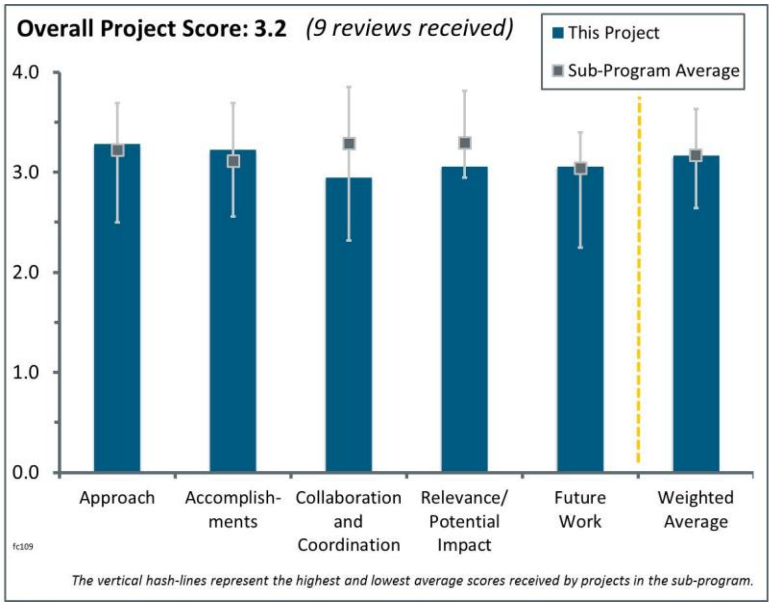
Recommendations for additions/deletions to project scope:

- The PI indicated that in ongoing work he will include a strong focus on the extent/yield of reactions, and on side reactions. This is good.
- There may be some utility in dropping fuel cell measurements from the scope of the project if the chemistry remains challenging. Conductivity, stability (ex situ), and water permeation data can all be prioritized if fuel cell data is not obtained. The water permeability data may be particularly interesting to automotive OEMs that would want to model how an AEMFC would freeze start for a given system operation and cell design.
- In the future work, the researchers should include characterization of the water transport properties, the mechanical properties, and chemical stability benchmarking against the state-of-the-art AEMs. Cost/performance benefit analysis is also needed.
- The researchers should continue to work on the fundamental materials and the synthesis chemistries. Conductivity is still an order of magnitude lower than for acid PEMFC membranes.
- The team should initiate AEM testing in a hydroxide environment. It would be good to understand membranes' tolerance to elevated alkaline exposure pH levels (10–14).
- The project should explain how it plans to address the high-current-density performance of catalyst layers, particularly the anode. It might be wise to include an anode catalyst development aspect in the project.
- The researchers need to get these AEMs tested.

Project # FC-109: New Fuel Cell Membranes with Improved Durability and Performance

Mike Yandrasits; 3M

Brief Summary of Project:



The overall goal of this project is to develop new proton exchange membranes—based on multi-acid side chain (MASC) ionomers and reinforced with electrospun nanofiber structures—that contain additives to enhance chemical stability. The membranes should have improved mechanical properties, low area specific resistance, and excellent chemical stability compared to current state-of-the-art membranes. Membrane electrode assemblies (MEAs) will be evaluated in single fuel cells and fuel cell stacks.

Question 1: Approach to performing the work

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

- The approach to develop MASC ionomers, reinforced with electrospun nanofibers and additives, is excellent because it will allow better ionic conductivity at low relative humidity in polymer electrolyte membrane fuel cells (PEMFCs). The side chains are not likely to cause any additional durability issues. Electrospinning is a key skill of Peter Pintauro and may help strengthen the membrane mechanically. It is unclear if electrospinning is scalable.
- The team is pursuing an extension of earlier work, also supported by the U.S. Department of Energy (DOE), on fluorocarbon sulfonimide ionomers, to include ionomers having more than one acid group in each side chain. The strategy seeks to prepare high-conductivity materials that also have good mechanical/dimensional properties, so that all DOE metrics related to membrane properties can be met in a single material. The researchers' approach is well grounded in prior work, quite feasible, and well integrated with other work. They are following a very logical and well-reasoned approach to achieving milestones.
- The project is focused on meeting all the membrane targets simultaneously. The systematic approach to look at varying the type of acid group and number of acid groups on the side chain is commendable. The combination of the support work and the ionomer work should lead to much improved performance and durability. The inclusion of General Motors (GM) for membrane testing ensures that the tests are automotive relevant.
- The project is well laid out and logical. The project is sharply focused on the critical barriers of durability and membrane performance at low RH. One note is that the goals are listed only to 80°C, whereas the Fuel Cell sub-program goals are for up to 120°C operation. Testing was shown at up to 115°C. Although it is stated that cost will be met simultaneously and is being considered, no approach has been provided to meet this.
- The investigators are using incremental changes in chemistry and supports to reach the project goals. They are very close to attaining the goals. Additionally, a thorough look at the sulfonamide functional group, and the effect of having multiple groups on side chains when there is a direct comparison for traditional perfluorosulfonic acids (PFSAs), is very relevant to the community.
- Overall, the approach is good. The development of membranes with new ionomers that have additional functionality for low-RH operation could represent a significant development. Similarly, novel supports could also afford better trades of mechanical properties with resistance. The researchers have a good early

focus on durability testing. The partnership strategy is sound. The approach should include information on the costs of these elements. It is unclear how the costs for both perfluoro imide acid (PFIA) and these various fiber supports compare with unsupported membranes. Cost should be applied as a screening criterion for these approaches to a greater degree than is evident. Although the nanofiber support work may represent an interesting combination, it is not clear why more traditional supporting materials, such as expanded polytetrafluoroethylene (ePTFE), would not be attempted, at least initially. It may be easier to understand the mechanical properties using known versus novel supports. Also, more characterization of the lambda as f(RH) and alpha parameters would be helpful to understand how these ionomers, when embodied in membranes, hold and transmit water. Also, gas crossover measurements should be conducted to inform how the minimum thickness may be affected. Perhaps this was done on the previous award.

- MASCs provide one of the most promising ways to further improve acid fuel cell membranes, which are already good enough for mass-produced applications. These materials were initially developed under a previous project but could benefit from significant additional attention, including the synthesis of further-improved ionomers. It is less clear whether electrospinning will prove to be a useful and practical approach to either membrane reinforcement or casting of the ionomer. A verbal statement that electrospun fibers should not significantly increase costs was not backed up in the presentation by any details that might show that the electrospinning process is manufacturable and cheap.
- This project is working on developing multi-acid chain ionomers for membranes and/or catalyst layers. The project is also developing mechanical support technology (e.g., for composite membranes). The mechanical support materials are based on electrospun nanofibers. The multi-acid incorporation is a side-chain extension from traditional PFSA, incorporating additional SO linkages to improve conductivity. After material development, there is ex situ testing and single cell testing, but limited characterization.
- The project focuses on making membranes and testing their electrical and mechanical properties. The testing of the polymers occurs very late in the project, giving no time to change course if they do not work or break down in the presence of Pt.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The project is at an early stage, and good progress has been made to evaluate approaches and determine feasibility. The PFIA membrane (unsupported) shows only marginal performance improvement over a 725 equivalent weight (EW) membrane (unsupported). However, the area specific resistance (ASR) shows a clear separation for both supported and unsupported, which provides evidence of a feasible path forward to meeting goals. A number of nanofiber samples have been fabricated on both the laboratory and pilot/production line, with objectives to study various parameters. Good progress has been shown on fabricating the fiber mats, with a number of required parameters identified. There has been good early testing on showing durability; however, it is not clear how much additive is used, and if this is consistent with good performance (e.g., the conductivity tests are likely without the additive, whereas the open circuit voltage [OCV] test is with the additive). Also, a fairly high EW was used for the durability tests, compared to the other testing.
- The work for the present project is just starting to progress, so it is difficult to gauge; however, the researchers have met the milestones they were supposed to have met as of this date and are on track to meet others. A major intellectual advancement for the project is the use of ionomers having more than two acids per side chain. The researchers are not yet reporting on the results from such materials—so far those are just proposed materials. It will be good to hear how well those materials are able to help the researchers meet the membrane milestones all in one material.
- The project has been underway for less than a year. Good progress has been made in screening nanofiber support materials and determining the effect of support on conductivity. The researchers have achieved <5% swell in the down-web direction with 12% fiber content—if this can be met in both directions, the conductivity decrease due to the reinforcement will be greatly diminished. Vanderbilt has achieved electrospinning of PFIA, obtaining identical conductivity for ionomer membrane from electrospun fibers as from solution cast membrane.

- The project has demonstrated considerable progress toward its milestones. The researchers demonstrated that PFIA does not have the EW trade-offs of PFSA and shows potential to hit DOE ASR targets when supported. The supported blend of PFIA and PFSA shows good durability. Results of the durability testing show that the unsupported PFIA meets performance targets but not durability, and vice versa for supported. With the project being so early, it seems that this trade-off will be reconciled. There are questions about the mechanical characterization of the electrospun PFIA. It is unclear how this material serves as a support. If it serves well, it could significantly address the mechanical/conductivity trade-off. Also, the project team should compare supported 3M materials to other commercial supported membranes, such as DuPont XL. Blister strength comparisons to unsupported membranes do not provide useful insights.
- Milestones have been accomplished, showing good progress. Blister strength shows the advantages of the novel membranes as tested using the GM procedures.
- Though it is relatively early in the project, the investigators are very close to the performance targets for the three-year project.
- The project team has made good progress toward scale-up.
- The researchers have demonstrated conductivity of 0.1 S/cm^2 at 80°C and 50% RH; these ionomers are primarily side-chain extensions of more traditional Nafion®. They have made measurements related to meeting the conductivity targets at 80°C 50% RH for PFSA membranes at a thickness of 15.4 microns versus 23 micron thick PFIA membranes, thus showing that they can minimize the thickness of the membrane, which could potentially reduce material costs, but it appears the chemistry is more expensive (8.2 versus 11 micron with reinforcement). The membrane thickness with reinforcement to meet conductivity targets is rather thin; hydrogen crossover numbers should be presented.
    - Made PFIA; measured conductivity and examined water solubility.
    - The nanofiber fabrication by Vanderbilt seems to be progressing well.
    - Looking for a support that provides x-y swelling of <5% after boiling in H_2O.
    - Need 25% fiber content to achieve <5% swelling.
    - Calculating skin layer membrane conductivity separate from the conductivity in the composite layer by making different thicknesses of the skin layer.
    - Completion of RH cycling and OCV testing with PFIA and 825 EW PFSA with a new nanofiber—this shows ~1,500 hours of acceptable performance on the OCV test (with radical scavengers).
    - Blister test conducted at GM.
- Because this project started less than a year ago, it is difficult to give a meaningful grade on its accomplishments. The synthesis of multiple batches of PFIA and the recent preparation of samples with several amide centers on the side chain constitute a good start. It appears from slide 13 that the electrospun materials are still limited to “test patches” of an undisclosed size. To be fully convincing, the electrospun materials will also have to be demonstrated as roll goods. “Welding” times of 10–60 minutes are far too long for a practical process. The reviewer wonders if this could be sped up several orders of magnitude by exposure to vapors at higher temperatures.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The project features collaborations with GM and Vanderbilt. GM is primarily responsible for ex situ, MEA, and stack testing; it has conducted the blister test to date. Vanderbilt collaboration includes the development of nanospun fibers.
- The collaboration with Vanderbilt is well underway and is already helping the team meet the conductivity and dimensional stability targets simultaneously. The collaboration with GM is longstanding and will presumably become more important as cell testing is undertaken. The direct interaction of this project with other projects is less obvious, but previous projects of a similar nature from 3M have involved many interactions with a variety of partners, so it would be expected that the present project would do the same.
- There is obviously a strong team, with a major manufacturer and an end user. GM screening tools should help the project considerably.
- Good collaborations have been identified. GM is an engaged partner.
- The collaboration with GM shows some usefulness in evaluating mechanical properties.

- There appears to be good collaboration so far between Vanderbilt, 3M, and GM.
- The collaboration should become effective once MEA and stack testing starts at GM.
- In general, it seems that the Vanderbilt support materials have not been integrated to the same degree as the 3M support materials. While this is understandable, more focus should be applied to get the Vanderbilt materials qualified in parallel.
- The role of Vanderbilt is not clear. GM is not participating yet. It is not clear how much preplanning the researchers are doing with GM.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The project is highly relevant and will have a significant impact. Successful completion would lead to a membrane that allows operation at higher temperature and under drier conditions without sacrificing durability, which would allow reduced systems cost.
- This team is the most likely to meet, or come close to meeting, the fairly aggressive goals originally laid out for the High Temperature Membranes working group. The team has taken to heart the need to meet all milestones simultaneously with one material, and its approach is the most likely to achieve the goal of high conductivity under hot and dry conditions, while still retaining dimensional stability. Achievement of this goal could have a large impact on many goals in the DOE Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan.
- A new, durable, inexpensive membrane is highly desirable.
- The potential impact is significant, particularly if the project can identify ways to solve the performance/durability trade-off, which it seems like it will. However, with no information on cost, it is difficult to understand how scalable and relevant this technology can be at the product scale.
- If successful, the project will significantly impact the ability to operate at low RH, thus supporting reduced cost. However, a critical assumption is that the electrode will not be performance limiting at low RH. There is no mention of work to address the catalyst layer conductivity for MEA integration.
- Acid membrane properties and costs are already good enough for mass production of fuel cells, but improvements would always be welcome. This project has a fair chance of success but would provide, at best, modest improvements in production fuel cell systems. Manufacturability would have to be demonstrated for the electrospun materials. At the very least, a calculation of how many spinnerettes would be needed to produce fibers for a given line rate of X m^2/hour of MEA should be given, and line speeds of mat formation and other post-processing steps should be estimated.
- Currently available membranes meet most of the requirements for PEMFCs and although useful, the development of a novel membrane is not as critical as a novel cathode catalyst.
- The project is just a marginal modification from previous work.
- The work is to achieve more durable low-cost membranes with structural reinforcement.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The proposed future work will answer some of the key questions (e.g., the effect of chain extension, how many acid groups is best, and the best method for fiber reinforcement).
- The proposed future work plan is sensible and likely to achieve the project goals.
- The project is well planned out and shows a good path to meeting targets of up to 80°C. However, no path is shown to meeting 120°C operation, and no path is shown on cost. There are alternate pathways on the polymer structure, but the overall approach is relatively narrow. If these structures do not work, there are no additional alternatives.
- The future work is logical and, if it includes the feedback provided, should be able to successfully address the performance/durability trade-offs. The project should explore ways to accelerate materials to larger-scale/stack testing earlier in the project, even if the metrics for those materials are not perfect. Doing this

can help identify and focus scaling constraints that can help focus the down-selection of prime-path materials.
- The proposed future work seems reasonable, although it could be accelerated to be completed in a shorter time by 2015.
- The future work area really needs a contribution in terms of cost modeling for both the PFIA material synthesis and electrospinning of fibers. It is unclear whether these materials will lower the requirements for RH and allow operation at higher temperatures. Measurements along these lines should be made.
- The researchers are about 1/6 through the project and about 90% through the goals; it brings into question whether the funding level was appropriate. Cost analysis would be helpful. It is not clear how the researchers plan to address excess in-plane swelling.
- More attention needs to be paid to the manufacturability of electrospun fibers and their processing into complete MEAs. Testing of newly developed ionomers in more conventional supported and unsupported membranes should be continued.
- The project team is doing a lot of scale up without rigorous MEA analysis.

Project strengths:

- The project features novel concepts for achieving DOE membrane targets. It has a rich design space for ionomer and support materials. The team has made excellent early progress.
- A strength of the project is the path toward a new polymer electrolyte membrane (PEM). 3M is highly competent at membranes and scale-up.
- A strength of this project is its interesting ionomer chemistry. Another strength is its possible ability to tune membrane structure through manipulation of electrospun fibers and the mats made from them.
- The project has an excellent approach; the project lead organization is highly experienced and well positioned to pursue the project’s work.
- The project features a strong technical lead and strong collaborators, as well as a clear approach and reasonable probability of success, given the early stage in the project.
- The approach is excellent and addresses three aspects of membranes that could benefit PEMFCs, including conductivity at low RH, mechanical durability, and chemical stability with additives.
- The team members and their expertise are a strength of this project.
- The dual proton conduction site can likely lead to improved performance.

Project weaknesses:

- It is hard to imagine that these PFIA materials are a lower-cost material compared with traditional Nafion, because the chemistry is more complex. The performance may be improved because of the dual proton conducting sites; however, the durability may be less. The researchers should provide some type of cost analysis to evaluate this. It would also be nice to understand the chemical stability of these longer side chains. The electrospun fibers for adding mechanical stability could potentially be a more expensive process than making ePTFE, and the durability may be less because these may be prone to chemical attack. The raw material cost is likely lower.
- Comparison of supported materials to other, commercially available supported materials is an area of weakness. Another area of weakness is the lack of modeling of materials and capital costs to DOE production levels to understand the economic viability of these approaches.
- Testing the new PEMs in fuel cells might come too late to find problems. S and N in polymers might break down over time and poison the Pt catalysts, so durability testing would be key.
- The team paid insufficient attention to manufacturability issues for electrospun materials (at least in the presentation).
- The project is still at an early stage; no results are forthcoming on ionomers with more than one acid in the side chain. It will be good to see results on such materials.
- Cost is not addressed. The early durability testing shown may not be relevant due to the higher EWs used.
- The project may not be scalable, especially the electrospinning.

Recommendations for additions/deletions to project scope:

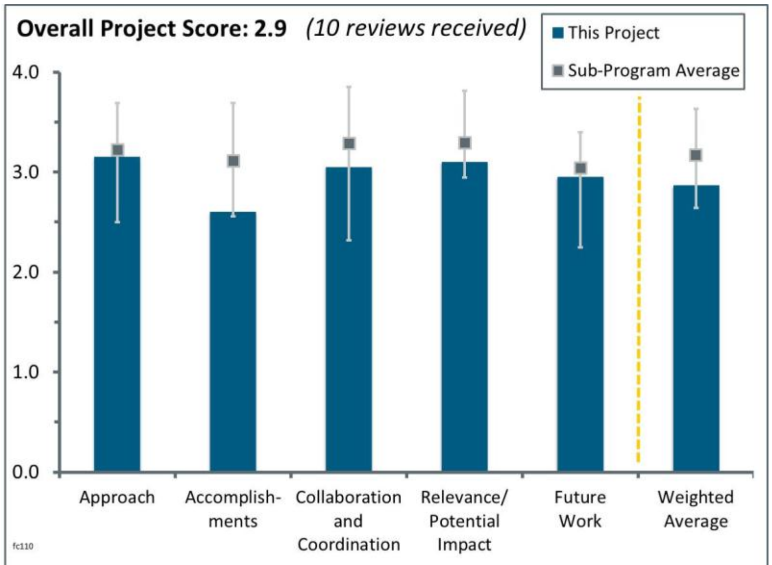
- The project team should make sure that enough testing is done and reported for new ionomers in unsupported membranes or those with conventional supports, as well as for electrospun materials. It should also add robustness (e.g., start-stop, flooded, and dry conditions) to the performance and durability tests planned for the new membranes.
- Both the PFIA and electrospun fibers should be examined in terms of cost and whether there will be a cost improvement. Because the PFIA material is similar to PFSA, there is likely to be an improvement in performance, but not as likely to be an improvement in cost.
- The team should determine the durability of lower-EW membranes—these seem to be necessary for performance.
- The researchers should test the new PEM in MEAs as soon as possible.

Project # FC-110: Advanced Hybrid Membranes for Next-Generation Polymer Electrolyte Membrane Fuel Cell Automotive Applications

Andrew Herring; Colorado School of Mines

Brief Summary of Project:

The overall objective of this project is to demonstrate a low-cost hybrid inorganic/polymer from super-acidic inorganic functionalized monomers with area specific resistance (ASR) less than 0.02 Ω cm^2 at the operating temperature of an automotive fuel cell stack (95º–120ºC) at low inlet relative humidity (RH). The project will also develop a 50 cm^2 membrane electrode assembly (MEA) with desired mechanical properties and durability. The objective in 2014 is to evaluate the ASR for three candidate hybrid inorganic/polymers in practical systems at 80ºC and 45 kPa.



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Question 1: Approach to performing the work

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

- The approach to synthesize membranes with tunable proprieties from super-acidic inorganic moieties to produce low-cost, durable membranes seems very solid.
- This project is working on incorporating hybrid inorganic polymers (HPA) into membranes. These materials have the potential advantage of having high proton conductivity at low water content. This project approach is different from other membrane projects and may lead to a large improvement, as opposed to smaller incremental gains. The project is addressing membrane material properties for high conductivity at relatively high temperatures (95ºC to 120ºC) at low RH. Colorado School of Mines (CSM) primarily does material synthesis, followed by partner scale-up and MEA testing. The initial work is related to a previous project nominally on the same topic and is repeating the synthesis work. The project is also working on Pt/HPA functionalized carbons, which is relevant to making MEAs and putting this ionomer into the catalyst layer.
- This project builds on extensive prior work, some sponsored by the U.S. Department of Energy (DOE), on the use of HPA protonic groups in membrane materials for use in polymer electrolyte membrane fuel cell (PEMFC) technology. The approach is fundamentally sound insofar as HPA-based acids have excellent properties, as shown by these researchers in earlier work, and are strong candidates for inclusion in future HT membranes. The Dyneon and perfluorocyclobutyl (PFCB) chemistries both include hydrocarbons that, at least according to conventional wisdom, could be sites for chemical decomposition. Presumably the researchers have a plan to address that, and when they get to the point of having materials suitable for performance testing, this could become an issue. Also, the investigators may not be giving adequate attention to swelling. Many otherwise promising materials have been shelved because they swell too much. DOE targets are pretty aggressive, and in many cases they require that the ionomers are supported in some way (e.g., with a nanofiber matrix). The researchers do not seem to be proposing this. It if fine if they can meet the DOE targets for swelling without supports, but the presentation did not discuss this point much. The researchers should probably address it in future presentations/reports.
- The multidirectional approaches taken by the team for the completion of all tasks are adequate. All the analytical techniques had been thought through appropriately. The study on material synthesis based on functionalized super-acidic inorganic moieties is the reasonable approach.

- The project does address the critical barriers—all of them relate on some level to unit cost, performance, and durability. The approach to solve the problem with HPA and ZrP has been tried in the past with similar additives that have been proven to increase ionic conductivity; however, the basic ionomer was not stable enough (mostly done with sulfonated polyetheretherketone [SPEEK] or some other membrane). This project addresses this problem by using Dyneon, which is a remarkably stable polymer. The only negative part of the approach that has been observed in the past is the washout of these additives as the ionomer degrades, leading to a strong decay in conductivity. The other problem is related to a DOE target—specifically, setting the target in terms of overall resistance. Engineering of the ionomer should be separated from specific material properties. Targets of materials should be set in terms of conductivity or resistivity. Conductance or resistance will be a function of processing parameters to reduce film thickness and area.
- This project builds on the principal investigator's (PI's) previous projects developing HPA-containing polymers. His previous work showed materials with promising conductivity at dry conditions that so far have not been repeated with the new project. His approach of relearning what capabilities were lost should enable matching the conductivity of previous materials. His approach to making water-stable, robust, and high-quality thin films is less clear. Only the approach using Dyneon seems to have a path to mechanical stability. While the PI claims to be able to tune the other concepts, the detailed approach was not provided. It was nice to see that the PI is considering cost, but it would be good to see more details about the actual projected cost of the membranes developed, not just the system. A 20% increase from the baseline material set is not acceptable.
- Barriers to be addressed are (A) Durability, (B) Cost, and (C) Performance (ASR <0.02 ohm cm^2, 120°C, 40-80 kPa H_2O). It is very early in the project, and while the high-level plan is laid out, specific details of risks and mitigations are not all clearly addressed. The project is addressing Barrier C first, which is a reasonable approach, as long as sufficient work has been done to confirm a potential path to meeting Barriers A and B, but this is not completely clear. In terms of durability, it was stated that all systems have tunable properties, either co-monomers for desired mechanical properties, or base polymers with desired mechanical properties, and thus the systems should be sufficiently robust. However, it was not clear what the trade-offs would be. To date, at this early stage, there is not consistent robustness (e.g., one polymer is not stable to boiling), but it is stated that this can be tuned to be made stable. The Dyneon-based system was chosen for mechanical properties and is expected to be the most capable of meeting mechanical requirements. The other two systems may have issues. Work to address Barrier B is an initial analysis indicating that costs as low as $40/kW could be attained at high production volumes, conducted using the Brian James model. However, it is not clear exactly how the model is adjusted to get this number, or where the membrane cost estimate is from. Work for Barrier C has been the focus to date and also builds on a previous project that showed very good low-RH conductivity. It appears that the approaches used have the feasibility to meet the performance target, given appropriate processing. However, much work needs to be done. Meeting the ASR is dependent on producing films of sufficient thinness. Two risks seem inherent in this: (1) the ability to produce the thin film with sufficient homogeneity and robustness; and (2) the assumption that the ASR will remain linear with thickness down to very thin films—there may be a risk that surface effects will come into play that result in a non-linearity. In addition, none of the testing has been done at low RH, an essential part of meeting the target. The maximum temperature tested has been 90°C, although the cost modeling was done at 120°C. Overall, it is very early in the project; the technical approaches may be feasible, but there is a lack of clarity on planned approaches to overcome barriers and potentially an understating of the difficulties expected to be encountered.
- It is unclear if it is implied that the fabrication of MEAs will not require the use of a solubilized version of the membrane, because the catalyst layer includes a support that is functionalized with acidic groups. This is an important question because the answer could determine the need to measure the reactant permeability of the polymer system. Questions remain about how stable these acidic groups are covering the catalyst support, what maintains the catalyst layer integrity, and whether any binders are being considered. On slide 6, it is unclear if Year 1 should have been used instead of the Year 2 first-quarter milestone entry.
- The investigators have been at this approach for a long time. It is a given that there is a lack of focus. It is early in the project, but chemistries have not yet been chosen; whether to blend or not to blend, and what type of polymers to blend with has not been thought out or rationalized. There are many promises of good films coming down the pipeline, but they have been at this long enough that many of these issues should have been settled.

- Incorporation of HPA into a membrane could improve the robustness of fuel cell systems by adding another proton conduction mechanism to the membrane. The presentation did not make clear how the present project differs from the earlier project, if at all.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.6** for its accomplishments and progress.

- The team has successfully conducted benefit analysis of a high- temperature electrolyte to show that costs as low as $40/kW could be attained at high production volumes. The team has also demonstrated the challenges and mitigation strategies for film fabrication using HPA-based polymers. The team may think about an encapsulation approach to immobilize HPAs in the polymer matrix.
- Although the project is still very young, the team has already made good progress in producing and evaluating several functionalized polymers, two of which already meet key milestones.
- This version of the project is relatively new, so there are not many new results to report yet. The team is mostly working to repeat prior work and establish continuity with the present project. The researchers obviously have some bugs to work out (e.g., blends comprised of perfluorosulfonic acid [PFSA] ionomers and HPA ionomers have lower conductivity than either component individually, which is obviously not expected or desirable), as the PI noted in his presentation. Conductivities on most materials are not yet very high, and the ones that are high appear to be water soluble. Obviously the researchers have to work out some bugs, but the reviewer is confident that they will do that.
- Because this version of the project has been going for less than a year, it is difficult to generate a meaningful rating for accomplishments. This incarnation of the project appears to have made progress in generating cleaner ionomers than were generated in the past. The project is not as far along at generating testable MEAs as one might have hoped at this stage.
- Even though the project is in the beginning and still gaining traction, some key points are drifting away from the key targets and need reevaluation:
    - The accomplishments with the first two systems are disappointing because the obtained conductivities are not better than state-of-the-art ionomer materials.
    - Ionomer conductivity is being measured at 95% RH, which is different from a typical automotive target of 50%. This is surely due to some limitation in the laboratory, but it needs to be addressed to provide systems engineers with information to calculate cost trade-offs between membranes and humidifiers.
    - There is no need to report overall resistance at this point. The project is about identifying a material that meets specific material targets. Therefore, from now on, the results should be reported as conductivity. Later on, the material can be made to achieve reduced thickness. Also, the researchers should plot a summary graph, with their baseline ionomer and all three systems.
- The purpose of slide 7 is currently unclear in relation to the three proposed polymer systems. It is not clear how this information is going to be used to ensure that one polymer system will meet the <$20/m^2 target. It is unclear if it should be assumed that if the stated cell performance and membrane cost targets are met, then the selected polymer system will meet the system cost target. It is not clear what purpose the co-monomers play during the synthesis of trifluorovinyl ether (TFVE) membranes. It is unclear whether the co-monomers introduce variability in both structure and equivalent weight. It is not clear why it is necessary to add either polyvinylidene fluoride (PVDF) or the 3M ionomer (slide 11). On the summary slide, it is claimed that the films created are robust; however, no evidence was given. Although the three chemistries have the potential to meet the resistance target with thinner films, it is unclear if System III is preferable, considering it will be thicker and may offer more resistance to pinhole formation.
- To date, the accomplishments on this project have been slow; significant work appears to have been required to come up to speed to where the prior project ended. Current membranes fabricated are thick: 107 micron thick film. While this membrane fabrication accomplishment demonstrates the material synthesis, the membranes need to be made with purity, thickness, and quality to be evaluated by the DOE Fuel Cell Technologies Office (FCTO). The researchers needed to relearn multiple synthesis steps—this is why the project should be continuously funded by DOE without gaps. It is also one of the disadvantages of using graduate students, who come and go in projects, because continuous staffing is difficult to maintain and knowledge is not always transferred.

- It is very early in the project (stated 20% completion); the systems chosen have a feasible path to meeting the goals, but there is insufficient information to provide a confidence level on the project's ability to meet the targets.
- There are many comments about work not being performed, or experiments not being reproducible, because the researchers are bringing new students up to speed on synthesis and characterizations.
- So far, the investigators have been focusing on repeating past work. Until they do that, it is hard to evaluate progress.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The team consists of a good mix of national laboratory and industrial partners. Collaboration with 3M, Nissan, and the National Renewable Energy Laboratory (NREL) is very advantageous to the team.
- Having 3M onboard to supply blending and base materials and to do characterizations is a big benefit to the project.
- The project features good representation of the organizations and expertise.
- The project includes collaboration with 3M in the form of PFSA ionomers that were supplied on an in-kind basis; with Nissan, which will do MEA testing; and with NREL, which is a longtime partner of the PI and his colleagues. Collaboration with NREL and Nissan has not yet ramped up—presumably it will do so as better materials emerge. The degree of interaction with these partners is appropriate at this stage.
- Appropriate partners are identified; however, there does not appear to be much collaboration to date. This is expected to ramp up later in the project.
- The project is relatively new; the contract with Nissan looks like it has not yet been signed. NREL does not yet seem to be involved in this project. 3M is supposed to be involved (in-kind partner); however, its role has not been defined. Also, the project indicated that General Motors (GM) will test materials; however, it has not yet provided materials to GM.
- The "reviewers-only" slide provides a good overview of how NREL and Nissan are involved in the project (not so much 3M—maybe because it is an in-kind partner). However, during the presentation, it was not clear what part of last year's work, if any, was done by these institutions.
- CSM will be interacting with both national laboratory and industrial partners, although these interactions have not yet begun.
- CSM has competent partners in NREL, Nissan, and 3M. To date—which is admittedly early—the contributions from the partners are not obvious. There were no plans shown regarding collaboration with other existing projects.
- Until MEAs can be made, the contributions of other partners will be limited. Collaboration will likely improve as the project continues.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The project is relevant to the objectives of the FCTO Multi-Year Research, Development, and Demonstration Plan (MYRDDP). All activities are aligned with DOE's goal. The focus of the project is to demonstrate a low-cost hybrid inorganic/polymer from super-acidic inorganic functionalized monomers.
- The project supports all of the MYRDDP goals and objectives related to HT membranes and protonic conduction under hot and dry conditions. The swelling/dimensional stability is a little concerning, and the PI should consider this topic more carefully. If he is able to meet the conductivity targets, his work will have the potential to have a large impact if his materials can also meet the dimensional stability targets and be easily incorporated into MEAs. It is too early to tell yet whether this will be so.
- High-performing, durable, low-cost membranes are needed to enable fuel cell electric vehicle commercialization. These materials must be low cost and chemically robust to meet requirements and be highly relevant, and the project should address these areas as well.

- The project does address the key barriers with System III:
    - Performance: The project seems to be able to achieve DOE targets at 95% RH. However, this RH level is not relevant for automotive applications at HT.
    - Unit cost of $20/m^2$: It is not clear how cost estimation of this membrane is going to be done, because it is not shown in the future work. The base polymer itself can probably be made at these cost targets, but the process and raw material costs of the additives should also be estimated.
    - Durability: No information was shared regarding this point, because it was not tested yet. An increase is expected in permeation rates, which would jeopardize the chemical durability and overall catalyst durability due to an increased number of start-up/shutdown events.
    - Efficiency: The idea to go the range of 10 microns will likely result in high permeation rates in order to achieve the target resistances. Therefore, the project should also have a constraint regarding permeation rates, because this would affect the total stack efficiency, which is another important DOE target.
- These materials have the potential advantage of having high proton conductivity at low water content. This project is not meant to make incremental improvements, but to develop basically a new membrane with significantly improved material properties in terms of conductivity at HT/low RH.
- The team aims to produce low-cost, durable, tunable films for fuel cell membranes that will meet critical DOE cost targets.
- The project supports the DOE Hydrogen and Fuel Cells Program's goals and objectives. However, the automotive original equipment manufacturers have reduced the importance of 120°C, dry performance. A membrane truly able to reach 120°C, dry operation with good conductivity will provide advantages, given the ability of the rest of the system to operate under these conditions (e.g., catalyst layer conductivity and seal and membrane lifetime). However, it is not clear if the rest of the system will tolerate these conditions, and how much advantage will be gained through a successful project.
- The project aligns very well with DOE targets. However, the issues are likely linked to the electrode/membrane interface, but this aspect was not discussed, raising some doubt about the usefulness of the proposed membranes. Inks with solubilized ionomers were mentioned as a possibility.
- No clear demonstrations of the benefits of the inorganic additives beyond what can be done with completely organic membranes were apparent in the presentation or the supplementary slides. The cost analysis seems unrealistic in assuming that the benefits of the materials being developed in this project will allow complete removal of the humidification components and controls from the fuel cell system. Catalytic gains associated with operation at 120°C rather than 80°C are likely to be quite small—the true activation energy (at constant practical potential) for oxygen reduction reaction is considerably lower than the oft-quoted, but inappropriate, activation energies at constant overpotential (the reversible O_2 potential also shifts with temperature).
- It is very unlikely that these materials will be utilized in PEMFCs. Even using the investigators' most optimistic assumptions, these materials would require a membrane of ~10 µin thickness without a support. This is very difficult, especially considering there is no particular reason to believe that their mechanical properties will be better than current materials that require a support. The investigators are adding PFSA to improve mechanicals, but those require a support at these thicknesses; considering these materials will in all likelihood also need a support, the membrane will need to be very thin. This also brings into question whether the investigators will be able to meet the hydrogen permeability targets or the mechanical targets.

Question 5: Proposed future work

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

- The future work described is aligned with the proposed overall work scope of the project.
- The team has proposed a good balance of synthesis development, characterization, and scale-up.
- The project is, at this stage, highly focused on future work because it has just started and the team is still working out some synthesis and membrane processing bugs. The investigators' plan for future work is reasonable, insofar as it is focused on achieving the goals that were originally set out for the project. It is OK for them to be at this stage now, but a year from now they need to be a little further along.

- The project path is well-defined; the work to make the ionomeric materials relevant to incorporation into catalyst layers is important, although it is not really a membrane project. The PI should be given the freedom to do this because it could lead to an important result.
- One hopes that the people newly trained to make the organic/inorganic composites will continue progress in their training to address the concern of student turnover, to allow attention to be shifted to making good MEAs for detailed testing. Plans seem to be appropriate, but detail is lacking in such items as “Electrode optimization and MEA fabrication.”
- The future work listed is the optimization and characterization of the various options, but no details were provided on what characterization would be done and how the optimization would be conducted.
- The high-level plan and milestones are appropriate. Increased details on risk mitigation are needed.
- For System III, the need for an increase in water stability should have been explained.
- It was not clear what membrane properties will be measured next, even though there is mention of a full membrane protocol testing. Systems I and II do achieve the target resistance performance at lower thickness; however, the material conductivity is not better than state-of-the-art ionomers. Thin-film processing on Systems I and II are not recommended, because of this.
- The project is off to a slow start, but it could really use some focus at the beginning. The investigators should settle on a single chemistry, see it through, then double back if needed.

Project strengths:

- This project approach is substantially different than other membrane approaches, and the theoretical possibility of successfully producing a high conductive membrane that requires low RH exists. The project is a departure from the approach taken by many of the Office of Energy Efficiency and Renewable Energy (EERE) projects, which show incremental gains; to achieve this gain, the project requires time to make progress, and addressing short-term quarterly milestone could very well take the project in wrong directions.
- The team is composed of respectable research organizations with adequate expertise. Overall, the team is equipped with the knowledge base, resources, and industry/academia/national laboratory mix that is required for the success of this project.
- HPA acids offer unique and attractive properties for PEM materials, and the lead investigator is ideally positioned to pursue research on HPA-based PEMs. He has a significant amount of experience and has assembled a good supporting team.
- The project shows a path to low-cost, low-RH, high-temperature capable membranes. Three paths are included to reduce the risk of failure. There are feasible paths to also meeting durability. Collaborators will provide strong support and ensure the project is commercially relevant.
- The project features varied and experienced team members. Several polymer systems with adaptable parameters will be explored, raising the probability that at least one will satisfy technical targets.
- Strengths of this project include its experience in handling inorganic proton conductors, and relearning experience used in coupling the inorganics to organics and making workable films.
- The team has a strong background in membrane development and has selected strong collaborators.
- This project is building on the promising results from the PI’s previous project.
- The team has a capable group of researchers who are well versed in the field.
- A strength of the project is the use of Dyneon as a base polymer.

Project weaknesses:

- This project, to date, is off to a relatively slow start. This version of the project is relatively new, and due to a lapse from the previous project, it has had to spend time repeating some prior work to bring the project up to speed. DOE funding to make these projects continuous, without lapses, is required to prevent this “wasted” effort. This project seems to be far from making materials that are relevant to the Program. It needs to concentrate on either making, or adding a partner that can produce, these membranes. Knowledge transfer from the previous project, from student to student, apparently was weak. The project partners do not appear to have been incorporated into the project yet. These project partners should be utilized (or a new partner added) to help with producing materials in terms of quantity and membrane thickness for evaluation by organizations like GM.

- The range of materials that the team will investigate is a bit diffuse—aryl trifluorovinyl ethers, Dyneon, and zirconium phosphonates. Only two of the three systems have HPA, and their chemistries are very different. The common theme in the research is not always clear. As the researchers work the bugs out of their chemistry, they may wish to down-select, giving up on some chemistries and focusing on those that are working the best.
- The project lacks a detailed plan. The PI claims to have a large toolbox of thin forming techniques, including solvent casting and thermal and pressure treatments to produce thin, robust films that will also meet the performance targets. It would be good to see more specific details of the plan and why the PI believes that his approaches will work. Otherwise, it seems more like a trial and error approach.
- This project's slow start is an area of weakness. After many years of investigation, these chemistries still have not shown a large advantage compared to PFSAs in low-RH conductivity. Coupled with all of the questions of stability, mechanical durability, and processability, the investigators have a daunting challenge.
- Project weaknesses include the team's inexperience with producing Dyneon and scaling up the batch size, as well as processing quality, reproducible thin films. Hopefully the team will be successful with both during the upcoming year.
- Even though Dyneon is a stable polymer, it is not the best ionomer in the industry for conductivity. Other polymers without the additives might still be better than this approach.
- Issues related to the membrane/electrode interface should be clarified and activities should be reinforced, considering they are crucial for a successful integration of the membrane to the MEA.
- To date, no thin films with adequate properties have been made to allow demonstration of the approach in fuel cells.
- There is a lack of clarity on detailed plans and risk mitigation.
- More polymer expertise is needed to prepare a polymer HPA composite.

Recommendations for additions/deletions to project scope:

- The investigators should reinforce activities related to the membrane/electrode interface to ensure the probability of a successful integration of the membrane to the MEA. For instance, at least one additional MEA structure should be considered to reduce risks.
- CSM should concentrate on the chemistry, materials synthesis, and advanced characterization. Either partnering relationships or another partner should be explored to "manufacture" the thin membranes (~20 micron) that will be required to make this project successful.
- The only recommendation is that if the zirconium phosphonate polymers continue to be water soluble, the work on them should probably be discontinued.
- The focus should be more than on reduced thickness and robust films. The team should continue to focus on conductivity improvements, as extremely thin films (<10 micron films will be required for Systems I and II) are likely to have crossover issues.
- The project team should delete activities for Systems I and II. Conductivities measured are low, even at 95% RH. The team should also investigate potential new systems where material conductivity is promising.
- The team should not make any changes to the scope.

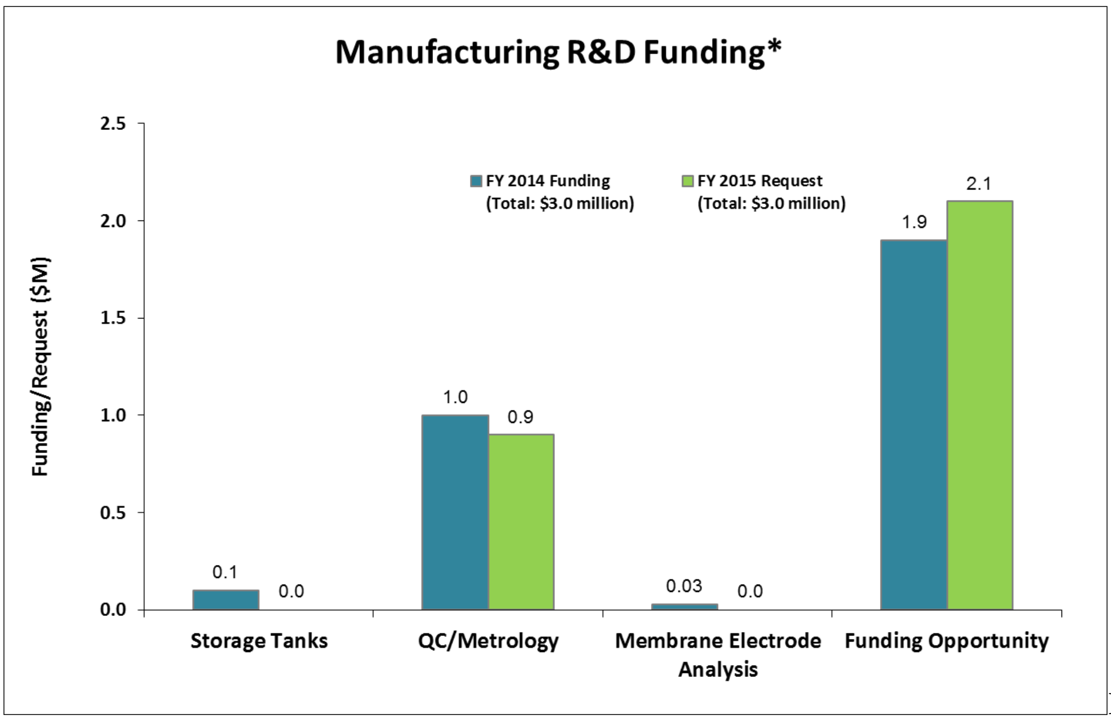
2014 — Manufacturing Research and Development (R&D)
Summary of Annual Merit Review of the Manufacturing R&D Sub-Program

Summary of Reviewer Comments on the Manufacturing R&D Sub-Program:

According to reviewers, the objectives and progress of the Manufacturing R&D sub-program were clearly presented and prior successes were described. The reviewers noted that the sub-program thoughtfully—and with industry collaboration—considered the needs and intent of the U.S. Department of Energy's Hydrogen and Fuel Cells Program (the Program) and generated a strategy to develop appropriate solutions. The reviewers suggested that continued collaboration with regions and clusters as well as industry and stakeholders would be helpful. In fiscal year (FY) 2014, one manufacturing project, which addressed fuel cell stack in-line testing, was reviewed.

Manufacturing R&D Funding:

Funding for the Manufacturing R&D sub-program was $3 million for FY 2014, and $3 million was requested for FY 2015. The FY 2015 request-level funding will continue existing Manufacturing R&D sub-program projects and provide funding for new analysis projects on supply chain development and global manufacturing competitiveness through a competitive funding opportunity announcement, subject to appropriations.



***** Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area and the relative merit and applicability of projects competitively selected through planned funding opportunity announcements.

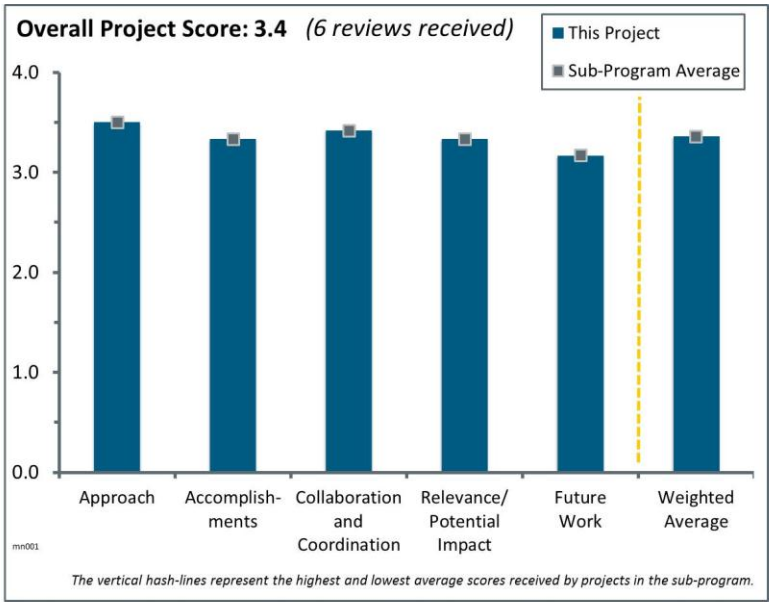
Majority of Reviewer Comments and Recommendations:

One Manufacturing R&D project was reviewed, earning a score of 3.4. Reviewers judged the project to be highly relevant to Program activities and to feature an excellent technical approach. They noted that project progress and accomplishments were extremely good. The project team was judged to be strong; participation and contribution from industry partners were identified as useful and coordinated.

Fuel Cell Membrane Electrode Assembly (MEA) Manufacturing: One project was reviewed in the area of fuel cell MEA manufacturing, with a score of 3.4. Reviewers noted that the approach for the project was very good and that collaboration with industry has been, and continues to be, very good. Reviewers also noted that NREL made significant progress this year in further developing diagnostic techniques and implementing them at Ion Power. The reviewers encouraged NREL to reach out to additional companies for technology transfer as well as correlate defect size (as detected in a webline) with fuel cell performance.

Project # MN-001: Fuel Cell Membrane Electrode Assembly Manufacturing Research and Development

Michael Ulsh; National Renewable Energy Laboratory



Brief Summary of Project:

The objectives of this project are to obtain quality control (QC) needs from industry partners and forums, develop diagnostics, use modeling to guide development and understand the effects of defects, validate the diagnostics in-line, and transfer the technology to industry.

Question 1: Approach to performing the work

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

- In general, the approach is good. The approach is relatively broad-based and covers a number of defects that can occur in the manufacturing process. Collaboration with industry has helped to sharpen the project focus. So far, the project has primarily focused on detecting known defects; the real proof of the value of these diagnostics will be testing on actual manufacturing lines with unknown defects. The follow-on steps that need to be addressed are to determine the impact of performance on actual defects that are detected by these diagnostics.
- The approach is very good and is broad-based and covers a number of defects that can appear in different manufacturing processes for membrane electrode assemblies (MEAs) and catalyst coated membranes by a roll-to-roll technique.
- The tasks in this project align very well with the Manufacturing sub-program's milestones in the Fuel Cell Technologies Office Multi-Year Research, Demonstration, and Deployment Plan milestones. The relevance of the approach to actual manufacturing practices and to industry is implicit in the collaborations with MEA and membrane suppliers and the implementation of the techniques in industry.
- The National Renewable Energy Laboratory (NREL) has a very good process to overcome the problems. The progress looks very beneficial to the fuel cell industry.
- Techniques for on-line QC are reasonable if it can be shown that detection limits are appropriate. Flaws that decrease performance or durability must be detectable. So far, this has not been demonstrated.
- NREL's approach incorporates key elements including industry input, modeling, and validation to evaluate prospective QC techniques.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- NREL made good progress this year developing the infrared/direct current (IR/DC) diagnostic for detecting imperfections on web coating lines showed good progress this year. The method was verified on the new roller system at NREL and the data agreed with previous results. The technology was implemented on the coating line at Ion Power, and data were collected on three coating runs. Defects (scratches and excess droplets) were successfully detected at speed of the drying oven. In additional testing at NREL, the Ion Power data were replicated; excitation conditions for line speeds up to 60 feet/minute were determined.

- Development of the infrared/reactive impinging flow (IR/RIF) diagnostic was also substantial for detecting defects in moving gas diffusion electrode (GDE) sheets. Laser drilling of holes in the gas knife should decrease measurement variability. Bare spot defects as small as 2 mm x 2 mm were successfully detected on GDE sheets moving at 30 ft/min. Modeling at Lawrence Berkeley National Laboratory (LBNL) has helped to optimize knife hole geometry and spacing. Model results are in fairly good agreement with experimental results.
- Variation in capacitance due to relative humidity variability led to the no-go decision on further development of the technique for measurement of the ionomer-to-carbon ratio (I/C) in MEAs.
- The establishment of an in-house roller system should speed the evaluation of new techniques and reduce the deployment time in an industrial environment. The effort successfully deployed IR/DC equipment with an industry collaborator.
- NREL made significant progress this year developing diagnostic techniques and implementing them at Ion Power. The no-go decision on I/C determination shows that the project is grounded in practical development of techniques and not wasting time on tasks that are not feasible.
- NREL shows excellent results at current manufacturing speeds; the results can be applied to the industry.
- Good progress has been made for the IR/RIF technique in an open environment with very high moving rates.
- Good progress in technique development has been made.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- Teaming with industry is essential for the success of this project. Collaboration with industry has been and continues to be very good. NREL has interacted with component suppliers and with automotive original equipment manufacturers (OEMs) to determine the critical quality assurance (QA)/QC issues that need to be addressed and detection methods requiring improvement. Modeling by LBNL has enhanced the development of the IR/RIF technique.
- There is strong collaboration with major industry leaders and coordination with the university system.
- The project has a good mix of industrial, national laboratory, and academic collaborators providing valuable contributions.
- The project is well-coordinated and has excellent cooperation with relevant industrial partners.
- Unfunded collaboration includes fuel cell component manufacturers and stack integrators.
- The coordination and input from Ion Power is clear. However, it is unclear what the Colorado School of Mines and LBNL are doing in the project and what value is added, at least from what was presented.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- Successful implementation will detect defects and will allow them to be removed before the MEA is installed in a complete system. This process could improve reliability and life of the cell stacks, as there will be smaller/fewer defects that make it into the complete stack.
- The true relevance of the project will be realized when one of the methods under development is implemented on actual manufacturing lines and proves to be effective in reducing the number and rate of reject parts. The relevance would be enhanced when the impact of detected defects on performance is known. This is an important development that needs to be addressed.
- The project is targeting the main challenges of the quality control in fuel cell manufacture. The identification of the impacts of the imaged defects on fuel cell operation and durability will be more relevant.
- There appears to be a critical need for QC in MEA manufacturing, based on industry input. This project is well-coordinated with a leading MEA manufacturer, Ion Power. The coordination with other industry collaborators is unclear.

- The effort is contributing to addressing a number of program goals and is a key element in moving technologies towards viable mass production. The reactive impinging flow method needs additional characterization and validation work.

Question 5: Proposed future work

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

- All of the proposed future work is good and important, especially the study of the impacts of the relevant defects on fuel cell performance and lifetime.
- Planned future work continues to address barriers, most importantly the effects of defects on cell performance and lifetime.
- The project has a strong path to achieving all the goals.
- Future work is briefly outlined. It includes demonstration of the RIF technique on the NREL web line, further development of the optical and infrared techniques, and most importantly, the study of the impact on performance and lifetime of relevant defects. No milestones are presented, so assessing the likelihood of success is not possible. The latter item is very important, and some details concerning the approach to the study of the effects on performance should have been presented.
- The two following activities mentioned under future work are the key to project's value and should be the main focus of future effort: (1) study the effects of relevant defects on cell performance and lifetime, and (2) develop and integrate models for optimizing diagnostics and for predicting performance effects of defects.
- Strong effort to relate detectable flaws to cell behavior must be shown, and the detection techniques must be shown to be capable of detection.

Project strengths:

- This is an all-around strong project with valuable collaborators and a methodical approach to QC technology development.
- NREL's process will be very beneficial to the manufacturing MEA industry and could have a major impact on the fuel cell industry. The project is well laid out and seeing good results. The costs/benefits of implementing in industry seem very good.
- NREL is addressing relevant issues and is adapting/improving available diagnostic techniques for use on high-speed manufacturing lines. Good collaboration with industry is evident.
- The project team has carried out excellent research and cooperates well with industrial partners.
- The integration and collaboration with Ion Power is a project strength.
- The knowledge and skill base of the collaborators are a project strength.

Project weaknesses:

- There is a lack of understanding of the identified defects' impact on the fuel cell's short- and long-term performance.
- The impact of defects on performance still needs to be understood. The NREL approach to determining the impact is not at all clear.
- Correlation of detectable flaws to performance and durability is lacking.

Recommendations for additions/deletions to project scope:

- The need for increasing the sensitivity of the measurements should be determined, which will require understanding the impact of defects on performance. The lower limit for a particular defect is not clear. Determining the I/C ratio should be included in future work despite the no-go decision on the capacitance method.
- The researchers should determine whether the reaction temperature at the surface of the catalyst layer will have an impact on fuel cell performance and lifetime when the IR/RIF technique is used.

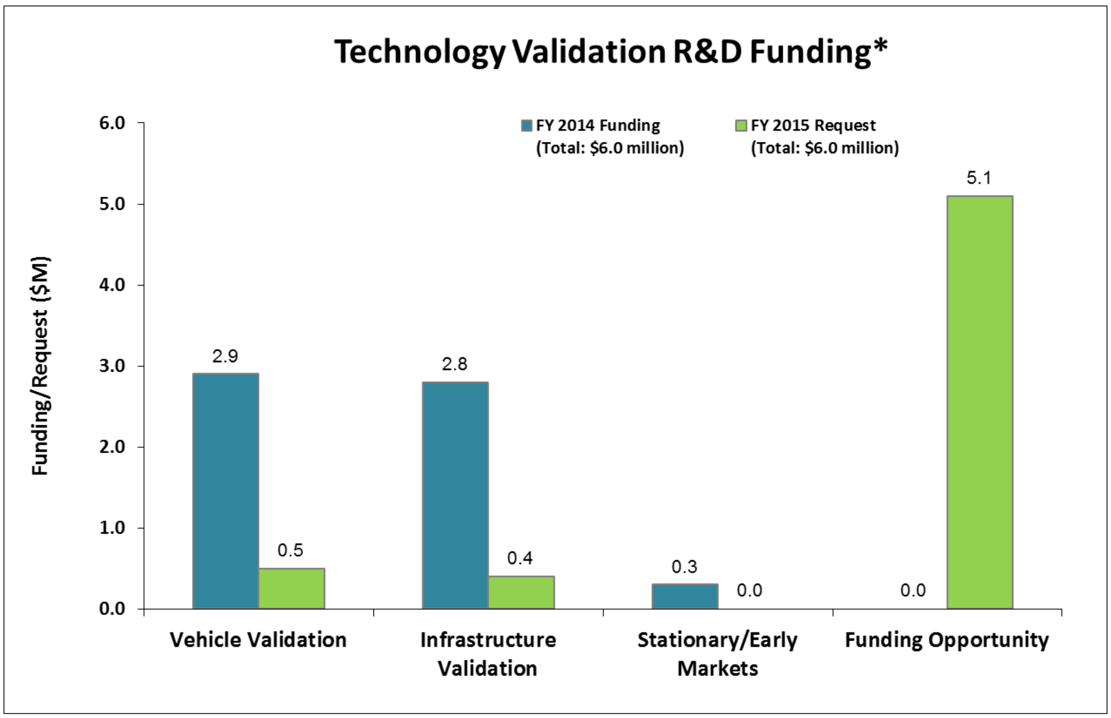
2014 — Technology Validation
Summary of Annual Merit Review of the Technology Validation Sub-Program

Summary of Reviewer Comments on the Technology Validation Sub-Program:

In general, the reviewers believed the sub-program area was adequately covered. They noted that the role of the Technology Validation sub-program within the structure of the Fuel Cell Technologies Office was clearly identified. Reviewers stated that progress related to projects was clearly presented, and that plans were identified for addressing issues and challenges. They characterized the partnership with NREL's data collection/analysis team as key to the success of the sub-program's efforts and to achieving its goals and objectives. Given the high failure rate observed with compressors, reviewers saw continuing to evaluate and validate compressors as important. Reviewers also suggested that the sub-program more explicitly state the top challenges in each validation area.

Technology Validation Funding:

The Technology Validation sub-program's funding portfolio will enable it to continue to collect and analyze data from fuel cells operating in transportation applications (e.g., light-duty vehicles, medium-duty trucks, and buses), stationary and early market applications (e.g., material handling and backup power), and hydrogen infrastructure activities (e.g., fueling stations and components). Analysis of several hydrogen refueling stations and fuel cell electric vehicles in California and the Northeast will be the main focus of the data collection activities. The fiscal year (FY) 2014 appropriation was $6 million. The FY 2015 request of $6 million is subject to congressional appropriations.



***** Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area and the relative merit and applicability of projects competitively selected through planned funding opportunity announcements.

Majority of Reviewer Comments and Recommendations:

The reviewer scores for the eight Technology Validation sub-program projects that were reviewed had a maximum of 3.8, a minimum of 2.5, and an average of 3.3. Key strengths identified by reviewers in all of the Technology Validation projects were (1) the excellent participation from collaborators and (2) the potential for the projects to contribute valuable data to gain enhanced insights and to successfully deploy hydrogen and fuel cell technologies. Reviewers also observed that NREL's approach for collecting, securing, and analyzing data is well established and trusted by project collaborators.

Fuel Cell Electric Buses: Reviewers noted that the fuel cell electric bus data collection project is critical to the wide-scale adoption of fuel-cell-powered electric buses, with tangible results providing a consistent history of technology performance and cost improvements over time as well as valuable insights for both U.S. Department of Energy (DOE) project managers and transit fleet operators. Reviewers suggested that more transit agencies should be involved in the evaluations, and that performance and reliability data from similar applications in other countries should also be considered.

Stationary Fuel Cells: Reviewers remarked that the data evaluation process for stationary fuel cells should be more clearly linked to key research or technology deployment questions, and that feedback should be given to DOE about the gaps in technology performance and market status. They also suggested that it would be valuable to evaluate stationary fuel cell deployments in various other states.

Material Handling and Backup Power: Reviewers observed that NREL's business case analysis of the economic and operating performance of fuel cell forklifts and backup power systems added value to the commercialization of niche market hydrogen and fuel cell technologies, as well as contributed to the commercial ramp-up of these systems. Reviewers recommended that industry should be encouraged to keep providing data, and that qualitative verbal feedback from operators of these systems could be obtained to provide enhanced insight.

The hydrogen component validation project's evaluation of compressor failure mechanisms was seen to have the potential for a large impact because compressors present key reliability issues in hydrogen stations. Reviewers also suggested obtaining input from other compressor suppliers and performing a technoeconomic analysis of the impact of the project.

Reviewers viewed the validation of an advanced high-pressure electrolyzer as a project with a real-world strategy and the potential to lower costs. An area identified as needing more attention was cost targets and estimation, along with evaluation of the economic impact of installing high-pressure electrolysis. Reviewers also wished to see evaluations of how the technology could be scaled up.

Fueling Stations: The hydrogen fueling station established at California State University, Los Angeles, was viewed by reviewers as having the potential to identify optimization potentials for components of electrolysis-based hydrogen fueling stations, while having an educational aspect as an added benefit.

The Gas Technology Institute and Linde collaboration to build and evaluate five hydrogen fueling stations in various California locations was viewed as having the potential to enable comparisons across stations and help expand the network of stations nationwide. Risk analysis and addressing costs targets were suggested as additions to the project goals.

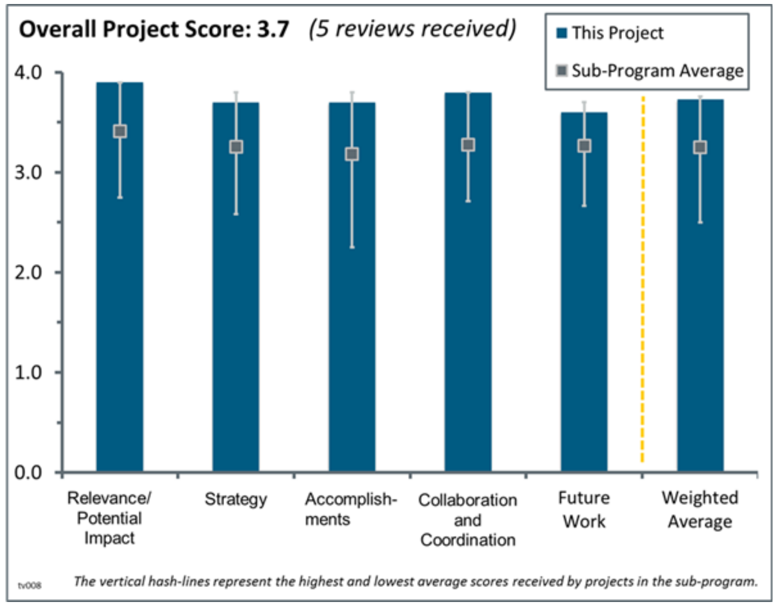
The Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) project was viewed as having the potential to contribute to the deployment of hydrogen stations and to address real-time technology performance and operation issues, but it was also perceived as too new to comprehensively evaluate. Reviewers suggested that further attention be devoted to characterizing H2FIRST and that indicators of project success be measurable.

Project # TV-008: Fuel Cell Bus Evaluations

Leslie Eudy; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to: (1) validate fuel cell electric bus (FCEB) performance and cost compared to U.S. Department of Energy (DOE)/U.S. Department of Transportation (DOT) targets and (2) document progress and lessons learned on implementing fuel cell systems in transit operations to address barriers to market acceptance.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.9** for its relevance/potential impact.

- The DOE Hydrogen and Fuel Cells Program (the Program) has devoted substantial resources to fuel cell electric vehicle (FCEVs) and hydrogen infrastructure technology development and validation. Buses are an important target opportunity for fuel cell development and demonstration. Significant funding, from both DOE and DOT, has been provided for FCEBs. The National Renewable Energy Laboratory's (NREL's) collection, analysis, and reporting of performance data associated with vehicle demonstration projects, including those focused on buses, have made a vital contribution to understanding the status of technology development relative to DOE goals. NREL's data products have been continually refined, increasing their value for both government and industry decision makers. The detailed, objective results of NREL's work are easily understood by those responsible for making decisions on public and private investment in technology research, development, and commercialization. Over the past decade, this work has been an important contributor to achieving unbiased and supportable conclusions about progress toward DOE and DOT targets—bus fuel economy, FCEB utilization, fuel cell lifetime and durability, road call frequency, cost, and other metrics.
- This project continues to produce tangible results and excellent analysis on FCEBs used by transit companies in real-world conditions.
- This project is highly relevant because the deployment of FCEBs is key to getting widespread acceptance of hydrogen and FCEVs.
- This data collection of FCEBs is highly relevant and has high impact potential.
- The continued monitoring of field tests is essential to advancing commercialization of fuel cells for transportation.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.7** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- Since commencement of bus evaluations in 2003, the principal investigator (PI) and the project team have steadily refined this project's data collection, analysis, and reporting activities. With ongoing feedback from data providers and users, NREL has developed superb procedures and protocols for data collection and processing. Relevant results of NREL's analyses are provided to each transit agency associated with the project. NREL publishes reports—including an annual FCEB status report—that provide comparisons among FCEBs; conventional buses; and other buses with advanced technologies, such as hybrid electric buses. These reports provide outstanding documentation for industry, government, and the public. During the past year, data has been collected from three transit agencies operating a total of 34 FCEBs. Characteristics of these buses, including the fuel cell hybrid power plants, are included in the project presentation. Data are collected on conventional diesel buses, as well as natural gas and diesel hybrids, for comparison purposes. Actions have been taken to continually improve project results and benefits. These actions include acquiring data on the performance of advanced technology hybrid electric buses, and analysis of the factors that contribute to bus downtime and non-availability.

- The project has a consistent approach and is reaching out to other transit agencies to increase the number of buses to be evaluated and to replace buses that are already evaluated. The technology readiness levels developed by NREL help in assessing the progress toward achieving the technical targets. The iterative process adopted will lead to a successful evaluation of technology performance.
- The reviewer cannot imagine how this evaluation of FCEBs could be improved.
- All the key aspects are covered.
- The analysis includes a nice mixture of technologies and locations. Perhaps the team can get more details on the duty cycle for each FCEB, because there are so few. It would also be useful to know if the miles between road calls are prescribed by the manufacturer, and if the project team is being overly conservative. Additionally, the project team should highlight which systems had the least and greatest maintenance costs. It is not clear whether there is some way to get details on the power plant's actual useful life. If possible, it would be beneficial to compare current U.S. findings to those of Europe and other countries to get a sense of how close this technology is to commercialization.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.7** for its accomplishments and progress.

- This project provides the best, most objective, and most independent indicators of progress toward the government's targets and goals for metrics associated with FCEBs and related hydrogen infrastructure. The PI's presentation includes readily understandable graphs that enable the reviewer to compare targets and bus performance for key metrics. These metrics include fuel cell hours of operation, bus availability, fuel economy, and reliability (miles between road call). Results are generally distributed by month for each transit agency. FCEB performance is compared to that of other buses. The comparisons are made to previous generations of FCEBs; details are provided on the improvements in performance and progress toward targets. The presentation's highlights of selected accomplishments during the past year are impressive. Taken together, they provide an appreciation for the merits of both NREL's data project (reported on here) and the progress (e.g., hours in service before repair or replacement) of fuel cells used in operational buses.
- The cost and performance of these buses have been tracked for quite some time, and it is good that new technologies are continually being integrated. The analysis is consistent and provides an excellent history of performance and cost improvements over time. A total of 34 buses are evaluated. Recording more than 7,000 hours on the fuel cell power plant that was used to run the buses is a real accomplishment. The miles between road calls have shown improvement from the first to second generation.
- There is an excellent set of accomplishments. There is a very good comparison between stage one and stage two.
- It is difficult to see how this project could be improved, other than through the deployment of more FCEBs, which is obviously beyond the control of this project.
- This is nice work. The summary report is something to look forward to.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.8** for its collaboration and coordination.

- As indicated, particularly on slide 19, NREL's FCEB data project involves collaboration and routine communication with many transit agencies throughout the United States. Funding for many of the buses evaluated is provided through the National Fuel Cell Bus Program managed by DOT's Federal Transit Administration (FTA). NREL's evaluation team routinely coordinates with federal and other government organizations; transit agency management and operating personnel; bus manufacturers; fuel cell and related system providers; hybrid electric technology providers; and others who have a stake in FCEB research, development, and commercialization. NREL's bus data collection and analysis team has earned the trust of all organizations participating in the FCEB demonstration projects. NREL's team also maintains excellent communications with many organizations sponsoring related activities, both in the United States and internationally.
- This research is all about coordination with the transit agencies. NREL has done an excellent job in working with them over time. Most likely, the transit organizations also appreciate working with DOE and NREL on this project.
- The PIs are striving to put in place as much collaboration with FCEB activities as they can.
- By definition, the PI must collaborate with the bus agencies to collect these data and must collaborate with the fuel cell and bus manufacturers to interpret and analyze the data.
- It is nice to see a schedule of current and future transit agency participants. It would be beneficial to know how this project compares with Europe and other countries doing similar programs.

Question 5: Proposed future work

This project was rated **3.6** for its proposed future work.

- The decision to find additional transit companies and establish a relationship is a good way forward. NREL should continue to work with different fuel cell configurations, such as hybrid FCEBs versus FCEBs. NREL has seen significant progress, but the ultimate technical targets need to be realized and demonstrated.
- This NREL data project was undertaken in conjunction with, and to support, FCEB demonstration projects supported by U.S. government agencies. New FCEBs are being funded with the support of FTA's National Fuel Cell Bus Program. Slide 21 of the presentation provides an excellent display of fuel cells and other advanced technology bus demonstrations that will begin during the coming year. It is anticipated that NREL's FCEB evaluation project will include fewer FCEBs; however, it will acquire data and report on FCEBs located in both California and other states. Data collection will begin in Birmingham, Alabama, and Austin, Texas, later in 2014. NREL's team intends to continue its dialogue with transit agencies and others regarding data collection at new sites.
- The plan to expand coverage to FCEBs in the East will help expose public and transit agencies to a wider audience than just the West Coast.
- The future work is well focused.
- The team should add information from other countries to gauge how close to commercialization this technology may be.

Project strengths:

- There are numerous project strengths: the experience and expertise of the NREL team leader and her team, funding support from FTA, active collaboration and interactions with manufacturers and users of advanced technology buses, the quality of information in reports published by the project, and a solid contribution to FCEB progress for a relatively small expenditure of total Program resources.
- The project covers all of the important aspects and provides valuable information.
- The experienced PI and staff who have analyzed FCEB performance for many years are a strength.
- The PI has been very thorough in her evaluations and reports, reflecting professionalism and expertise in working with transit organizations.

- The relationship with a number of transit agencies is commendable.

Project weaknesses:

- The BC Transit buses are going out of service this year. There will need to be some clarity on the related statistical analysis.
- The impact of the fact that the BC Transit buses are no longer in operation is not clear.
- A better job could be done of leveraging global relationships to understand time to commercialization for the technology.
- Achieving statistically valid performance comparisons among buses is inherently difficult, due to factors beyond NREL's control. Such factors include regional differences, transit agency procedures, the variety of bus types, multiple FCEB designs, the variability of duty cycles, and differing service profiles. Changes in bus fleet management can cause problems in acquiring data needed by NREL for analysis. This is an issue that NREL is currently addressing at two agencies, as noted in reviewer-only slide 25.

Recommendations for additions/deletions to project scope:

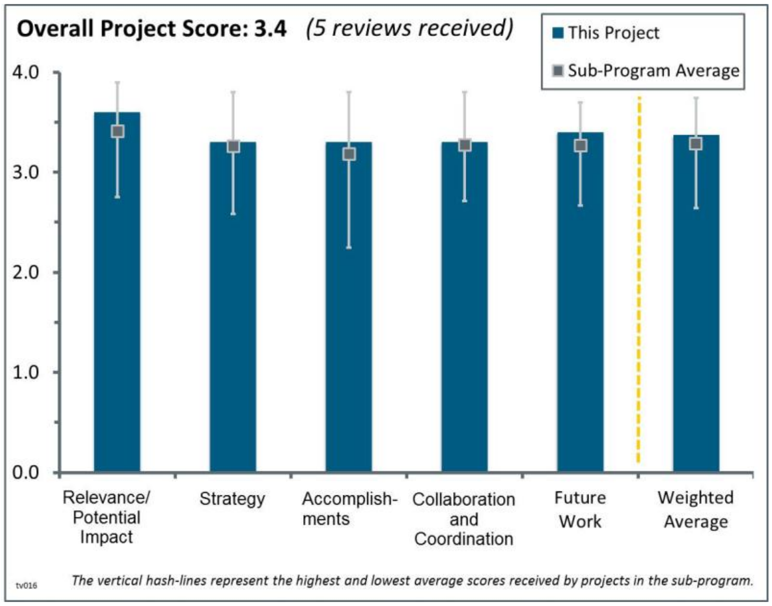
- DOE is encouraged to maintain and continue using NREL bus data analysis expertise, which has been built as a result of this project. Ideally, all FCEBs operating in the United States should be included in NREL's evaluation project. DOE and DOT managers should do what they can to ensure that this is the case. The PI mentioned sharing information with organizations in other countries. Continuing this initiative is encouraged, leading to comparisons of performance results for FCEBs around the world.
- The project team should add more transit companies.
- The project team should add details on the best-available FCEB technology from other countries.

Project # TV-016: Stationary Fuel Cell Evaluation

Genevieve Saur; National Renewable Energy Laboratory

Brief Summary of Project:

The objective of this project is to independently assess, validate, and report operation targets and system performance for stationary fuel cells under real-world operating conditions. Research addresses the lack of data on stationary fuel cells in real-world applications and provides data and context for codes and standards.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- The project is well targeted and beneficial to the research goals of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program). Having these data is essential for evaluating models that forecast ownership cost.
- The project provides a necessary service to measure how well stationary fuel cells compete.
- The project is a very good initiative to gather data from existing demonstration projects, taking into account the accessibility of data, which is on a voluntary basis and makes the project very challenging.
- The project relevance is high.
- The project appears to have good coverage of the different types of fuel-cell-combined-heat-and-power (CHP) applications and fuel sources. The project also appears to capture data from a meaningful proportion of the market, and in that sense it could be considered to be representative, although the fact that data collection is based on voluntary participation means that there could be bias in the data (poor performers may be less willing to share data compared to good performers).

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.3** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- This is a very good strategy for data collection. The proprietary nature of the feedback provided to companies is an incentive for them to participate.
- The project has set up good partners and collaborations for the collection of data.
- The goal of this effort is to collect more data on stationary fuel cells in real-world applications. This effort addresses the dearth of information on how stationary fuel cells work in practice and provides important supporting information for setting codes and standards, as well as for defining best practices. The research effort has focused on independently assessing, validating, and reporting operation targets and stationary fuel cell system performance. This project makes strong and effective use of the National Fuel Cell Technology Evaluation Center (NFCTEC) and the National Renewable Energy Laboratory (NREL) fleet analysis tool. There are concerns about statistical and data evaluation methods used in the effort. These concerns arise in part because the principal investigator, both in the presentation and during questioning, did not communicate how the project is dealing with issues such as data quality, aggregation of data, and

representativeness of data. The design of the data collection process was not clearly linked to a key research question or a DOE technology-deployment question. One of the key strengths of this effort is making links between detailed data products and composite data products (CDPs). However, the researchers were not fully aware of the limits that arise from the structure of the data compilation process. A broad range of systems are being assessed, such that there could be limited data for some CDPs, and are raising questions of statistical power, which should be addressed. When collecting data, it is important to have some concept of how the data might be used. This is a potentially very valuable data set, but it would be even more valuable if the data collection/evaluation process were better linked to some key DOE questions.

- The project gives a good overall picture of where the fuel-cell-CHP market stands (e.g., in terms of installed capacities and number of projects) in relation to competing technologies. However, there is a lack of disaggregation according to technology sub-categories, such as specific CHP capacity ranges (small/residential and large/industrial), operation under different climatic conditions, etc.
- The strategy is good; however, mixing all ranges of applications and types of technology creates aggregated data, which might cause the project to lose credibility when trying to extrapolate data, e.g., for 1 kW systems. The manipulation of data is very simple and based on too many assumptions; the average is not representative for all validated data. The purpose of collecting the data is unclear; perhaps NREL is going to analyze the data and forecast an evolution over the next 20–30 years.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- There are very good accomplishments.
- The project is well focused on key barriers to expand and understand the use of stationary fuel cells. The project is on budget and on schedule. The researchers have made very good progress with regard to both the specific project goals and the broader goals of the Program. The work is up to date with respect to all project goals. However, it should be noted that, based on the project milestones, the researchers have just started to collect operations data; this is an important opportunity for future work, and attention needs to be given to the statistical data quality and data relevance issues.
- The project is directly assessing market status compared to DOE targets on costs, efficiencies, availability, and operation. What is missing is the feedback from this aggregated information into the orientation of the Program and efforts. A snapshot of the market is useful; however, recommendations should be made regarding gaps in the technology performance and market status coming out of the data/analyses that should be addressed by the Program.
- Although similar recommendations were already provided last year, not much progress has been made in defining the different power applications and collecting/reporting data for each of them. Missing sufficient "points" should not stop NREL from collecting literature data/benchmarking data from other units in the field.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- This project has very strong and essential collaboration with agencies that provide data. The research team has been very effective in establishing initial collaborations, but it needs to expand these collaborations in order to have more robust and relevant data. It is not clear that there is a sufficiently strong link with statisticians who are experts on data evaluation with regard to the reliability and credibility of the conclusions drawn from collected data.
- There is excellent communication with the California Self-Generation Incentive Program in collecting data from the portfolio of projects that can possibly provide the data.
- The coordination with California stationary fuel cell projects is excellent.

- The project appears to have good collaboration with state bodies and with original equipment manufacturers for data collection. However, some data, such as the 27% electrical efficiency figure, is surprising, and leads one to question whether there is adequate coverage of the market.
- There is not so much of a team, as there is NREL, plus a reviewer, a subcontractor, and five suppliers.

Question 5: Proposed future work

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

- The proposed work is in line with the plan and recommendations from reviewers in previous years.
- The researchers have organized the work to meet project goals in a sequential and logical manner. In planning their future work, it will be important for researchers to address an analysis of sensitivity about cost/use with regard to data quality and relevance.
- The future work is very reasonable.
- The researchers have foreseen the disaggregation of the data into sub-technologies and applications, and they are also addressing the need for more data, although it is not clear what the probability of success will be in establishing such partnerships for data collection.

Project strengths:

- The project is leveraging a large pool of data that has been collected at NREL. It makes strong and effective use of NFCTEC and the NREL fleet analysis tool. The project is well focused on key barriers to expand and understand the use of stationary fuel cells.
- The project is necessary and will bring to DOE some data about the performance of stationary fuel cells and how far they are from the commercial stage.
- This is a well-designed project with useful market data and analyses.
- The large number of California stationary power projects is a strength.

Project weaknesses:

- Greater data points are needed to make conclusions at more disaggregated technology/application levels. The researchers should provide feedback, based on the analyses, regarding technology performance areas that are behind and potential gaps to feed into the present and future orientation of the Program.
- Currently, the project does not address metrics of reliability in the collection and interpretation of data. It is also limited by having access to only California data. The researchers do not make clear how they control for and/or address small data problems, particularly in the process of data aggregation (that is the effort to produce composite data).
- More information is needed to clarify the related market segments in the United States for stationary fuel cells. If necessary, information from literature should be sought and a better aggregation of data should be performed. The average cost per kilowatt is not credible (already below the target of DOE), while the electrical efficiency is very low (27%).

Recommendations for additions/deletions to project scope:

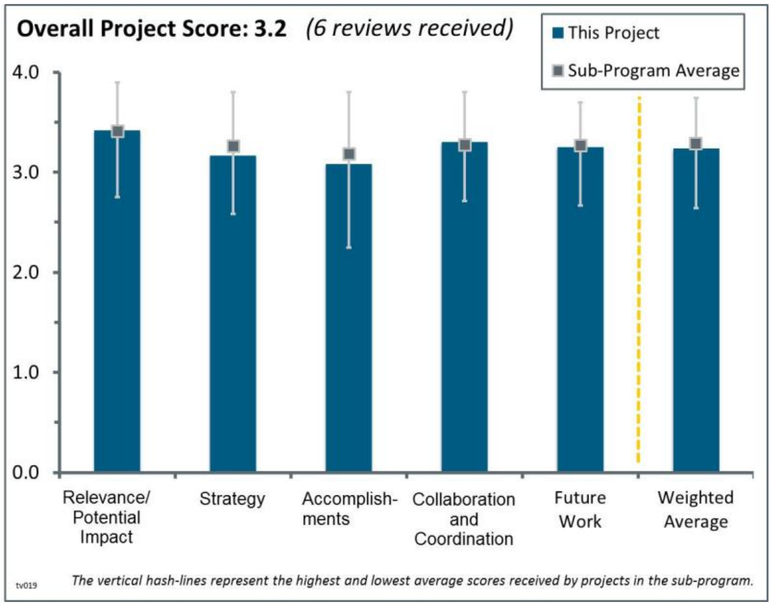
- More data should be analyzed, and a better aggregation/sensitivity analysis should be performed. As a possible usage of data, scenarios should be looked at and forecasts should be produced.
- More state partners are needed to provide geographic variability. In the data collection design, it is important to consider research hypotheses that will be explored using these data. It is still not fully clear what these data will be used for. The researchers should spend some time with DOE and its California collaborators to make some "value of information" assessments with regard to what and how much data to collect.
- It would be nice if this project could be expanded to cover nationwide stationary power systems.
- Collection of more data (which the project is already doing) and feedback on the Program gaps and areas of focus needed to cover those gaps is recommended.

Project # TV-019: Hydrogen Component Validation

Kevin Harrison; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to: (1) perform highly accelerated life testing on hydrogen infrastructure components to reproduce failures on a shorter time scale; (2) correlate results to real-world usage with statistical methods; and (3) work with a manufacturer to improve designs and reduce downtime for air, dispenser, control electronics, and hydrogen compressor systems.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- Compressors are a key reliability issue in stations, and improved characterization of failure mechanisms is critical to improve performance. Available data is also lacking, so this is an important addition to the knowledge base.
- Data collected on hydrogen stations confirms that compressor failure is a leading contributor to downtime and maintenance requirements. Therefore, focusing attention and resources on understanding failure mechanisms—and determining corrective measures to extend compressor life and improve performance—is appropriate. Project objectives include both accelerated testing to determine causes leading to failure and working with a manufacturer to address causes and solve problems through improved design.
- Compressors are a key aspect of the refueling station, yet they are high cost and have inadequate reliability. Consequently, a technology validation project to better characterize and understand them, as well as lead to higher-reliability compressors for hydrogen application, is a worthy goal.
- This is a highly relevant project for distribution of hydrogen fuel to vehicles.
- The project is very relevant for addressing one of the main components, which is the source of more than half of total maintenance hours. If successful, it might have a big impact (however, it is difficult to judge the real impact because no estimation of cost reduction has been produced yet).
- It is critical to focus on validating compressors, which have a significant impact on system reliability.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.2** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- Presentation slides 5–10 provide extensive coverage of the National Renewable Energy Laboratory's (NREL's) approach to the project. Equipment targeted for accelerated testing is incorporated into NREL's Integrated Renewable Hydrogen System at the National Wind Technology Center. The compressor currently being tested is manufactured by PDC Machines. In order to reach failure more quickly, a recirculation loop has been added so the compressor can be run without producing hydrogen. The test compressor is well instrumented. Test data is shared with the Pacific Northwest National Laboratory

(PNNL) as input for its dynamic compressor modeling activity. That is good. Slide 9 lists project targets, activities, and milestones. All in all, the project approach and test plan being implemented by NREL and PDC Machines seem logical and complete.
- The project is placing emphasis on compressor failure, the major component problem.
- Highly accelerated life testing is typically a good approach for device testing. Instrumenting of the system will be important to gather failure data. Using a high sampling rate and analysis for early indicators is a rational approach for a validation project.
- The strategy is good and follows the original plan; however, after only 100 hours of operation, out of the 1,000 hours planned, it is difficult to judge the possible results. The real target of the project is not clear either. No technoeconomic analysis of the total impact on the operation costs is provided so far. The approach is limited to only one technology; comparison with other existing technologies is missing.
- Overall, the strategy is worthy, but it has some weaknesses. Specifically, the "accelerated" test program is not accelerated by the normal definition of the phase. As currently structured, the plan is just to run the compressors continuously to amass hours of operation. Instead, what should be done is exploration of the specific failure modes of the compressor and then an intense repetition of those things that will make it fail. That would be a truly "accelerated" test.
- Detailed information on accelerated testing and the basis for selecting this approach is lacking.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The PDC Machines compressor has been installed and testing has been initiated. Slides 12–15 provide selected graphics and data related to test compressor performance. Slide 14 has an example of data being transferred to PNNL. The information provided is quite effective in making the case that the project is moving well toward achieving its objectives. However, significantly more test hours will be required before final conclusions about project results can be determined.
- Excellent progress has been made on evaluating compressors.
- A 6,000 psi accelerated testing compressor system is now operational, and a 12,000 psi test compressor system will soon be operational.
- The project follows the original plan and complies with the proposed objectives.
- It appears that the project is more than halfway through, and only now is preliminary pressure/energy performance data being collected. Even accounting for the upfront cost of the compressor, it seems like the project is progressing slowly with little (so far) to show for it.
- It is still very early in project: there are <100 hours of data. It is hard to tell how much progress has been made.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- There is good collaboration with PDC Machines on equipment and instrumenting. It is also good to see two laboratories working together with the addition of PNNL's analysis of data.
- Evidence of collaboration between PNNL and NREL exists, and the collaboration proved to be effective to predict possible failures.
- NREL's collaboration with PDC Machines is an important element of the project and seems solid.
- Information sharing with PNNL should result in significant additional benefits for the project.
- Two cooperative research and development agreements are in place with compressor manufacturers.
- It appears that the only compressor coordination has been with the project partner, PDC Machines. This may be appropriate, but benefits would be gained from discussion of failure modes with other (non-PDC Machines) compressor companies.
- Inputs from other component suppliers are desirable.

Question 5: Proposed future work

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

- The future work is nicely summarized on slides 17 and 18. The test plan is well designed, and project plans include extensive reliability analysis. In his oral presentation, the presenter noted that NREL would like more compressor models and types to be offered for testing. Testing could take place at multiple NREL sites, including the new Energy Systems Integration Facility. In response to a question, he stated that compressor testing like that at NREL may not be unique, but it is probably the only compressor testing that is integrated into a complete hydrogen production system.
- The proposed future work is appropriate.
- The future work looks quite reasonable.
- The proposed future work is in line with the original plan.
- A good test plan had been laid out. However, it is questionable whether a meaningful mean time between failures value can be obtained with the small sample set.

Project strengths:

- There is a clear project focus on compressor performance and reliability. The project has a well-structured approach to accelerated failure testing and use of test results. Collaborations with PDC Machines and PNNL are also project strengths.
- The project is addressing the improvement of one of the components that requires most of the maintenance hours during operation; therefore, if improved, it can have a very high impact and reduce maintenance costs.
- The project addresses a key area that has had little transparency in the past.
- Development of compressor accelerating testing would benefit the entire hydrogen community.
- Accelerated testing and data analysis are the project strengths.
- This is a well-focused project.

Project weaknesses:

- There is no specific mention of a plan for sharing and dissemination of test and analytical results, other than with PDC and PNNL. Slide 19 includes a general statement on technology transfer.
- A better definition of direction and project metrics is desired.
- The accelerated compressor testing plan is not really accelerated.
- It is not clear if it will be possible to reduce the maintenance time and associated costs. Moreover, it is not clear if this can be applied to the other technologies.

Recommendations for additions/deletions to project scope:

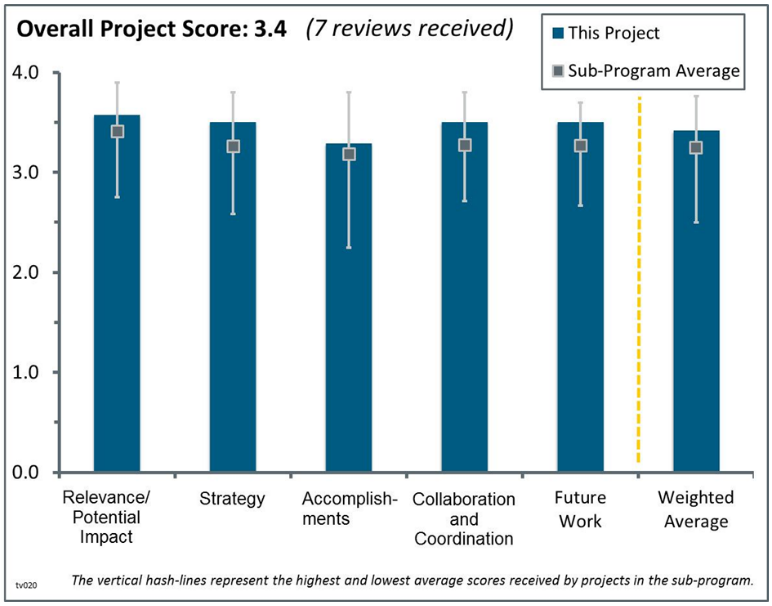
- DOE should work with NREL, compressor manufacturers, and other stakeholders to identify additional compressor models and types for inclusion in a future—and perhaps expanded—project. Test results achieved during the next year should inform a decision on continued support for similar testing of additional and redesigned compressors.
- Technoeconomic analysis of the impact of the project should be provided. NREL should explore the possibility of developing a “generic” tool that can be further used by the other technologies.
- The project needs to devote more attention to defining the specific compressor failure modes. Only then can an accelerated test plan be devised.

Project # TV-020: Validation of an Advanced High-Pressure Polymer Electrolyte Membrane Electrolyzer and Composite Hydrogen Storage, with Data Reporting, for SunHydro Stations

Larry Moulthrop; Proton OnSite

Brief Summary of Project:

The objectives of this project are to: (1) save up to 8 kWh/kg hydrogen compared to a commercial 30 bar polymer electrolyte membrane (PEM) for advanced PEM membrane electrode assemblies (MEAs), (2) save up to 3.6 kWh/kg hydrogen compared to 30 bar hydrogen supply for an advanced 57 bar PEM water electrolyzer, (3) double the usable storage per unit volume compared to first-generation storage tubes for advanced composite hydrogen storage, and (4) collect and report SunHydro station performance and technology reliability data.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- The project is very relevant to a crucial need for infrastructure. The principal investigator's (PI's) presentation was exceptional: clear, direct, and complete. It was a complete and thorough presentation of a project, presenting a useful and needed data set of knowledge to advance the critical challenge of hydrogen refueling infrastructure. In the presentation, Proton OnSite states that it expects the U.S. Department of Energy (DOE) to compare and contrast Proton OnSite's evaluation to data from other projects and technologies. That comparison might be worthwhile.
- This project is extremely relevant. It aims at the important cost and infrastructure barriers to practical hydrogen fueling stations via on-site, high-pressure electrolysis and improved high-pressure composite storage.
- The project aligns well with DOE objectives and has the potential to lower operational, as well as capital, costs.
- The project fits well with DOE goals to decrease the hydrogen production costs for PEM-electrolysis-based fueling stations by improved electrolyzers and optimized system components.
- The project directly addresses the barriers outlined in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. The integrated approach of using other fuel cell successes in the cell stack is nice.
- Clearly, if this project can hit the cost, permitting, safety, reliability, and performance targets, it will move stakeholders much closer to behind-the-gate fast-fueling for fleets. However, if the design does include containerized high-pressure hydrogen storage, there would be concern about unplanned releases. If there is an unplanned high-pressure release, the team would want to have the hydrogen quickly vent away before a major upset could happen. There will be controlled venting, but it is not clear whether that will provide the right level of safety. Also, this concept would probably not play out at a forecourt, but it is a good initial step.
- Because SunHydro already has a fuel cell electric vehicle (FCEV) fleet and operating stations, it is in a good position to provide real-world data on vehicle fueling.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.5** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The project's strategy is practical and excellent. It aims at the cost and infrastructure barriers to practical hydrogen fueling stations via on-site, high-pressure electrolysis and improved high-pressure composite storage. A key deliverable will be real-world operating data, especially cost data, hopefully within the $2–$4/gge DOE target range. The project is indeed well designed, feasible, and integrated with other efforts to achieve a real-world and highly instructive validation analysis. It includes all-important codes and standards considerations.
- The team is building a better cell stack to use 57 bar gas—then the compressor will be more efficient. It is good that safety, codes, and standards (SCS) are part of this project. Data collection on the SunHydro station is a plus for this project. Making equipment more efficient results in improved costs.
- Proton OnSite's presentation of specific improvement metrics was well received and appreciated. Proton OnSite offered a detailed discussion and approach to improving high-pressure fueling.
- The approach for cost reduction by integration of the new PEM stack technology and new advanced composite hydrogen storage vessels seems to be a good solution to reach the hydrogen cost targets.
- The project approach is solid and has decision points that are well thought out. The goals and objectives align with DOE targets.
- The U.S. Department of the Navy's high-pressure electrolysis unit does offer proof of concept; however, because this unit exists, it seems that there would be more details around cost targets. Also, Proton OnSite has known for some time that the voltage reduction target was a challenge, yet there was no information on any new approaches to address this issue.
- The project seems to be a reasonable approach to evaluating electrolytic hydrogen stations.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- This is an extremely complete and impressive project, with extensive accomplishments and progress. During the Proton OnSite presentation by the PI, it became clear that not only was the presenter prepared for the DOE Hydrogen and Fuel Cells Program Annual Merit Review presentation, but the focus of Proton OnSite on its project task was remarkable, complete, and commendable. The presentation of its data seemed to answer every possible question before it was asked. The project was outstanding across the board.
- The team has completed the cell stack and proven it can produce gas at 57 bar. The team has six new storage systems. The project had some delays, but it was worth it. The compression, storage, and dispensing container will finally be plumbed. It will be 40 feet long, and the team is making good headway with it. The data collection and analysis has been helpful toward achieving DOE goals to produce hydrogen.
- The project has made good progress so far. Data submissions from the first station are underway, and the second should begin data collection soon. The progress is reasonable, considering the project start date.
- So far, the project has made good progress toward providing a higher-pressure MEA electrolyzer (30–57 bar), compressor system, and improved composite storage tanks. Data acquisition systems are in place and operating for SunHydro-1 and almost in place for the improved SunHydro-2. The project is slightly, but not seriously, behind schedule in a couple of areas. There are no significant new problems detected. A contractor reported a seven-month delay in the delivery of six new 280–870 bar storage tubes but indicates that the end result should be newer and better technology.
- The team seems to be making reasonable progress on implementing higher-pressure electrolysis.
- The data presented are promising in terms of cost reduction. However, it would be helpful to have adequate data for the total energy consumption (kWh/kg hydrogen) of SunHydro-1.
- The late arrival of the cylinders, the slow spend rate, and the less-than-transparent progress on permitting serve as flags that some critical potential showstoppers need to be addressed.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- Anytime a project has good partners, such as Air Products and Chemicals Inc. (APCI), SunHydro, and Toyota, there is bound to be good collaboration. This project has great partners and is taking advantage of their expertise as well as their technology development.
- The principal collaboration is with SunHydro, a manufacturer of fueling stations whose vested interest is strong enough to provide cost sharing. FCEVs are provided by Toyota and storage tanks and associated instrumentation by ACPI. This is an excellent working collaboration of real-world partners.
- While Proton OnSite only had three other partners (SunHydro, Toyota, and APCI), the coordination between the four appears to be a true partnership, with each bringing its strength to the team. Proton OnSite does not appear to have added members to build its roster; instead, each team member brought its strength. It was suspected that this was a solid, high-performing team, and those results were evident in the presentation.
- The project has a strong combination of collaborators, suppliers, and other partners.
- The project seems to have good collaboration between the component suppliers.
- Collaborations are as expected, and the project partners are coordinating to meet objectives.
- The collaboration with Air Products, SunHydro, and Toyota seems routine.

Question 5: Proposed future work

This project was rated **3.5** for its proposed future work.

- This is an outstanding project that would be truly difficult to improve.
- The future work is appropriate and should proceed as planned. The validation and operating data derived should be valuable to DOE and all hydrogen technology shareholders.
- The team still has to complete Phase II and determine if it is a go/no-go project. The team will continue to collect operating data and improve the components. This project's future work will likely be successful and will help toward commercializing the technology at the station level.
- The future work plan seems to be well outlined.
- The plan seems reasonable.
- The future plans are as expected for the project.
- It looks fine, as long as there are no serious bottlenecks.

Project strengths:

- The project strengths include a good existing station base and FCEV fleet; an excellent company electrolyzer history, including high-pressure electrolysis; a good platform to demonstrate a low-cost, factory-built containerized hydrogen station; and the ability to minimize cost with appropriate, separate safety zones.
- Project goals include increasing efficiency—this should contribute to lowering overall costs. Reducing the station footprint will contribute to commercialization because it could facilitate adoption at more sites where space is constrained.
- The project is completely professional in all respects: well thought out, well executed, and well documented.
- The project can clearly show which components are able to reduce the overall electrical costs for electrolytic-produced hydrogen by PEM electrolysis for mobile applications.
- The project has an excellent real-word technology validation and partnership.
- This project will be useful and will likely result in tangible results for hydrogen infrastructure.
- The team of experts that will be working on the project, including APCI, gives some additional confidence that the system will be set up and monitored properly, albeit not likely cost effectively.

Project weaknesses:

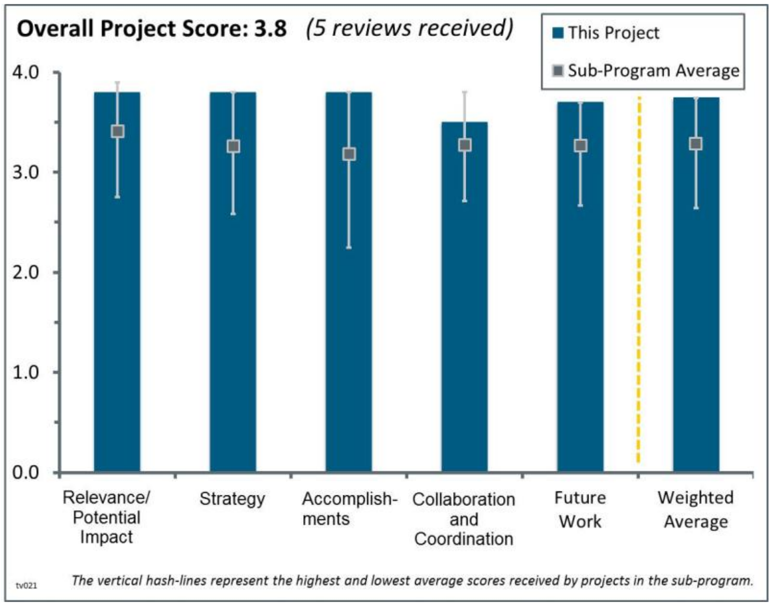
- There are no weaknesses.
- There are no weaknesses to identify.
- There are no visible major weaknesses.
- It would be good to see this project palletize the components, including all the safety features, in one pallet in order to enable ease of shipping and transportation.
- It is not clear whether the team is incorporating knowledge already gained from the previous technology validation and Clean Urban Transport for Europe (CUTE) projects, especially with containerized high-pressure hydrogen in International Organization for Standardization (ISO) containers. It would have been helpful if the team had given a high-level review of key findings during hazard identification analysis and hazard and operability analysis. The project needs a deep dive on costs.
- The team has not shown any propensity to conduct hydrogen cost estimation or determine the economic impact of installing higher-pressure electrolysis. The PI could not answer simple questions, such as the total energy requirement for electrolysis; all he showed was the net savings in energy in kWh/kg of hydrogen (and even here, he showed two estimates for savings—3.8 kWh/kg and 8 kWh/kg—and did not explain the difference).

Recommendations for additions/deletions to project scope:

- The team should integrate the two 40-foot containers showing all the required SCS. It is not clear whether it is possible to integrate all components into one pallet. The reviewer believes that Teledyne had a similar product, which it produced. The team should check this out.
- It would be nice to see more on how the station could be scaled up to meet a need for larger quantities of hydrogen as station load increases. It is not clear whether this would increase the footprint to a size where it would not be easily integrated into any site.
- It would be helpful to see the overall and single-component power consumptions of this project.
- Scale-up work is needed in due course.
- The team should add a task to estimate the full cost of electrolytic hydrogen (capital and operating cost) for the existing lower-pressure electrolyzer. It should then compare that with the estimated costs using the higher-pressure electrolyzer, as well as compare these costs with the DOE target of less than $4/kg.

Project # TV-021: Forklift and Backup Power Data Collection and Analysis

Jennifer Kurtz; National Renewable Energy Laboratory



Brief Summary of Project:

The objectives of this project are to: (1) assess fuel cell and hydrogen technology status in real-world operations, (2) establish performance baselines, (3) report on fuel cell and hydrogen technology, and (4) support market growth by evaluating performance relevant to the markets' value proposition.

Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- The American Recovery and Reinvestment Act (Recovery Act) provided significant resources to demonstrate fuel cell systems for material handling equipment (MHE) and backup power requirements. With this project, the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program) is leveraging that investment to ensure that fuel cells and the associated hydrogen infrastructure performance is well documented. The project also leverages the world-class data processing and analysis expertise resident at the National Renewable Energy Laboratory (NREL), which has been built over the years as part of DOE's support for vehicle and other fuel cell demonstrations. NREL's collection and analysis of data, and reporting of results, have made a vital contribution to understanding the status of technology development relative to DOE's goals and targets. NREL's data products have been continually refined, increasing their value for both government and industry decision makers. The detailed, objective results of NREL's work are readily understood by those making decisions on public and private investments in technology research, development, and commercialization. Results achieved by the team at the National Fuel Cell Technical Evaluation Center (NFCTEC), including those for this project, are an outstanding and integral element of DOE's support for hydrogen and fuel cells.
- It is wise to evaluate the numerous backup systems funded under the Recovery Act to determine their durability during operation in real-world commercial settings. This project is adding value to the commercialization of niche market hydrogen technologies. This project is therefore highly relevant to the Program goals.
- The project is well targeted and very beneficial to the research goals of the Program. The cost of ownership data are very valuable and well characterized in terms of reliability and uncertainty. The work is also very valuable to the overall Program in illustrating the effective use of sensitivity analysis.
- The project is clearly very relevant and is helping to address barriers to widespread near-term applications of fuel cells.
- Per the definitions associated with the scores, and combined with the nature and results of data collection for this project, compared to a project that directly advances and/or improves hydrogen infrastructure, fuel cells, or the like, it became difficult to assign a grade higher than 3.5 to this project. While measuring and analyzing forklift and backup power data is of great importance, projects that fundamentally advance the actual deployment of fuel cells and hydrogen infrastructure may have an advantage in this category.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.8** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The NFCTEC's approach to collecting, analyzing, and reporting real-world operational data is captured quite nicely in presentation slide 5. The approach reflects NREL staff's extensive experience gained by working on a variety of technology validation projects. With ongoing feedback from data providers and users, NREL has developed superb procedures and protocols. The result is an outstanding and constantly expanding collection of Composite Data Products (CDPs) and Detailed Data Products (DDPs). The milestone information on slide 4 is outstanding. The reviewer recommends this as a model for how milestones can be displayed for other projects being reviewed—and commended it at the oral presentation. Slide 3 has an excellent statement of project objectives.
- There was a good quarterly analysis provided and a final report on backup power systems. It is a good strategy for the NFCTEC to allow end users and academia to access the collected data and related analyses. Everyone will then benefit from such analyses. This is a solid approach to maximize the benefits of this project's results.
- The general goal of this project is to collect and analyze data that promote early commercialization of fuel cells in key markets, with a specific focus on forklift and backup power applications. This effort collects the data needed to address barriers to early commercialization and support efforts to assess the technology status in real-world operations, establish performance baselines, report on fuel cell and hydrogen technology, and support market growth. The work gives appropriate attention to the types of questions and metrics needed to confront these barriers. One of the challenges the researchers confronted was how a limited numbers of observations impacts the reliability and relevance of their results and recommendations. They have been very effective in addressing this issue by reporting confidence intervals and using a sensitivity analysis. This effort creates an important opportunity to use sensitivity analysis to set goals for future data collection.
- This project is a relatively routine data collection effort for numerous fuel cell backup power and materials handling systems in service. This is indeed the correct strategy. The data will be very useful in documenting and summarizing the owner costs and performance data relative to diesel and battery alternatives.
- There is little doubt that this team's presentation at next year's DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) presentation will be significantly improved. Given the presenter's professionalism and follow-up since the AMR presentation, it is a certainty that the presenter and her team will significantly improve a year from now. The principal investigator's (PI's) patience and professionalism during her presentation, her patience during the reviewer's persistent questioning during the question-and-answer session, and her extraordinary post-AMR follow-up were much appreciated.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.8** for its accomplishments and progress.

- The 32 backup power analyses were outstanding. The amount of work performed was outstanding, namely, 252 of the 852 operational systems were evaluated. The project also completed analyses on backup power that clearly show that the cost of fuel cell backup power systems is equivalent to that of backup power systems powered by diesel. The variable costs include the permitting process—this piece of information is not trivial. Fuel cell systems with incentives are comparable with diesel-based systems, with up to 72 hours of runtime. Researchers found that 94% of the fuel cell backup systems had no interruptions in operation or start-ups. This is a significant finding. The results on hydrogen safety for forklift operations sound really valuable and should be more widely published. Fuel cell forklifts have demonstrated more than 2 million hours of operation, using more than 275,000 kg of hydrogen safely. These same forklifts had 10,000 hours of operation with less than 10% degradation in power output. These are outstanding accomplishments.

- The presentation has 14 slides that show project accomplishments. They are packed with information on the number of fuel-cell-based MHE and backup power units deployed; backup power operations, performance, and cost; fuel cell MHE operation and performance; and hydrogen infrastructure use. The project has produced 32 backup power CDPs and 75 CDPs related to MHEs and the associated infrastructure. A report, "Backup Power Cost of Ownership Analysis and Incumbent Technology Comparison," has been published. The results of a sensitivity analysis, relevant to the cost of ownership, are summarized in the presentation. Metrics studied for sensitivity include capital cost, installation cost, discount rate, operational life, maintenance cost, and fuel cost. Project accomplishments associated with MHE include results on fuel cell voltage degradation, hydrogen fueling station usage, and the contribution of selected infrastructure equipment to maintenance requirements. This project provides the most objective, comprehensive, and independent indicators of fuel cell progress for MHE and backup power applications. All in all, the project is a good deal for about $200,000 annually.
- The project is well focused on key barriers to understand and expand the use of early market fuel cells. The project is on budget and on schedule. The researchers have made very good progress with regard to both the specific project goals and the broader goals of the Program. The work is up to date with respect to all project goals.
- Substantial quantities of data have been collected, analyzed, and reported to date. In general, the results are positive and will help DOE in promoting early fuel cell applications. The low in-service fuel cell degradation levels are especially encouraging. The cost disadvantages of battery storage were clearly quantified.
- While the presenter and her team are performing a necessary task, their approach and findings are questionable. For example, their presentation of data on page 9 of their presentation appears to mix both actual cost data with projected cost data. While both sets are probably valid, mixing the actual cost data with projected cost data without delineation between the two should be avoided. Furthermore, the chart appears to be seeking a result rather than reporting a result, because the chart does not account for refueling, indicating that the cost of fuel for fuel cell and diesel systems is minimal for two, three, or seven or more days. The chart should have included the additional and full cost of delivering additional fuel in an emergency and the near impossibility that could occur for certain instances (e.g., lower Manhattan 3–7 days after a 9/11-type event). If a cost curve that demonstrates that the cost of batteries would be ridiculously expensive after a certain point is going to be generated, the truth is that fuel cells and even diesel generators may equally not make economic sense. There were also other problems with other data on other charts.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- As indicated on slide 20, this data project has involved collaboration and routine communication with fuel cell users, fuel cell manufacturers, and hydrogen providers. The project has also contributed to safety and risk assessment initiatives. NREL's data collection and analysis team has earned the trust of all organizations participating in the Recovery Act and other demonstration projects. Contributing factors include ongoing communications, opportunities for input and feedback on the process, and NREL's system for the protection of sensitive and proprietary information. Excellent communications are maintained with many stakeholder organizations, both in the United States and internationally. Slide 28 (reviewer only) has an impressive list of publications and presentations since the 2013 AMR.
- The list of partners and collaborations to date is outstanding.
- The presentation has demonstrated good links to collaborating partners. The analysis is well informed by the questions that are most relevant to technology validation. This came about through thoughtful planning and through listening to and interacting with partners.
- Collaborations are widespread and apparently excellent. The result is excellent real-world data collection and analysis.
- While collaboration partners were listed, collaboration could be improved—little discussion detailed the collaborators' roles.

Question 5: Proposed future work

This project was rated **3.7** for its proposed future work.

- The final report and project closeout will be highly anticipated by everyone involved and for early market adopters.
- Presentation slide 21 clearly describes work to be done during the coming year. A final report on backup power will be completed, as will final CDPs for MHE operations. The PI stated that the project will be completed with the winding down of the MHE and backup power fuel cell demonstrations funded by the Recovery Act. Slide 22 states that MHE validation will continue with data voluntarily supplied by industry collaborators.
- The researchers have organized the work to meet project goals in a sequential and logical manner.
- The project is almost finished. The proposed lists of final work for fiscal year (FY) 2014 and FY 2015 are fine.

Project strengths:

- The project's strengths are the following: the experience and expertise of the NREL project team and project leader, the active collaboration and interaction with manufacturers and users of fuel cells for MHE and backup power applications, the quality of information in reports published by the project, and the continuous improvement of CDPs and DDPs. This is a solid contribution to fuel cell and hydrogen infrastructure progress for a relatively small expenditure of total Program resources.
- The researchers are very effective in both meeting project goals and in providing results that are relevant to Program goals. The researchers were also very effective in identifying and evaluating key assumptions.
- There is good real-world data and analysis for early market fuel cell applications of backup power and MHE. These are likely to be the first widespread commercial applications for fuel cells.
- Outstanding sensitivity analysis is provided on slide 12 that helps to evaluate various options for backup power.
- The PI appears to be more interested in being accurate and getting things right versus appearance.

Project weaknesses:

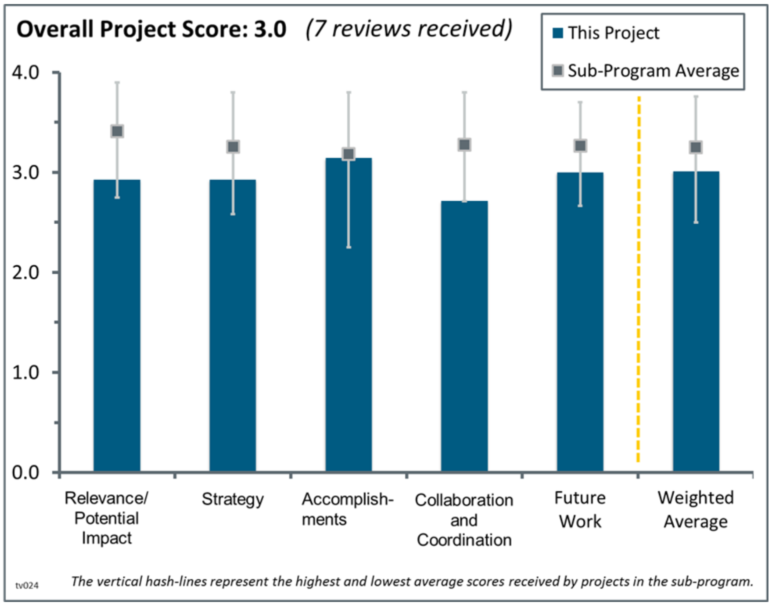
- No project weaknesses were identified.
- Mixing data types to prove a point is a weakness of the project.
- It is not yet fully clear how future research will be used to guide data collection in ways that reduce uncertainty, increase reliability, and address the data that are most sensitive to presented findings and conclusions.

Recommendations for additions/deletions to project scope:

- This project is expected to be finished in conjunction with completion of the Recovery-Act-funded MHE and backup power projects. It is recommended that DOE and NREL investigate whether industry would continue to provide data voluntarily on operations of MHE powered by fuel cells, as well as on current and new installations of fuel cell backup power systems. DOE should encourage industry to collaborate with NREL on continued data collection, analysis, and reporting.
- The team should consider including qualitative verbal comments from operators, especially those who have experience with diesel and battery systems.
- It is still not fully clear how these data will be used for informing decisions at DOE and other entities. The researchers should spend some time with DOE and project collaborators to make some "value of information" assessments with regard to what and how much data to collect.

Project # TV-024: California State University, Los Angeles, Hydrogen Refueling Facility Performance Evaluation and Optimization

David Blekhman; California State University, Los Angeles



Brief Summary of Project:

The objective of this project is to test, collect data, and validate hydrogen refueling architecture deployed at California State University, Los Angeles (CSULA) and its individual components in a real-world operating environment. The performance data will be provided to the National Fuel Cell Technology Evaluation Center at the National Renewable Energy Laboratory (NREL).

Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.9** for its relevance/potential impact.

- The CSULA project aligns with and meets the goals of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program), as well as DOE research, development, and demonstration (RD&D) goals, but the advancement of progress can best be described as incremental rather than significant. The principal investigator (PI) and the project presented some improvements and are deserving of recognition. However, when the PI was asked if the project offered evolutionary or revolutionary improvements, he confirmed that it offered "gradual" improvements, and the PI may have a point—that the advancement of hydrogen and fuel cells needs both evolutionary gains and also the less-spectacular "gradual" gains of projects such as this. This PI did a good job on the project and a better job on the response because the PI was right—not all necessary gains will be spectacular. Often, the very necessary, if not the key advances, will be gradual rather than dramatic.
- In theory, the potential seems to be almost outstanding; however, without a clear technoeconomic estimation on the possibility for cost reduction following this project approach, the impact cannot really be measured.
- The project fits well with DOE goals and can help to identify optimization potentials for components of electrolysis-based fueling stations.
- Evaluation of hydrogen station performance is important for the deployment of more stations and fuel cell electric vehicles (FCEVs).
- The team's expectation for thousands of FCEVs in California in 2015 seems overexaggerated. This kind of integrated project, where hydrogen production and dispensing are combined, is excellent, and the educational aspect is very practical as a side benefit. The main areas of work include (1) integrating hydrogen stations with smart grid technologies, (2) "full scale hydrogen station operating in real life," and (3) educating students. The operating data will be collected and sent to NREL. The safety aspects of this project, such as first respondent training and education, are also good.
- This project has a weak alignment with DOE goals and targets. The presentation does not outline specifics for how the project will address barriers and help further the development of hydrogen station technology.
- It is not clear how this project will have impacts on reducing the costs of hydrogen production and delivery.

Question 2: Strategy for technology validation and/or deployment

This project was rated **2.9** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- This project has an excellent design, and its phased approach allows for go/no-go decisions. Phases II and III and Tasks 4–7 look reasonable. This project is slated to produce tangible results to improve station performance.
- With the view that improvements are not always spectacular and that CSULA's approach was academic and methodical, the reviewer believes that the CSULA project effectively contributes to the Program, as well as DOE RD&D goals. With that, and because of the PI's answers during the question-and-answer session, the reviewer would have preferred to have graded this section as a 4.0 but could not determine a way to fairly do so.
- The approach for cost reduction by complete systems validation seems to be an effective tool to identify the weaknesses of single system components of hydrogen fueling stations based on electrolysis.
- The approach seems reasonable.
- The project does not provide sufficient detail on what is being done to help meet goals and targets for the technology. There appears to be no defined plan for optimizing the station to increase efficiency or address costs. The plan for data collection and submission is solid.
- A detailed description of the approaches employed in the work (e.g., data acquisition) is lacking.
- The approach misses details on grid connection (e.g., it is not clear if there is any smart grid approach) and clarity on which impurities of hydrogen are to be addressed.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The project can produce up to 60 kg per day with pressure up to 10,000 psi. The project can fuel 15–20 FCEVs per day. The project has power meters that help the control interface. These power meters help the flow of hydrogen to the compressor. The successful installation of components to collect data is set, and progress on this project looks excellent. The project can generate data over time to determine the performance of compression and dispensing components. The team installed buffer tanks to take care of car manufacturer concerns. The team expressed thanks to The Linde Group for its excellent work in this area. A hydrogen purity unit was purchased at a cost of $512,000. This is a huge investment by the project developer—CSULA. Dispensing meter testing is being demonstrated. The team hosts students and foreign visitors to look at this demonstration project. This is helping DOE to achieve its goal of education of hydrogen technologies.
- The CSULA project aligns with and meets the goals of the Program, as well as DOE RD&D goals, but the advancement of progress can best be described as incremental rather than significant. The PI and the project presented some improvements and are deserving of recognition, but when the PI was asked if the project offered evolutionary or revolutionary improvements, he confirmed that it offered "gradual" improvements, and the PI may have a point—that the advancement of hydrogen and fuel cells needs both evolutionary gains and also the less-spectacular "gradual" gains of projects such as this. The PI did a good job on the project and a better job on the response because the PI was right—not all necessary gains will be spectacular. Often, the very necessary, if not the key, advances will be gradual rather than dramatic. With that perspective, the CSULA project appears to have clearly been focused on the systematic collection, validation, and testing of hydrogen refueling infrastructure in order to advance the hydrogen economy. Compared to other presentations/projects, CSULA's adaption of equipment and equipment improvements appears less impressive, but it was its academic approach and results gathering that were impressive, worthy of significant grading, and worthy of continued effort.
- The data collection and automation is a great start. The project needs to outline the plans and objectives for optimizing the station. At this point, it seems to be random research and not focused with specific goals.
- There seems to be reasonable progress to date.
- From the presentation, it looks like the project is on track (as originally planned).

- A hydrogen fueling station has been established.
- There is not enough data presented to say something about the progress of the overall project.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.7** for its collaboration and coordination.

- The team has excellent partners, including the California Air Resources Board, AAA, DOE, and the California Fuel Cell Partnership (CaFCP). CaFCP is essential in providing its long history of expertise in developing such a hydrogen station.
- The project collaboration with California Weights and Measures is good and will contribute to allowing the sale of hydrogen as a fuel.
- This reviewer cannot justify more than a satisfactory grade (2.5) for collaboration—CSULA's only stated partner was Hydrogenics, and CSULA seems to have performed the work, as stated in the presentation. However, the reviewer is not sure how this project could have been done differently, given its nature and its great and necessary accomplishments.
- The collaboration with Hydrogenics, the electrolyzer supplier, seems ordinary. The team should be reaching out to other organizations in the arena of hydrogen purity; for example, coordinating with government agencies and possibly others measuring hydrogen purity.
- Collaboration with other organizations that have developed hydrogen fueling stations is strongly recommended.
- There are no noticeable collaborations with other institutions and industrial partners such as Hydrogenics.
- The collaboration with the University of California, Los Angeles (UCLA) is not clear (some steps have been taken, but there is no formal commitment in this respect).

Question 5: Proposed future work

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

- The proposed future work is 100% outstanding—CSULA appears very sharply focused on performing the necessary grunt work that is important to advancing the hydrogen economy.
- The plans for future short-term and long-term work are excellent. Smart grid and load shedding are areas the team wants to study in the future. The emphasis on continued data collection and educational outreach is essential and important.
- The future work plan seems to be well outlined.
- The proposed work is again in line with the plan.
- The team hints at a possible project to evaluate the utilization of intermittent renewables, such as wind. This would be valuable, but the team did not show how it might accomplish such a project without direct access to wind turbines. Because it has three hydrogen compressors, it would be beneficial if the team could assess the reliability (or lack thereof) of its compressors, but the team may not have the capability for compressor testing.
- A more detailed description of future work with the focus on meeting the project objectives is recommended.
- The proposed future work is poorly described. It is unclear what, if any, progress will be made toward moving the technology to commercial readiness.

Project strengths:

- The project strengths include the educational outreach, collaboration with CaFCP, and establishment of high-quality components in operation with data collection.
- The project provides data on another hydrogen station, which will be valuable for DOE's technology validation activity. Education and awareness of the public will be important for future adoption of hydrogen.
- The project addresses one of the main issues to be solved before going into commercialization and real deployment of fuel cell and hydrogen technologies; therefore, the impact can be very high.

- Project strengths include CSULA's laser-like focus on the task at hand, and the incremental advancement of technology validation. While a great leap forward would have been desirable, the PI correctly pointed out that progress is not always spectacular, and that when it is incremental, one can do no more than only report the incremental, but necessary, progress.
- The project seems to have good instrumentation capabilities. The project has an excellent opportunity to educate and expose new students to hydrogen and fuel cell technology.
- The project can clearly show which components are able to reduce the overall electrical costs for electrolytic-produced hydrogen by alkaline electrolysis for fueling stations.
- A hydrogen fueling station was established.

Project weaknesses:

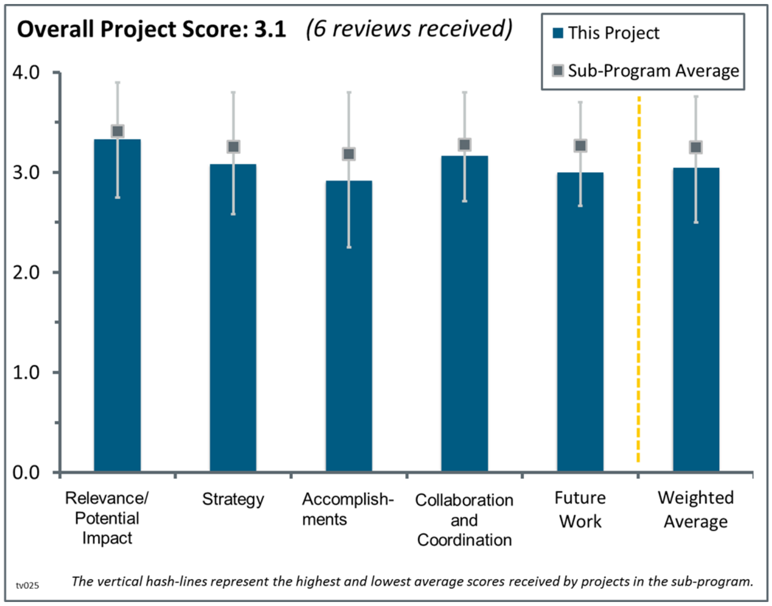
- There are no weaknesses to identify.
- Beyond the apparent absence of greater collaboration with partner organizations, no weaknesses were noted.
- The project needs more emphasis on SCS.
- Without a technoeconomic plan to assess the economic advantage of the proposed solutions, it is difficult to judge if the approach is correct and to quantify the benefits.
- There are unclear project objectives and metrics.
- The team does not seem to have any idea how to reduce the cost of the station; because this is a university, it needs to release some of that creative academic talent on analyzing the station cost and suggest avenues to reduce costs in the future.
- The project is not well defined and does not have measurable goals for meeting technical targets. There are no apparent plans to address cost barriers. The project presentation did not include some required elements—specifically, it did not address comments from past reviewers.

Recommendations for additions/deletions to project scope:

- The team should work more closely with other California state universities, such as Irvine, Fullerton, San Diego, and Santa Barbara, and private colleges in Los Angeles, such as the University of Southern California.
- It would be helpful to see the overall and single-component power consumptions.
- A technoeconomic analysis of the proposed solutions should be introduced. Clarification and use of possible collaboration with UCLA should be better addressed.
- The team should add a task to analyze station cost and make creative suggestions about how to reduce capital and/or operating costs.
- The researchers should review the goals and technical targets in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan, as well as develop specific, measurable goals and objectives for addressing barriers and meeting technical targets. Further funding would not be recommended until this project demonstrates that it has value other than for educating the current student body.

Project # TV-025: Performance Evaluation of Delivered Hydrogen Fueling Stations

Michael Tieu; Gas Technology Institute



Brief Summary of Project:

The objectives of this project are to: (1) integrate largely nonintrusive data collection systems at five 100 kg/day delivered hydrogen fueling stations located in California for a 24-month period, (2) submit station data specified in the National Renewable Energy Laboratory (NREL) Hydrogen Station Data Templates, and (3) provide useful data to accurately characterize stations' performance.

Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- This project's relevance is outstanding. What the team is proposing to do—namely, develop a data collection regime at five existing stations—is good. This requires analysis of permitting, construction delays, and integrated fueling equipment.
- Acquisition of accurate data on "real-world" hydrogen stations is important. Such data—if properly collected, organized, and analyzed—can contribute significantly to understanding actual station performance relative to the U.S. Department of Energy's (DOE's) development targets for hydrogen fueling systems.
- The project is highly relevant to the optimization of hydrogen delivery stations.
- The project is expected to provide valuable data on hydrogen stations to validate the technology and better understand performance.
- It is impossible to tell from the information provided the extent to which this project will be successful in delivering the expected results, because no actual data collection has begun yet. It is expected that the data will start to be collected during the fourth quarter (Q4) of 2014 for the first station. Thus far, only the design and drawings have been completed, and some of the equipment has been ordered/received.
- It is not clear why a third party has to come in to measure station performance. It is not clear why the project does not rely on The Linde Group (Linde) to report on its own progress.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.1** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The team's strategy for this project is outstanding. The phased approach looks reasonable. The team expects to collect data at five fueling stations in California in a 24-month period. Then the team will share the data in the way NREL expects it, which will result in outstanding benefits to the progress of the technology.
- Five seems to be a reasonable number of stations for which to collect data. This will enable comparisons across stations to determine the degree of consistency in station performance, better understand factors that

affect performance, and identify issues requiring further development. The station data to be collected and submitted will be as defined in NREL's Hydrogen Station Data Templates. It will be provided to the National Fuel Cell Technical Evaluation Center (NFCTEC) at NREL for storage, processing, and analysis. This link to NREL is vital to the achievement of project goals and for the comparability, objectivity, and confidentiality of results. The approach to the project, summarized on presentation slides 4–6, and the brief task descriptions seem straightforward and reasonable. Based on the information on slide 5, the $800,000 budget (slide 2) is sufficient only for collection of data on two stations. During his oral presentation, the principal investigator (PI) stated that Linde has obtained the funding for Budget Period 2 projects. The reviewer assumes that the $400,000 from DOE will be sufficient for data collection from all five stations. If that assumption is not correct, a clarification is needed. A slide showing the project's original and current milestones would enhance the section on project approach and strategy.

- The team seems to have a reasonable approach to monitoring the liquid hydrogen stations.
- Data collection will be unobtrusive to the Linde station operation.
- The presenter has not indicated what barriers the project is addressing, instead indicating the barriers that the project is facing. This should be corrected for the next review. Presumably this project is addressing Barriers C, D, and E. No details are given as to the performance parameters that are being measured/validated. No connection is made between the work that is being done and the goals of the DOE Hydrogen and Fuel Cells Program (the Program). It is not clear why the project chose to base the entirety of its activities on newly constructed stations (which have a lead time), rather than using, even partially, already existing stations to test the data collection systems and generate data during the construction phase of the new stations. There is potential for the project to contribute to Barriers C, D, and E, but it is unclear whether the approach of using only new stations is the most efficient. The project appears to be feasible, however. The project proponent should ensure that the way in which the project addresses technical barriers to facilitate proper evaluation of this metric is directly addressed.
- The barriers listed in the overview slide do not align with those identified in the Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan (MYRDDP). While the barriers listed could pose a problem for hydrogen stations, they are not the primary focus of DOE. It is unclear how the project will address those barriers or how the solutions will be shared with the industry.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- As described in presentation slides 7–11, progress has been made during the Budget Period 1 (BP-1) work. Accomplishments include station permitting, initiating the construction bid process for a station, completion of station data acquisition system design and drawings, and acquisition of instrumentation and data logger components. The small amount of project funds spent during the first year (slide 2) was noted and questioned. The PI stated that permitting for the station planned at San Juan Capistrano has been held up. In response to a question, he said that a no-cost extension has been requested, to reflect a six-month delay in completing actions required to provide an operational station.
- The project developers recognized that the permitting processing in the state of California took longer than expected. This is surely part of the lessons learned. This has been the case in many other technology validation projects and needs to be documented well. There has been excellent planning and accomplishments with regard to the purchase, installation, and testing of the instrumentation and data logger components needed for this project.
- Accomplishments are reasonable, considering the time from project start.
- Progress during the past year is good, but one would have liked to have seen more. Perhaps this was due to issues outside the control of the investigators. It is not clear whether the initial data collection will be finished in BP-1, as per the go/no-go.
- It is not possible to evaluate this metric, because no results have been generated from the project yet. The first sets of results are expected during Q4 2014 for the first station.
- The team has just installed monitoring equipment, so it is too early to judge the accomplishments.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The Gas Technology Institute (GTI) and Linde have a good relationship with well-designed and delineated responsibilities. This will make for a good partnership. Linde is the subcontractor on this project, and it will likely perform, as usual, in an outstanding manner. GTI is an incredible organization and its results from this project are something to look forward to.
- Project partners GTI and Linde are well qualified to carry out the project. Slide 14 nicely summarizes the respective roles and responsibilities of GTI and Linde. The quality of collaboration and communication between GTI and NREL will be a key factor in the smooth functioning of the project and reporting of results. There was no mention in the presentation about collaboration/communication with California state and local government agencies. Slides 12 and 13 provide a glimpse of GTI's and Linde's capabilities and experience. However, they do not provide information that is particularly relevant to the project being reviewed.
- The project includes good collaboration so far. Project partners are working together to meet objectives.
- Collaboration between the project partners appears to be good and well-coordinated.
- The project appears to have a good collaboration with Linde.
- The team seems to be working with Linde and the California Weights and Measures representatives to resolve the issue of selling hydrogen by the kilogram.

Question 5: Proposed future work

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

- Finalizing component integration and data collection plans at the last three stations, which Linde has already planned, looks reasonable and indicates how reliable this project will be going forward. A no-cost time extension was approved for this project.
- The proposed future work is expected for the level of effort.
- The proposed future work seems like routine monitoring work.
- The future work should produce more rapid progress with data collection.
- There is no apparent strategy for risk identification and mitigation. There is no identification of go/no-go decision points.
- The work to be done during the remainder of 2014 is summarized in slide 15. The presentation should provide more information on plans for the remainder of the project. It should include, for example, at least a few major milestones, such as the months in which data will begin to be delivered to NREL for each of the five stations. The lack of detail on future work could cause a reviewer to conclude that the plans are not yet developed and that time frames for completing activities are not settled or important.

Project strengths:

- The partnership between GTI and Linde is a huge plus for this project. The collaboration brings an incredible array of talent and experience to the table for such a project. Outstanding results can be expected.
- The project adds data from three more stations to DOE technology validation efforts. This is important for validating the performance of station components, including compressor technology and other components.
- GTI and Linde are well-qualified organizations. They have the expertise and experience to successfully accomplish the project goals. Project data will be submitted to the NFCTEC. Among the states, California is a leader in promoting and investing in hydrogen infrastructure.
- The project has the potential to contribute to data collection and validation. It expands the network of fueling stations.
- There is good data monitoring and instrumentation experience.
- The collaboration with Linde is a project strength.

Project weaknesses:

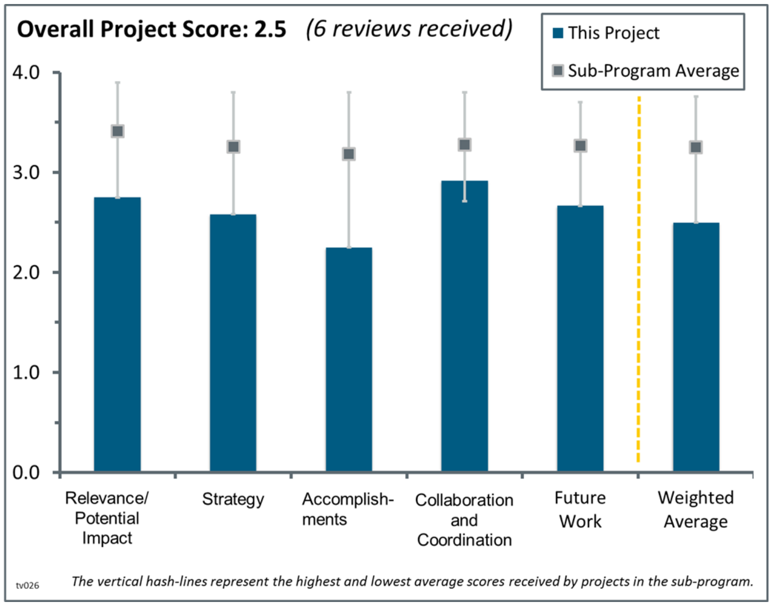
- The project timeline is dependent on factors outside the control of the investigators.
- The project goals do not appear to include addressing cost targets.
- There is a lack of information on original and current project milestones. It would be helpful to have a better understanding of the factors considered in selecting sites for the stations that will be included in the project and what organizations GTI and Linde must coordinate with in making decisions on station locations.
- The lead times for the construction of new hydrogen refueling stations as well as delays in the construction (six months) have meant that no project results are available to date. The project does not demonstrate its relevance to the Program by highlighting its goals and achievements in relation to goals and technical barriers addressed by the Program. There does not appear to be risk planning, which could affect the feasibility of the project.

Recommendations for additions/deletions to project scope:

- Decisions on changing scope should wait until after data is being successfully acquired and submitted for at least a few stations.
- If possible, the researchers should report on the performance of the Linde ionic compressor. They should also determine why Linde would use the ionic compressor instead of a liquid hydrogen pump to provide high-pressure gaseous hydrogen.
- Project partners should review the FCTO MYRDDP to ensure that the goals align with technical barriers and targets. The project intends to address the barriers of permitting and construction delays, but it does not outline how this information will be shared. To ensure that the learnings from the project are shared, the project partners should develop a plan for documenting this for the industry.
- The project should evaluate whether it will be feasible to implement all foreseen stations within the specified time, given that a period of data collection of two years is designated, and given the delays in the construction in the hydrogen refueling stations.

Project # TV-026: Hydrogen Fueling Infrastructure Research and Station Technology

Brian Somerday; Sandia National Laboratories



Brief Summary of Project:

The long-term objectives of this project are to: (1) reduce the cost of hydrogen fueling stations to be competitive with conventional liquid fuel stations; (2) improve the availability, reliability, and cost of high-pressure components while ensuring their safety; (3) focus a flexible and responsive set of technical experts and facilities to help solve today's urgent challenges and the unpredicted needs; and (4) enable distributed generation of renewable hydrogen in a broader energy ecosystem.

Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.8** for its relevance/potential impact.

- The project's objective is articulately stated as ensuring a positive fueling experience, and thus covers all aspects that could be a problem. The structure of this project facilitates a positive outcome, through execution, communication, and follow-through.
- This is a highly relevant project. This is a key part of the H2USA partnership.
- It is still very early for this project, so there are relatively few concrete achievements to date, but the scope is meaningful because the project seeks to address real-time technology performance and operation problems related to hydrogen storage and hydrogen refueling stations, while gathering data for these for technology validation.
- This project is too new to evaluate. It has the potential to contribute to the deployment of hydrogen stations, but the team did not provide sufficient information to judge its potential.
- Almost any project related to advancing hydrogen fueling infrastructure has to be good, and while this project in particular appears to fully align with U.S. Department of Energy (DOE) goals, there was almost no discussion on how the Sandia National Laboratories (SNL)-National Renewable Energy Laboratory (NREL) team would meet the challenge. It appears the team's time was spent on the fact that there was a relationship between the two organizations, instead of on the how and what. For example, slide 11 advises that a laboratory was brought in to the project, but there is no discussion on that page or subsequent pages of what that laboratory has done or will do for this project. Accomplishment slides lack information on accomplishments.
- Presentation slides 2–4 were confusing. The budget (slide 2) is $400,000; plans likely anticipate this or a similar amount to be provided annually. On slide 3, the first objective is "Reduce the installation cost of a hydrogen fueling station to be competitive with conventional liquid fuel stations"; there is a huge and obvious disconnect between the budget and objectives. DOE and others are spending tens of millions annually to achieve what are stated as H2FIRST objectives. Therefore, the presentation has a communication deficiency right off the bat. H2FIRST may be understood by its planners, DOE, and H2USA participants; however, the presentation content and the 30-minute oral presentation did not clarify the objectives and relevance of a project with a $400,000 annual expenditure.

Question 2: Strategy for technology validation and/or deployment

This project was rated **2.6** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The project appears to be well designed and feasible. Some groundwork has been done in setting up the various technology support panels; at the same time, the project appears to have the flexibility to set up and disband panels according to how needs evolve. Barriers appear to be effectively addressed, and there is significant coordination with established entities in the public and private sectors, building on existing competencies and ensuring representation of industry and public interests.
- The project seems well thought out and logically divided into project teams.
- The approach is nicely targeted at optimizing cost, reliability, and public acceptance of hydrogen fueling stations.
- This reviewer had difficulty discerning the SNL-NREL strategy from the presentation.
- The project approach, as communicated in slides 5–8, does not alleviate this reviewer's confusion and concern about the rationale for, and merits of, this project. There are some nice goals, such as "ensure relevance of activities through appropriate industry engagement," but no clear project approach. Slide 7 provides a glimpse of what H2FIRST is about. The approach is evidently to create a coordination panel and manage project teams, which will support H2USA. No task descriptions or milestones are provided for the project.
- This is just a concept, and there is too little information to make a reasonable judgment as to how it might succeed.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.3** for its accomplishments and progress.

- The project has just begun, but the project is primed for rapid progress.
- The project is still in its early stages but seems to have convened initial meetings in most of the project team areas. The signing of the SNL-NREL Memorandum of Understanding (MOU) is a big achievement. However, this achievement marks only the start of the process, not an end goal.
- The accomplishments and progress are tough to judge. The SNL-NREL team does not appear to really have done anything yet, but that may be almost wholly a function of a single factor: the effort is new and the team is just getting started.
- It is impossible to say what the progress is at this time because the project has not yet started to generate concrete results in terms of its objectives. With some teams there appear to be clear, measurable indicators with which results can be evaluated (e.g., reference stations); however, for others it is less clear (e.g., station acceptance).
- On slides 11 and 12, SNL and NREL capabilities and facilities are cited as "Accomplishments"—presumably project accomplishments. This is certainly curious and confusing. The accomplishments so far seem to be an MOU, a meeting, and the establishment of project teams. On slide 13 it was unclear what has led to the determination that this initiative is needed to fill gaps in an environment that already has many hydrogen-related initiatives, projects, plans, analyses, etc. Hydrogen stations are being planned, built, and accepted now. It is unclear how the H2FIRST project team will accelerate that process. It is assumed that some project funds are being used by SNL and NREL to provide H2FIRST leadership and management. During the oral presentation, it was stated that project funds are supporting a reference station project team, which will address near- and mid-term technical challenges associated with deploying hydrogen stations. The presentation materials and a half-hour briefing were not enough to educate the reviewers about this project.
- There has been no progress, other than some preliminary meetings.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The project has an excellent collaborative team, as well as excellent facilities and expertise with high-pressure hydrogen.
- The project can easily become unwieldy, given the varied subject matter and number of collaborators involved. The listing of collaboration entities seems logical and appropriate for tasks.
- A lot of partners are listed, but when questioned, the presenters indicated that not much effort had been forthcoming from the listed partners. Nevertheless, the score is mitigated by the fact that the project is so new.
- The list of collaborators was satisfactory, but the team should try to include Ford Motor Company, General Motors, and Daimler AG.
- A number of project partners are listed on slide 19. Curiously, SNL and NREL are not among them. Roles and responsibilities of the various project partners are not defined.
- There seems to be a significant degree of collaboration across DOE laboratories and with the H2USA partnership. The composition and indicated modus operandi of the project teams also indicate that a significant degree of collaboration and coordination among the involved entities will be engendered and will indeed be required for these teams to be effective. The project will also use data from other validation projects and experiences to establish what has been achieved and determine the way forward, so there is also collaboration with other DOE projects. Care should be taken to make the entities involved in various parts of the project manageable so as to be efficient.

Question 5: Proposed future work

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

- The future work is directed at beginning key projects.
- The team seems to have a good plan to bring people together, but there is no basis to judge how it might turn out.
- Further work needs to be devoted to articulating and characterizing H2FIRST. Presumably, most of the future work described on slide 20 will not be funded by the project. However, it is unclear if research tasks or the reference station design task will be led or managed as part of H2FIRST. It was not made clear that the H2FIRST project will not displace the processes by which programs, priorities, solicitations, etc. have been previously determined and accomplished.

Project strengths:

- The project meets a “catch all” need—it troubleshoots all aspects of the fueling experience. The PIs appear to be very well suited to the project. The organization of the project teams allows the project to simultaneously be focused and have a wide range of topics under investigation.
- This is a key project for the successful development of a hydrogen fueling infrastructure.
- The project is well thought out and well structured. It has significant potential to make real-life impacts.
- Two good national laboratories are leading the charge.
- There are no project strengths yet.
- No project strengths were noted.

Project weaknesses:

- Specificity of goals/objectives within each project team will be vital to project success.
- There is a risk that significant effort may be spent in coordination of various groups and interactions if not managed carefully. Care should be taken to ensure that indicators of the project’s success are measurable; at this stage it is not clear how the success of certain aspects of the project will be measured.
- Basically, it is not clear where H2FIRST fits in the world of hydrogen plans, programs, and initiatives being pursued by both public and private interests. The value added is not clear.

- There is too little progress to make a meaningful presentation. The researchers' plan is too vague.

Recommendations for additions/deletions to project scope:

- DOE should review this initiative with major hydrogen stakeholders in the public and private sectors to check on expectations that it will add value. Adjustments to H2FIRST should be made based on feedback.

2014 — Safety, Codes and Standards
Summary of Annual Merit Review of the Safety, Codes and Standards Sub-Program

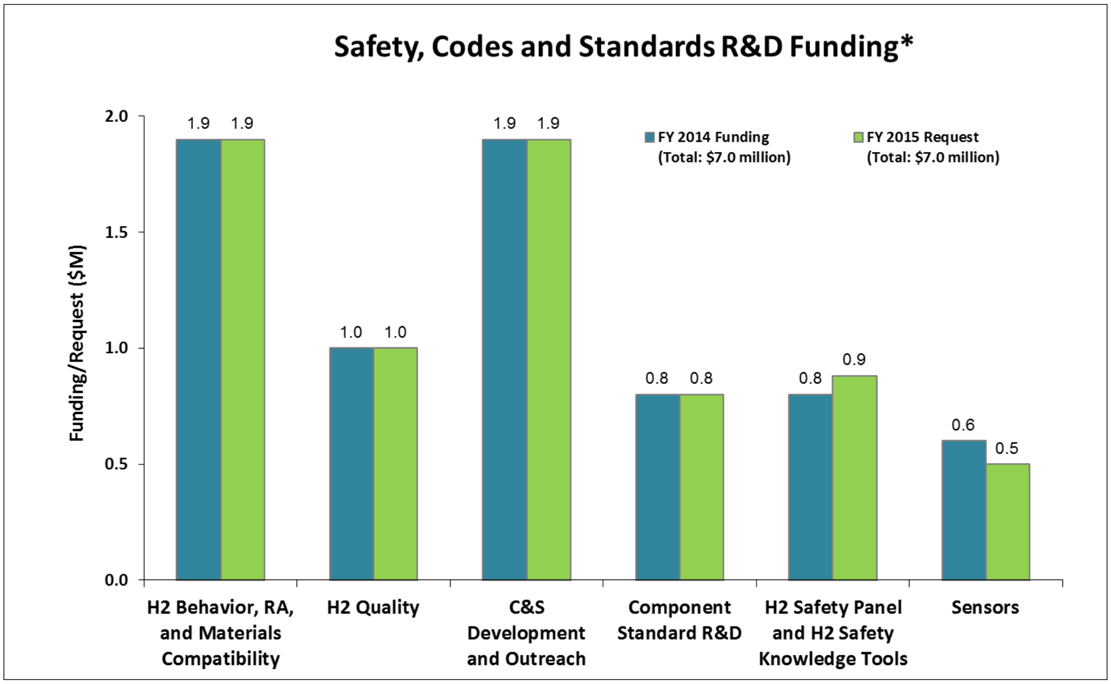
Summary of Reviewer Comments on the Safety, Codes and Standards Sub-Program:

The Safety, Codes and Standards sub-program supports research and development (R&D) that provides the critical 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. The sub-program also conducts safety activities focused on promoting safety practices among U.S. Department of Energy (DOE) projects and the development of information resources and best practices. Reviewers recognized that the sub-program's objectives and strategy are well defined and continue to provide strong support to enable early deployment of hydrogen and fuel cell technologies. The sub-program's portfolio was commended for its balance of activities in the following critical areas: hydrogen and fuel cell codes and standards, including domestic and international harmonization; permitting outreach; hydrogen sensor technology; hydrogen components and material compatibility; hydrogen behavior and fuel quality; hydrogen infrastructure risk assessment; hydrogen safety and related tools; and safety training for first responders and researchers. Reviewers made similar observations as they have in prior years, such as that projects in this sub-program have effectively leveraged the resources of academic institutions, standards development organizations (SDOs), code development organizations (CDOs), national laboratories, government agencies, industry, and other offices within DOE. Overall, the sub-program was acknowledged for its comprehensive approach for addressing challenges related to codes and standards.

In addition, although reviewers commended the sub-program for the strong international participation through existing engagements with the international standardization and regulatory communities, they noted that more active involvement would be beneficial. Reviewers also felt that the sub-program was well focused and well managed, but they stated that it could increase its effectiveness in reaching its goals and impacting the marketplace with improved outreach and technology adoption efforts to better market sub-program outputs. Finally, while reviewers recognized the sub-program's sound approach, they recommended shifting the current emphasis of activities from the deployment of hydrogen refueling stations to a more long-term perspective that also supports other applications.

Summary of Safety, Codes and Standards Funding:

The sub-program's fiscal year (FY) 2014 appropriation was $7 million. FY 2014 funding has allowed for continued support of codes-and-standards-related R&D and of the domestic and international collaboration and harmonization efforts for codes and standards that are needed to support the commercialization of hydrogen and fuel cell technologies. The FY 2015 request of $7 million will continue these efforts.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area.

Majority of Reviewer Comments and Recommendations:

In FY 2014, 12 Safety, Codes and Standards sub-program projects were reviewed, with a majority of the projects receiving positive feedback and strong scores. Reviewers' overall scores ranged from 2.5 to 3.8, with an overall sub-program average score of 3.3.

Hydrogen Behavior, Risk Assessment, and Materials Compatibility: Two hydrogen behavior and risk assessment projects were reviewed, earning an average score of 3.5. Reviewers commended the performance-based approach to risk assessment and sound scientific approach to addressing technical gaps. The reviewers suggested adding efforts to communicate findings regarding lower-cost steels and the benefits of automated welding to station builders and design engineers because these efforts relate to real-world service conditions. According to reviewers, the hydrogen behavior and risk assessment work should more closely engage industry and end users (i.e., code officials) of the Hydrogen Risk Assessment Models (HyRAM) tool and expand collaborations with international entities, hydrogen suppliers, and car manufacturers.

Hydrogen Quality: One hydrogen quality project was reviewed, receiving a score of 3.6. Reviewers commended this project for its progress and strong contributions to the international harmonization of fuel quality standards due to participation from international and domestic CDOs and SDOs. Reviewers suggested reexamining the project focus to ensure support of SAE J2719 (i.e., via compliance testing), bolstering national outreach and feedback activities, and extending testing from the membrane electrode assembly to the stack level.

Codes and Standards Development and Outreach: Two codes and standards development and outreach projects were reviewed, receiving an average score of 3.1. The reviewers praised these projects for their potential impact, their extensive lists of collaborators, and the Fuel Cell & Hydrogen Energy Association's (FCHEA) focus on multiple technology applications. The scope and breadth of this work and the depth of expertise and experience of the project teams were noted as clear strengths. However, the reviewers commented that the projects' approach and strategy for codes and standards deployment and outreach should be improved. They noted that the projects' approach and strategy should include more substantive and proactive engagements and input from industry stakeholders, with increased collaboration between national laboratories and trade associations (i.e., FCHEA) to gather critical input and better leverage resources and expertise to optimize the projects' impact.

Component Standard R&D: One component testing project was reviewed, receiving a score of 2.5. The reviewers commended the project's collaborations with manufacturers, system installers, CDOs, SDOs, and other stakeholders to ensure appropriate standards development efforts are undertaken to provide certified products to commercial markets. Reviewers strongly recommended (1) teaming with national laboratories to better leverage existing, relevant expertise and (2) focusing component standard development on system-level components of infrastructure hardware rather than a single component.

Hydrogen Safety Panel and Hydrogen Safety Knowledge Tools: One project in this area was reviewed, receiving a score of 3.8. Reviewers noted the project team's flexibility in developing tools and resources to keep pace with the changing stages of technology commercialization. The project was also acknowledged for its outreach efforts to insurance groups and authorities having jurisdiction to better understand user needs. Reviewers recommended that the project team collaborate more closely with FCHEA and the National Renewable Energy Laboratory (NREL) to avoid duplication of effort and provide more robust products.

Hydrogen Emergency Response and Safety Training: Two training projects were reviewed, achieving an average score of 3.2. Reviewers praised these projects for their continued training activities, both online and in person, and for filling an important knowledge gap. They recommended that the project teams examine long-term strategies to better engage targeted stakeholders (i.e., local fire and police departments) and broader audiences, with the potential to "train the trainer" and hand off training course activities for industry to continue.

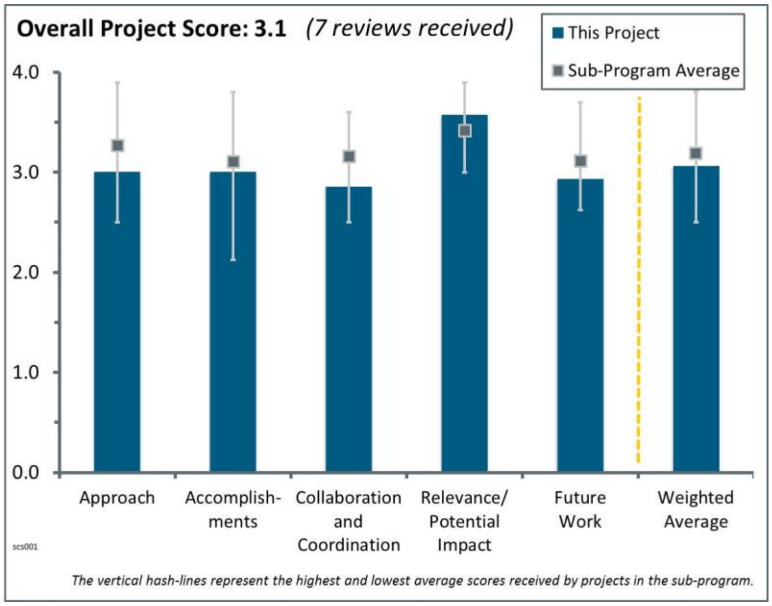
Sensors: Three sensor projects were reviewed, receiving an average score of 3.0. Reviewers applauded the overall approaches and notable progress made in developing a hydrogen-specific sensor and addressing technical barriers such as reliability, durability, and cost. However, reviewers recommended expanding the focus of project work to cover stationary applications instead of just vehicles, and ensuring the alignment of sensor development activities with related work in the Safety, Codes and Standards and Hydrogen Production and Delivery sub-programs. Reviewers also suggested identification of a mainline manufacturer to partner with and more rigorous estimation of real-world life cycle cost, market price, and overall operations and maintenance cost savings from new sensors compared to commercially available products. The Small Business Innovation Research project was commended for its progress and high accuracy over a wide temperature range, but reviewers recommended continuing the project only if NREL testing shows promise for the technology and at least one partner (e.g., fueling station builder or original equipment manufacturer) is added to the project.

Project # SCS-001: National Codes and Standards Deployment and Outreach

Carl Rivkin; National Renewable Energy Laboratory

Brief Summary of Project:

The main objective of this project is to develop and support the codes and standards required to safely deploy hydrogen technologies. Current year objectives are to develop outreach tools, conduct training, and address key codes and standards to support the safe deployment of hydrogen technologies. Long-term goals are to support the development of integrated codes and standards and to provide the outreach required to have these codes and standards used effectively.



Question 1: Approach to performing the work

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

- Continuous codes and standards improvement (CCSI) is a very good approach. Having a video with the Orange County Fire Authority is excellent; the reviewer is looking forward to it. Continued support of the permitting workshops is needed, critical, and extremely valuable!
- The overall approach for CCSI—continuous monitoring of the code development organization/standards development organization (CDO/SDO) community and user community—is critical to the success of regulations, codes, and standards (RCS) development. The other half of this is deployment—targeting the regional areas that are deploying hydrogen technologies and creating tools to be utilized regionally (the California permitting guide, for example) and extended to the national level—is very good. However, teaming with other entities working in the same space (e.g., Pacific Northwest National Laboratory [PNNL], Fuel Cell and Hydrogen Energy Association [FCHEA], Sandia National Laboratories [SNL]) was absent. FCHEA, H2USA, SNL, and the international community (through the European Infrastructure Project H_2FC, which includes members such as the Karlsruhe Institute of Technology, the University of Ulster, etc.) are all working to provide tools to help with the local authorities having jurisdiction (AHJs), with codes and standards (C&S), and with the needs of the permitting, design, and construction process. The partnering with appropriate groups—such as the local fire departments, programs that support deployment (such as the California Fuel Cell Partnership [CaFCP]), and now H2USA—is spot on. However, the project also needs to embrace fuel suppliers and original equipment manufacturers (OEMs). This was pointed out during the question and answer (Q&A) period. The presenter responded that the training sessions are open to the public and that these groups are free to attend. That was the wrong answer; these groups need to be included in the program, design, and execution of the product, not just be the recipient of the product. Attention and focus on the deployment product to make sure the intended audience can use it easily is very good; for example, putting this information in a form that is easy and effective to use and suitable for AHJs that need to have state-of-the-art knowledge. This was an issue correctly identified by the principal investigator (PI); however, no mention of the PNNL resource tool web portal was mentioned. Both activities would benefit greatly by a strong collaboration, which is clearly absent. Not including other efforts as 100% collaborators results in duplication of effort and a product that is less impactful than it could be. The approach to this work has a serious shortcoming centered on the notion of collaboration and teaming.
- The approach discussed is appropriate for this type of activity. Elaboration of the CCSI process by explaining “code cycles” would help the uninitiated.

- This is a never-ending task since C&S are always evolving. The best approach is to focus on the codes—not the component standards at this point, which seems to be the plan. Effective "training" for AHJs is likely to be a low-return investment unless timing/personnel work out perfectly. The permitting guide might have merit but requires actual use/verification to gauge impact.
- The approach consists of two activities, CCSI and deployment. This is a valid concept. However, the targets of these two activities are exclusively U.S. actors and entities (as indicated in the project title). Deployment of hydrogen technologies, in particular for transport applications, however, is a global issue. Hence it is recommended that the project include international activities, in particular those related to the hydrogen refueling infrastructure in the work.
- The approach for this work lacks a sense of direction. It appears that the PI does not have a coherent vision for the project. Also, there are insufficient industry feedback loops built into the project. It is not clear whether the project lead anticipates a transition to industry C&S improvement and, if so, where in the approach and plan there is this transition to the industry. There is not a clear building of consensus. The approach illustrated a myopic view of the development process with very little sense for the valuable work of building consensus and helping stakeholders through technical communication, barriers, and concerns.
- It is not clear when partnerships will be realized, nor whether there is a plan to engage even more code officials. Granted that only a few code officials now have to deal with hydrogen siting and certification now, but still, there is no clear strategic plan to roll this out to a larger audience because there are more and more hydrogen systems being deployed.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- There has been good progress harmonizing the International Building and Fire Codes and the National Fire Protection Association (NFPA). There is much work yet to do on liquid hydrogen, but the project has a realistic timeframe.
- The updating of the permitting guide with a focus on California's needs is very timely. What makes this particularly nice is that the PI is keeping national needs in sight. This will make it easier to create a "national" permitting guide. The PI should be careful to clearly delineate accomplishments (for example, "4 FY 2014 [training] sessions May 19 and 27" or "Draft 2014/final 2015") versus the activity: "active participation on C&S technical committees including identifying…" (slide 12). On slide 20 in the summary table, the reviewer credits as accomplishments: the formation of the NFPA Liquid Hydrogen Task Group (albeit a weak accomplishment) and the C&S workshops held. The reviewer considers activities such as the National Permit Guide/tools and the Stationary Fuel Cell Permitting Guide to be works in progress, not accomplishments with 2014 publication dates. There are only two items listed on slide 23 (for reviewers only), and these are for "inside audiences." One is for DOE, and the other is an internal National Renewable Energy Laboratory (NREL) technical report, and is not in the refereed literature. This is weak. While the workshops are an appropriate and important outreach activity, there should be a more aggressive effort to put the accomplishments of this work in the open literature for larger distribution—for example, the *NFPA Journal*, the *International Journal of Hydrogen Energy*, or in DOE records—whatever is appropriate, given the article. The PI for this project is perfectly able to identify suitable open public domain outlets of this work. Paying attention to this, the number and impact of the accomplishments this reporting period listed are weak, particularly considering the average funding for this work is two full-time equivalents. Largely, this could improve substantially if this project would aggressively embrace the collaboration/teaming of others in this topical field.
- The accomplishments and progress are uneven. There are a number of reasons for this that are mainly outside the National Renewable Energy Laboratory's (NREL's) control. The progress seems to be predicated on the CDO/SDO committee makeup, which actually reflects industry interest. When there is a lack of interest or urgency expressed by industry, the void is often filled by individuals with good intent but lack of hands-on experience.
- It is unclear where the results are in the accomplishments. The example cited that this project provided a bridge in NFPA 2. It is not clear how this project is coordinated with other national training efforts. This project seems to exist as a reporting service for DOE on the outcomes of codes and standards meetings. The

project should reevaluate its direction. It is not clear how CCSI—an acronym, a PowerPoint slide, and a process—is considered an accomplishment. It would be good to have a relevant example of how this was implemented or how the process has been used and how it directly benefited standards development efforts. The National Permit Guide had no collaboration; the reviewer was concerned about the utility of this information, and that it may hinder, rather than ease, permitting of hydrogen infrastructure.
- It sounded like training deployment was well-received, but people receiving the training sounded like they wanted more information on how code requirements could be applied to an actual project.
- Progress is measurable but could be larger with increased availability of resources (as also mentioned in last year's review).

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The project has a nice extensive list of collaborators.
- The CDO/SDO coordination list is impressive but has a couple of key omissions. The American Society of Mechanical Engineers (ASME) is also highly active on a newer section of the B31 Piping Code, specifically, Section 12. The next chapter for Section 12 would cover residential/commercial applications. The envisioned activity would be to address guidance in Class 150 or lower pressure classes, borrowing from ASME B31.2 and NFPA 54 for experience-based input on important design considerations.
- The list of collaborators on slide 16 would seem to be great partners, but the presentation did not highlight the extent of involvement or significant activities in which each collaborator has been involved. The interactions should be better highlighted.
- The project seems to have interfaced with appropriate organizations.
- NREL needs to strengthen their teaming/collaborations on this project. The partners need to be working with the PI to develop the C&S outreach and documents (permitting guides, training, etc.). There are several examples; the PI discussed the need to address separation distances in the NFPA model codes specifically in liquid hydrogen applications, yet no mention was made of teaming with others in this program who are also addressing this issue—specifically the quantitative risk assessment work from SNL. During the Q&A session, this shortcoming was noted several times, i.e., requests to embrace industry, OEMs, fuel suppliers to be part of the team to ensure there is alignment with vested industry interests. The PI did comment that the training sessions are open to the public, so all the aforementioned groups can attend. However, these stakeholders need to be on the development side of this activity, not just the receiving side. International CDO/SDO involvement in the product development needs to be strengthened as well; the comment was made that NFPA needs to be updated/fixed. NFPA is on a five-year cycle. International Organization for Standardization Technical Committee (ISO/TC) 197 is now back on track, and indeed a couple of the working groups (WGs) are on a fast track (WG 24 – gaseous hydrogen fueling stations, for example). This ISO product will be completed before the next NFPA cycle is complete. WG 24 will proceed without the benefit of this NREL effort as a result of the project's waiting for NFPA to complete its work. Often ISO documents are used as backup supporting language if there is a gap in local codes (per the reviewer's private discussion with CaFCP). Therefore, making sure these efforts proceed with cross-fertilization is important. This effort needs to have a less nationalistic focus and be more open to the international community. The lack of teaming/collaboration with other key stakeholders in this field was almost singularly the reason for a score of 2.5. This is a serious shortcoming of this work and its approach.
- Evidence has been provided that interaction takes place with other organizations (however exclusively within the United States). From the information included in the slides and presented at the DOE Hydrogen and Fuel Cells Program Annual Merit Review, it is difficult to assess whether the interaction has also led to effective collaboration/coordination of activities. There is no evidence provided on the mentioned interaction with ISO/TC 197.
- Documents that serve the needs of industry were produced by this group with no industry feedback. The deployment training has been "sold" to the state of California without any vetting from the industry participants putting in the stations. California is trusting that DOE has oversight of the development of this type of reporting and training material. This project would benefit from additional oversight to ensure sufficient peer review criteria for use as a training tool and published report.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- The relevance and impact from this NREL project in conjunction with the PNNL H2 Safety Panel are key to the success of hydrogen and fuel cell commercialization—not only for reaching a commercial phase but for sustaining it, either directly or indirectly. The response to the reviewer comments is encouraging and on the right path; it is good to see the continued work in developing C&S and their application. The project should keep up the good work!
- Promulgating national model codes to local AHJs is critical to the deployment of hydrogen technologies. Indeed, since rollout of fueling infrastructure is happening now, there is an immediate urgency for this to enable safe rapid deployment. The purpose and relevance of this project is right on.
- This activity is becoming critical path for the adoption of hydrogen as a transportation fuel. Completion of the first editions of the various documents is a must. Revision based on usage of many of the existing documents is needed.
- Training AHJs and getting their approval on hydrogen applications is crucial to future system deployment.
- This is an indispensable activity that could benefit from additional resources.
- C&S are very relevant to the successful roll-out of hydrogen fuel stations.
- This project has a great deal of potential relevance but does little more than participate in activities. Building consensus and eliminating technical barriers is a proactive role. It does not require leadership of the committees, and in fact, government should not be leading standards development activities unless specifically requested by the committees. But this project could be doing the work of building consensus—communication between meetings to determine the differing points of view and to see whether and how relevant science or information can help establish a resolution to those differences.

Question 5: Proposed future work

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

- The liquid hydrogen work had stalled, and now it has restarted. A similar concentrated push is recommended for two additional areas: (1) separation distance to lot line and how to handle this, and (2) mitigation means to reduce distances.
- Constant assurance that the workshops and guide are in line with industry is imperative. The presenter commented that the project team will "accelerate rather than decelerate the momentum—going through the California Fire Code and specifying the requirements and getting feedback from AHJs. Training sessions are not limited to just permitting officials—they are open to project developers as well." The effort to track the permitting timeline of stations with the training may be cumbersome but provides a valued statistic—especially in California as a model for the rest of the nation—and something that H2USA can run with. It is very important to show the international collaboration with ISO!
- The proposed future work is appropriate for this overall activity. This group needs to aggressively embrace the efforts of others in the development of NREL's work product. It would be good to see an activity that is focused on developing that team in a couple of areas: (1) national and international tools (PNNL, FCHEA, SNL, H2FC [Europe], ISO, etc.), and (2) training and workshops (embrace OEMs, fuel suppliers, industry, etc.).
- It appeared that a significant amount of work was planned for the future. During the presentation, the speaker would make the following comments: "We still need to do that," or "...in the future, we will do that," etc. Sometimes it seemed like work that was supposed to have already occurred had not been accomplished.
- The project should enhance coverage of and interaction with international activities.

Project strengths:

- This PI is very knowledgeable and enthusiastic about RCS, specifically in code language, process (from AHJs through model code development), permitting, etc.
- The project lead has a wealth of knowledge. There is ability for a broad reach from a respectable source (or team of sources).
- The scope and breadth of the project is a strength.
- The project has great potential.
- There is good focus.

Project weaknesses:

- It really boils down to the closed nature of this work. Significant improvement needs to be made in embracing from a teaming perspective in the creation, execution, and delivery of the work products. Excluding others who are working in the specific area can lead to duplication of effort or, worse yet, inconsistency in information, resulting in a work product that is not as complete or powerful as it could be. This is a serious shortcoming in the execution of these work products.
- It is of concern when NREL chairs the authoring of documents in this arena, especially fuel cell documents. The product standard and the installation standards have been in use for about 20 years. These documents are embedded in both the International Code Council (ICC) International Mechanical Code (ICC IMC 924) and NFPA (NFPA 5000 – 6.4.2.61) code sets and have been used to site thousands of power plants in the United States. If another fuel cell document is desired, the effort should be led by the FCHEA, not a national laboratory.
- The effort needs to be broadened to more “hot button”-type issues that also cause practical constraints on siting of hydrogen systems.
- The project should reevaluate its scope and implement a more rigorous approach so that this work can reach its potential.
- The project lacks international dimension.

Recommendations for additions/deletions to project scope:

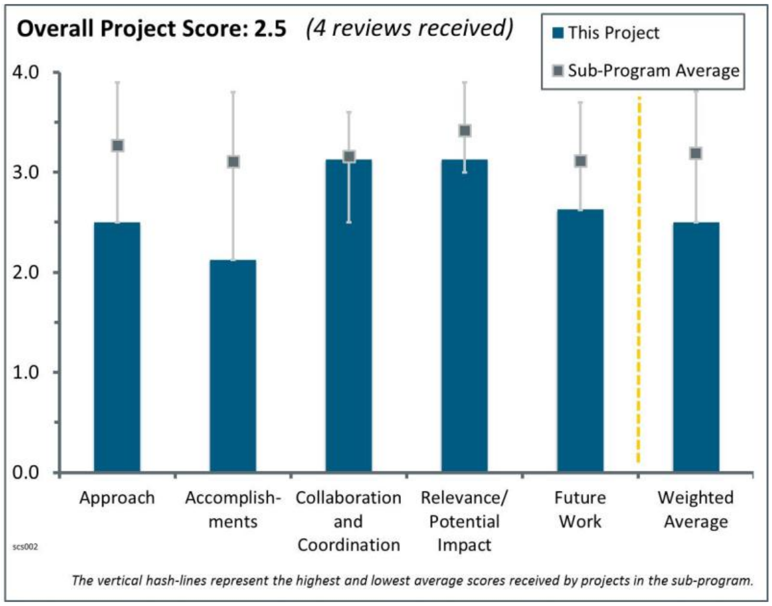
- The project should work across the board with other DOE and laboratories/programs—specifically, the Hydrogen Safety Panel and their resources and efforts in C&S outreach, education, and harmonization. There is certainly synergism among these projects that would result in an even stronger result!
- More attention and support must be paid to the piping code and the CSA HGV activities.
- The project should add coverage of international activities.
- The project should establish clear guidelines for success and expectations.

Project # SCS-002: Component Standard Research and Development

Robert Burgess; National Renewable Energy Laboratory

Brief Summary of Project:

Successful deployment of hydrogen infrastructure will require components that are proven to perform safely and reliably as measured against new safety and performance standards. National Renewable Energy Laboratory (NREL) component research and development test efforts are focused on supporting component manufacturers and system installers so that fully tested and certified hardware is available.



Question 1: Approach to performing the work

This project was rated **2.5** for its approach.

- This work is critical and directly supports the program objectives and addresses key barriers. Test results will be used (in the future) to inform Safety, Codes and Standards (SCS). International Organization for Standardization /Technical Committee (ISO/TC) 197 has already begun international working groups to write standards on many of the component topics being addressed by this work. It would be very worthwhile to ensure project investigators are directly engaged with these efforts sooner rather than later. This will not only inform the standards developers sooner, but also may provide some specific direction to follow-on activities.
- Working from the systems level and piecemeal down to the sub-component level is a good approach in the testing and qualifying of systems and components. This team really needs to work much more closely with the Materials Group at Sandia National Laboratories (SNL). There is much proposed overlap in capability and activity. SNL has been working in hydrogen effects on materials for many decades and is the globally recognized expert in this area. This project can benefit greatly from not reinventing this space, but from teaming with the SNL staff in hydrogen effects in materials. The word teaming is used instead of collaborating to denote a teaming at the time of project planning and participation rather than an occasional consultation, as was described in the valve testing work. For example, instead of the NREL team performing stress fracture fatigue testing at NREL to "understand" the fundamental processes of crack growth, this part of the effort should be moved to SNL, where the equipment and staff have decades of leadership in this very area. Components testing at NREL and the fundamental fatigue crack growth work done at SNL would make for a powerful activity. The discussion on slide 15 was an unnecessary investigation into the Emeryville failure on a problem that has already been thoroughly investigated by the SNL incident investigation team—which included the staff material scientists. The topical area of fatigue crack growth on a fundamental level is being investigated by the SNL team, and those results are being published in the refereed literature and promulgated to the appropriate ASME codes. This NREL activity is a duplicative effort with one this office is already funding. There is value in understanding component and systems behavior (which is what this project was presumably all about)—the fundamental material behavior in hydrogen environments is the focus of the SNL materials work. Teaming between these two activities will prove to be very valuable. This activity needs to partner with SNL, not duplicate and compete with SNL. A major activity for this project is to build up the new Energy Systems Integration Facility (ESIF). It is of concern that this approach does not seem to be embraced as one executes individual projects. The current example is the relief valve project; the temperature and pressure range for the *facility* should be made to represent the domain required for the hydrogen infrastructure. There are regions of the world that will reach -40°F and upwards of 80°FF (Alaska and Palm Desert above black sun-backed

asphalt, respectively). Also, contemporary fills needs to be J2601-compliant, which also involves the -40°F to 80°F range, as well as operations at ~87.5 MPa for a 70 MPa fill—this laboratory falls short operating in this domain and will have to be upgraded in the future as projects (components) are tested that need to operate in this more challenging domain. If one builds up a new facility and does not configure it for foreseen near- to long-term measurement domains in the beginning, it simply means that the facility will have to be modified in the future at greater expense in time and financial resources. In this particular example, temperature and pressure domains are specified by J2601 fill protocol and environmental conditions found around the world—as noted above. There is no evidence that this approach is being implemented with the construction of the new facility. Indeed, it appears that the capability is designed around the testing campaign at hand, leading to restricted temperature and pressure range of operation ($T \leq 0°C$, $T \geq 50°C$, 48 MPa, as shown in slide 13—albeit the project team is planning to upgrade the facility to 70 MPa, but there was no mention of upgrading the temperature range). This alone might indicate that the appropriate capability is being constructed; however, during the question and answer period, this point was identified as a weakness, and the presenter did not supply a defending answer to suggest embracing a more correct operating domain. This is a serious shortcoming since it will lead to unnecessary delay and additional funds to upgrade the facility to perform testing in the appropriate domain. Teaming and collaboration is critically important for this project in developing and executing all work products.

- There appears to be good coordination between the barriers being addressed and the subtask developed to address it. At the same time, however, there appears to be a good deal of risk involved in relying on so many other projects to provide the bulk of the data. The Fuel Cell Technologies Office (FCTO) SCS-funded project itself appears to be involved with only the pressure relief valve (PRV) failure mode investigation, and this project is relying on other resources to address compressor reliability (FCTO Technology Validation sub-program), flow metering (State of California), hose reliability, receptacle wear and nozzle durability (FCTO Delivery sub-program), and low-temperature sealing (dependent on ESIF readiness and availability). While this may result in successful data acquisition, it appears to focus risk from these other resources on this project.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.1** for its accomplishments and progress.

- This is an early-stage effort for components—an important activity. It would be good to see more workshops with industry and code development organizations/standards development organizations (CDOs/SDOs), as well as discussions at the National Hydrogen and Fuel Cell Codes and Standards Coordinating Committee (NHFCCSCC) on this effort.
- Accomplishments presented in this review are really discussions of activities and sometimes future planned activities, not accomplishments. Only three activities are recognizable as accomplishments: (1) the publication of an internal NREL technical report; (2) the successful launch of the Hydrogen Fueling Infrastructure Research and Station Technology project (H_2FIRST), which is a noteworthy accomplishment, but this is an accomplishment for a larger team at NREL/SNL, not just this one project; and (3) the identification of the failure mode of the pneumatically operated valve, which would result in release in a vent pipe. A weak publication output is expected for a laboratory that is being moved and/or under new construction since the time is spent on the upstream side of research projects. However, this laboratory has been operational since the summer of 2013 (about one calendar year; see slide 6), and the project has been active since October 1, 2013. This implies that it has been operational since last summer to produce the studies/testing on components it is intended to perform. Results from this work should be evident by now, yet the only accomplishment that was presented was identifying that the failure mode would release into a vent pipe. A more substantial discussion on the year’s accomplishments was expected. Indeed, a discussion of a test plan for a pneumatically operated valve was presented, but it appears that even this fell short of what would be expected, as that opportunity was not used to configure the facility for efficient operation in the future. This is a big concern. This is funded at a level of about 0.7 full-time equivalents (FTEs); there should be more concrete output from this project—at least concrete preliminary data/results from initial component testing; only one simple straightforward observation was presented.

- There seem to be good data being generated for PRV failure, but it was stated that the failure mode tested was for failing open, while valves failing closed or partially open would be necessary failure modes to consider as well. No data were reported other than PRV data.
- It was very difficult to discern what value this project brings to the SCS sub-program. It is quite unclear what the results of this investigation will uncover, other than not using this particular valve with hydrogen, which the manufacturer already prohibits.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The peer-reviewed NREL publication, the collaboration with the Colorado School of Mines metallurgy laboratory, and the NREL Component Webinar, which is now planned for July, are all commendable. This activity is in the early stages, developing data that will be available for specific regulations, codes, and standards (RCS) activities in the future. It is good to see the collaboration with the National Institute of Standards and Technology to exchange knowledge to improve metering work. This work is critical to H2USA and other efforts. Monthly engagement on this project with the NHFCCSCC would benefit industry.
- A large list of collaboration partners includes other national laboratories, industry, universities, and SDOs.
- The work/collaborations with CDOs and SDOs, manufacturers, and system designers are critical, so this interaction is very good. Stronger teaming with other laboratories will strengthen this work and keep it unique. A stronger teaming with SNL material staff was specifically noted as needed to ensure efficient use of DOE funds and to avoid duplication of effort. H2FIRST will help in this area if the teaming approach is embraced aggressively. The collaboration criterion was scored solely on the type and quality of the collaborators.
- It is impossible to distinguish what unique collaborations this project provides, as so much of the project presentation and information references other projects (e.g., compressor reliability, H2FIRST, etc.).

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- Understanding component safety and failure potentials is extremely relevant to preventing catastrophic failures. If the project team is able to pull in all the variously sourced data, it will be most valuable.
- Infrastructure requires components that meet performance safety standards. This work is much needed and extremely relevant to deployment of hydrogen technologies and the development of the fueling infrastructure.
- Flow meter accuracy and hose reliability are both critical issues for developing RCS.
- This project could be very relevant and have a tremendous impact, as components are important. Based on that potential, this project is missing on all elements: scope, vision, planning, execution, results, presentation, etc.

Question 5: Proposed future work

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

- The future work plan is good, provided teaming with appropriate institutions is embraced.
- The list of future work tasks seems to cover the areas that NREL has included in their overall plan. However, most of these areas (hose reliability, nozzle and receptacle wear/durability, flow metering) appear to be funded by sources other than SCS. The future work area really needs to emphasize the work to be done for the SCS sub-program.

- Proposed future work is not yet well-defined, as it is largely dependent on results of current efforts and DOE direction. Review of developing component standards in ISO/TC 197 is highly recommended for possible suggestions of future work.

Project strengths:

- This project in principle should prove to be very valuable to the community in identifying failure modes and quantifying system behavior in hydrogen service under conditions expected for operating hydrogen systems. This can only be obtained with constructive partnering (teaming) with others who are working in similar and related activities.
- The overall concept of treating standards development at the component level is very necessary as part of the entire standards development effort. The coordination with other entities and the breadth of the entire proposed component work are other strengths.
- Strengths include working with manufacturers and system installers to provide data to ensure tested and certified components are available.

Project weaknesses:

- Overall concrete accomplishments for the second year of a program are weak. This team needs to re-evaluate their teaming approach. This project in particular needs to establish a team/collaboration with other facilities that have expertise and background in the fundamentals of hydrogen effects in materials, particularly stress and fatigue fracture growth physics. SNL is the facility suggested.
- While the overall description of the components addressed is good, it seems like the specific technical work directly for the SCS project is narrow and only involves PRVs at this point. In addition, it seems only one PRV failure mode is being addressed.
- The biggest weakness is the limited industry engagement in this project.
- The project's purpose, scope, and approach should be reevaluated to ensure that the project team's expertise and collaborative efforts with industry are optimized to add critical value to this research area.

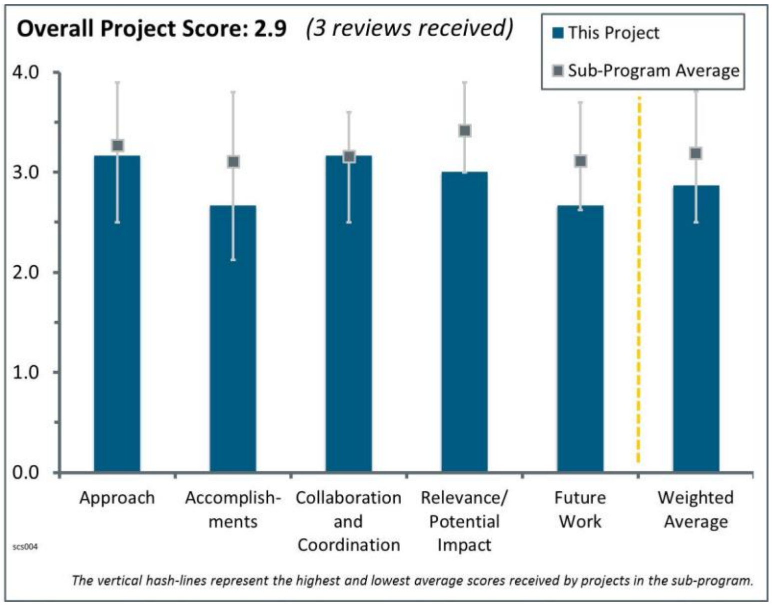
Recommendations for additions/deletions to project scope:

- A serious gap is not yet being addressed, and that is one of metering. The current NREL mass measurement device satisfies the California Division of Measurement Standards' current need to measure the mass of delivered hydrogen in a way that complies with NIST Handbook 44 (HB44), which is all very good. In the immediate future, however, the fueling infrastructure needs the ability to qualify mass flow rate meters that have the capability to satisfy HB44 under a J2601-compliant fill. Mass as a function of time needs to be measured accurately from the consumer's perspective and one must be able to integrate accurately to get the total mass delivered at any point in the fill process. A meter is needed that will deliver performance in a similar fashion to today's consumer's experience with conventional fueling stations. The consumer needs to be able to stop the fill when the quantity filled satisfies the consumer's fill constraints. There are several technology gaps that currently prevent this from occurring: (1) a fueling station mass flow rate meter that meets HB44 under a J2601 fill does not exist, (2) a mass flow rate device to qualify this meter does not exist (note: it needs to be 3 to 10 times more accurate than item [1], a "master meter"), and (3) a facility to qualify the master meter very likely does not exist (another factor of ~10 times improvement in accuracy). This is a serious gap, and attention to these gaps needs to be given.
- The project needs to find a scope. A great use of this project would be a comprehensive report on components—all components for a given part of the infrastructure. Instead of in-depth investigation of a single component (which this project is not equipped to do correctly anyway), the project should instead focus on a higher-level overview and exist as a resource that publishes periodically the status and opportunities with respect to components, perhaps choosing only components at a hydrogen forecourt or similar segment of the hydrogen delivery and use pathway.
- Component certification issues would be a welcome addition for discussion at the monthly NHFCCSCC meetings.
- The project should widen the scope on PRV work to include other types of failure modes (e.g., the valve never opens).

Project # SCS-004: Hydrogen Safety, Codes, and Standards: Sensors

Eric Brosha; Los Alamos National Laboratory

Brief Summary of Project:



The objectives of this project are to develop a low-cost, durable, and reliable hydrogen safety sensor for stationary and infrastructure applications that can be extended to use in vehicles; to demonstrate working technology through performance evaluation in simulated laboratory and field tests, initiate rigorous life testing, and evaluate sensor performance in relation to codes and standards; and to pursue commercialization of the new sensor technology through industry partnerships.

Question 1: Approach to performing the work

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

- Tailoring the already very successful automotive O_2 sensors for hydrogen is very good. Success should lead to a cost-effective sensor. Running the ignition study at the minimum ignition energy (MIE) (22% to 26% hydrogen) would have been better than 20%—ignition energy varies by a couple of orders of magnitude (this is really an issue in execution). The presenter agreed to address this in the future.
- The main effort is to develop a reliable hydrogen-specific sensor. Success would enable wider use of sensors in hydrogen technologies; however, there are other ways to detect leaks without detecting hydrogen.
- The technology is interesting and appears viable. Some of the approach regarding the ability to have a certified product may not be robust or valid. The true benefit seems to be low operating/maintenance cost. There is discussion of "low cost." The sensor itself is a small cost of a total package, so whether the sensing element is $1 or $5 won't affect a $500 list price item.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.7** for its accomplishments and progress.

- This is a modestly funded program (~0.5 full-time equivalents [FTEs] at Los Alamos National Laboratory, ~0.25 FTEs at Lawrence Livermore National Laboratory). This project has been making good steady progress appropriate for the amount of funding received. Last year, the project team successfully solved some signal processing issues; this year, they have systematically moved into a testing phase utilizing the National Renewable Energy Laboratory (NREL) sensor testing laboratory with good results. They are ready to go to the field. From a safety perspective, however, it would be good to see a bit more assurance that there is no potential for ignition in an upset condition. While safeguards are in place to ensure that the heating unit does not exceed a nominal temperature, an upset could occur resulting in significant over-temperature of that unit. The principal investigator (PI) ran some experiments in hydrogen/ air mixtures up to 20%; however, the MIE point is between about 22% and 26%. Albeit the curves are fairly flat in this region, the ignition energy does vary by several orders of magnitude depending on energy deposition characteristics (in this example, spark gap). The experiment should be repeated with a mixture closer to the minimum MIE; measurement or calculation of the hot zone temperature should also be known. The auto-

ignition temperature for an MIE mixture is on the order of 773 K to 858 K. It should be ensured that the hot zone of this device never gets to these temperatures to ensure that ignition from this thermal source does not occur.
- While there has been some demonstrated improvement in sensors because of this project, there remain significant challenges. In particular, there are not yet manufacturers on board to produce sensors for the necessary applications. It is not yet clear this project will lead to commercially available, economically viable sensors.
- There is good progress for now, but the lack of involvement of a main-line sensor manufacturer is getting critical. Without that, there will be little potential for mass deployment.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- This project team has proven to be very valuable to this project, advancing the development of this device at a very appropriate pace. At this stage of development, however, a commercial manufacturer of these systems should be sought. This project team understands this need and pointed it out during the presentation.
- Several partners are involved, but the project needs a name brand sensor manufacturer with the manufacturing and marketing muscle to use the product.
- Sensor testing is conducted in enclosed spaces only. The NREL sensor laboratory seems to be looking at using sensors at refueling stations (outdoors). There may be a disconnect here.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- There is a fundamental problem with point sensors in hydrogen applications. They have proven to be largely an unreliable method of detection (not that the sensor is unreliable but that the use of point sensing technology is unreliable). Unfortunately, the safety community (NFPA, etc.) requires the use of point sensing for detection in these environments. This project is very relevant, given the community’s requirement for these devices.
- Sensors are important but not critical to mass deployment. They are a small percentage of overall cost, both capital and operating. There are solutions, albeit not perfect, in the marketplace. It is not clear yet whether this effort will yield a meaningful benefit or a small incremental benefit of relatively low value.
- This project is focusing on filling the gap of good sensors around the lower flammability limit. This is a very specific target technology. Hydrogen sensors are not the only way to detect hydrogen leaks.

Question 5: Proposed future work

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

- The “real world” testing is good, but it is in a very benign climate. Consideration of extremes of heat, cold, and humidity would be important. The project should take place in Chicago, not Los Angeles.
- Risks are well-defined; however, some of these may not be influenced by project outcomes. It is not clear that fuel cell electric vehicle manufacturers want hydrogen sensors, even if performance is improved and costs are reduced. Perhaps this issue could be addressed at a workshop with significant original equipment manufacturer participation.
- Future work plans are right on to move this to a commercial product. In response to the safety concerns mentioned during the question and answer session, the presenter did comment that the team would investigate the ignition question, further per reviewers’ comments.

Project strengths:

- The presented overview of project activities since 2008 was excellent, showing the progress over the years. This is a good project that includes testing to determine whether the sensor itself is an ignition source. The project took advantage of an excellent opportunity to run parallel testing to compare the sensor with an expensive commercially available alternative. Other key features include wireless communications and backward compatibility.
- This project is making good progress, given the funding levels. The makeup of the team is appropriate for the development phase of this project. The team understands the need to embrace additional commercially oriented partners to move this to the commercialization phase.
- The technology appears to have potential for improved sensors. The advantage is in the operating, not capital, expenses.

Project weaknesses:

- While attention was given to the safety aspects of this technology, it fell a little short of being as thorough as it should be. With that said, however, the presenter did agree to include the necessary studies in the program. No real weaknesses were noted.
- Lack of a mainline manufacturer means that: (1) final market pricing cannot be determined, so it is unclear what the overall savings are, (2) actual certifications cannot be done in a professional, knowledgeable environment, and (3) the technology will not necessarily be commercialized effectively, as it needs the appropriate marketing/sales channels.
- No analysis of sensor performance versus International Organization for Standardization (ISO) 26142: Hydrogen Detection Apparatus was shown.

Recommendations for additions/deletions to project scope:

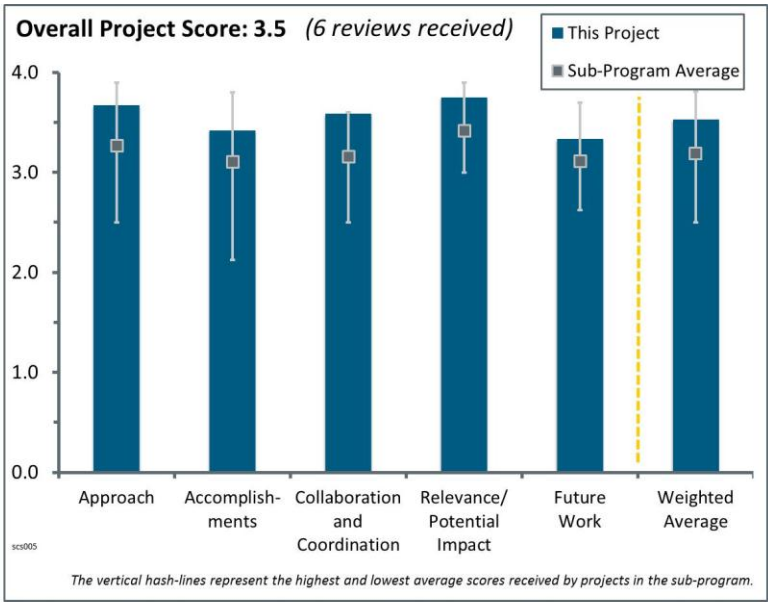
- Analysis of sensor performance for use in refueling stations in accordance with ISO 26142: Hydrogen Detection Apparatus would be useful information to provide to the standard committee for this ISO Standard for potential revision or to find additional uses for the sensor technology beyond those described in the project report. Analysis of potential benefits of hydrogen detectors in fuel cell electric vehicles would be interesting. It is not certain that this project is important to the target market today.
- The project should get a mainline manufacturer onboard with a business plan as soon as possible. Cost savings should be defined in operation/maintenance for an overall life cycle cost.

Project # SCS-005: Research and Development for Safety, Codes and Standards: Materials and Components Compatibility

Chris San Marchi; Sandia National Laboratories

Brief Summary of Project:

The objective of this project is to enable technology deployment by providing science-based resources for standards and for hydrogen component development. This project will also participate directly in formulating standards for use in development and testing of materials and materials compatibilities in hydrogen systems. The standards database will be expanded through materials testing targeted at data gaps in the existing body of research.



Question 1: Approach to performing the work

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

- The approach is fully correct and consists of identifying gaps in knowledge, establishing and validating representative test methods and methodologies, performing targeted testing and ensuring adequate knowledge transfer to Standards Development Organizations (SDOs) and Code Development Organizations (CDOs). Exchange of views and cooperation with non-United States (US) advanced materials testing experts (in addition to the Japanese institutions mentioned, and in particular within the European Union) should be further explored to increase efficiency of the work and disseminate its outcome. (Note that the recently extended EU-US Scientific and Technical Agreement can provide a frame for such a collaboration.)
- The overall approach to addressing targeted data gaps is addressed through comprehensive CDOs and SDOs evaluation. Working with the relevant SDO/CDO, gaps are being identified, and data are being generated to revise various materials and component standards that comprehensively cover high-pressure system safety.
- The idea of generating a database is nice, but what is really needed is for the information to be in Section II of the American Society of Mechanical Engineers (ASME) Boiler and Pressure Vessel Code and to be in Section 12 of the ASME B31 piping code. The avenue for both might be the ASME B31 T committee. This ASME venue will hopefully be the final repository.
- The project is a good exploration of automatic versus hand-welded specimen testing.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- Several accomplishments for the year are listed and easily identifiable on the publications and presentations page and elsewhere in the slides. Concrete progress in informing/developing standards is presented. Populating the materials properties database will make safety data information more easily available and accessible.
- The literature search on plastics is very nice; sharing the information with the ASME B31-12 and the ASME National Modeling and Simulation Coalition (NMSC) would be better. This ASME venue is, one hopes, to be the final repository.

- Compared with the future work identified in the 2013 DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) slides, relatively little progress seems to have been made in the execution of mechanical tests. This impression is strengthened by the facts that: (1) no information is provided on a number of activities identified as proposed further work in the 2013 AMR, and (2) the fiscal year (FY) 2013 funding is double that expected in 2012 ($0.8 million versus $0.4 million). The addition of the composites chapter to the technical report is commended.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- SDO/CDO representation is excellent. Industry partners and international research institutions appear to be excellent collaborations as well. This reviewer has no knowledge of who the big players are in this area, though. It is assumed that they are adequately represented because of the codes and standards (C&S) that are being developed through evaluation of relevant materials supplied by vendors.
- There is great highlighting of international and partnership work sharing and milestones.
- Collaboration with relevant institutions and organizations within the US is purpose-oriented and sufficient. Reaching out to specialized non-US testing houses is recommended to increase density of experimental data and to include materials extensively used for hydrogen applications in other parts of the world. Including international standardization bodies is also recommended.
- Additional outreach is necessary at ASME. In many large organizations, the left hand and the right hand do not communicate. Oftentimes, multiple approaches need to be tried to get to the parties that would welcome the data.
- The project should maintain relationships with the international community and SDOs. This can also be a source of material and information, as mentioned in the “Critical Assumptions and Issues” slide.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- As presented, materials and components compatibility data and test methods have impacts on multiple standards relating to high-pressure system safety, which is the paramount issue with fuel cell fuel storage and delivery systems.
- This activity is highly relevant and has the potential to make a major impact in the development and acceptance of hydrogen-related C&S.
- The project effectively addresses a knowledge gap.

Question 5: Proposed future work

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

- Proposed future work continues similarly to develop and evaluate test methods to evaluate component performance. Proposed procurement of variable-temperature testing equipment to test at low temperatures should be continued.
- The proposed future work follows logically from the achievements to date.
- The data being produced are really good, but engineers who can use this data need to be educated, or some recommendations on how to apply this data to real-world systems would be a good idea.
- The proposed work is interesting but could be considered incomplete. Sandia National Laboratories seems to have forgotten the end goal: the support of the American National Standards Institute (ANSI) standards.

Project strengths:

- Strengths include value for industry, potential cost reduction by use of lower cost steels (such as 304L, etc.). There is applicability to welded pipe fittings in hydrogen stations; results suggest that automated welding is preferred because of fusion zone consistency compared to results of manual welding.
- The project appears to be addressing gaps in test procedures and material property data that are relevant and concurrent with industry progress.
- The work has a direct impact on current and near-term decisions and standards in development.
- There is a direct link to SDOs and CDOs.
- The research is a strength.

Project weaknesses:

- The project targets implementation aspects in C&S used for the assessment of material behavior in a hydrogen environment. It does not address the adequacy/relevance of these standards and codes in terms of being representative of actual service conditions of these materials (temperature range, loading conditions, and presence of residual stresses).
- There are limited resources. Also, the project needs to develop further international relationships.
- A weakness is the lack of distribution of lessons learned information to station builders/design engineers.
- There is a lack of understanding as to who the end customers are.

Recommendations for additions/deletions to project scope:

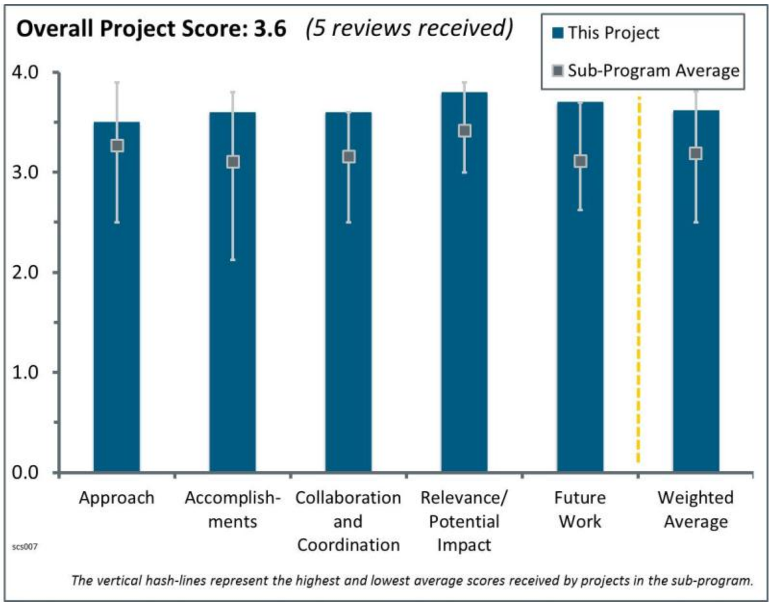
- The project should add materials modelling to establish validated material behavior laws in terms of static and cyclic performance and fracture under hydrogen and relevant loading conditions to: (1) enable fast screening of materials and (2) provide input to identify testing requirements to be included in codes and standards to better cover actual service conditions. The project should work with the ASME B31.12, ASME B31 T, and ASME NMSC committees to get the information into a venue where it will be used.
- The project should conduct notched-specimen testing of welded tube–tube samples.

Project # SCS-007: Hydrogen Fuel Quality

Tommy Rockward; Los Alamos National Laboratory

Brief Summary of Project:

The objectives of this project are to carry out the duties of the ASTM sub-committee chair for D03.14 gaseous hydrogen fuel efforts; to investigate the impacts of contaminants at the levels indicated in the Society of Automotive Engineers (SAE) J2719 and International Organization for Standardization/ Technical Committee (ISO/TC) 197 Working Group (WG) 12 documents using 2015 U.S. Department of Energy (DOE) loadings; to collaborate with international partners to harmonize testing protocols; and to develop an electrochemical analyzer to detect low levels of impurities in hydrogen fuel.



Question 1: Approach to performing the work

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

- The continuous in-line fuel quality monitor to alert to fuel grade onboard, in-stream, and in the nozzle is important for commercialization of fuel cycle electric vehicles. This technology would allow users to have confidence in hydrogen fuel quality at the pump. The research is well-thought-out, yet flexible enough to increase focus as results begin to appear or industry needs change.
- The inter-laboratory round robin of the ASTM test methods in SAE J2719 is the final step to completion. Activity on one of these methods is a good start. The in-line impurity detection device may be a useful exercise. It is of concern that the end users will not maintain (or even purchase) such devices, based on their track record with security camera systems. The cocktail work is interesting.
- The project is well-focused on evaluation of hydrogen fuel quality to ensure fuel cell life. Participation with international partners (Japan Automobile Research Institute (JARI), the European Union [EU]) and ASTM, SAE, and ISO will assist with international harmonization of fuel quality standards.
- The inclusion of an activity on harmonizing testing protocols with international partners (JARI, EU) is necessary and highly welcomed. The approach used for experimental activities is solid and correct.
- The description offered by the presenter is one of project overall task identification rather than a technical or philosophical approach. The list of projects is very appropriate and advances the topical area of fuel quality from the perspectives of standardization (ASTM) and fuel quality assurance (in-line fuel quality). This is done in conjunction with international collaboration (JARI and the EU). The category score was somewhat reduced because there was really no discussion on approach. Some discussion on approach was provided throughout the presentation—but fell short of providing the needed technical or philosophical information.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- Milestones have been completed in several areas covered by multiple standards limiting contaminants, and the project has begun development of an in-line fuel quality analyzer. Work is finely targeted to fuel quality effects and assessment.
- The presentation addressed previous reviewer comments very effectively. Experimental data are relevant to developing regulations, codes, and standards.
- Considering the cost of a full-time equivalent (FTE) plus direct costs, this project is funded at the level of about one experimental FTE. The quantity and volume of work is good for this level of funding. Nice progress has been made in overall detectability. Significant progress has been made in overall sample characteristic timing from five hours to one hour. When asked what the final response time target is, the presenter answered that the target fill time is four minutes; therefore, the response time target is four minutes. While this is an appropriate target, this answer glossed over reality. A response time of four minutes from an hour demonstrates that this technology has a long way to go. The project team successfully reduced the time by a factor of five (five hours to one hour); however, this technology needs another factor of 15 (60 minutes to four minutes). At the current response time, this maps to about 15 fills before detection is achieved. The continued work on contaminant effects on membrane electrode assemblies (MEAs) continues to be very good. The contributions to ASTM are excellent and very much needed.
- The inter-laboratory round robin of the ASTM test methods in SAE J2719 is the final step to completion. Activity on one of these methods is a good start. The reviewer does not know the status of the other 12 ASTM test methods.
- The project addresses a number of critical issues and is structured accordingly. Whereas progress has been clearly demonstrated for the development and performance characterization of the in-line analyzer and for impurity testing at low platinum group metal loadings, it is difficult to assess the progress over the last year for the standardization work, in particular that related to the follow-up of the ISO TC 197 WG 12 activities.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- The international collaboration with JARI and the EU is very appropriate and good for this work. Listed collaborations with code development organizations (CDOs) and standards development organizations (SDOs) are not all-encompassing but sufficiently broad with appropriate industrial breadth to ensure this project remains focused on hydrogen fueling infrastructure needs. The focus on hydrogen fueling infrastructure needs is appropriate since SAE and ISO have both published hydrogen fuel quality standards that are driven by fuel cell needs, to which this team contributed significantly. Attendance on the National Hydrogen and Fuel Cell Codes and Standards Coordinating Committee (NHFCCSCC) call may not qualify as collaboration.
- Collaboration with United States (US) entities is purpose-oriented and effective and includes the relevant players. Efforts targeted at harmonization of test methodologies at the international level are highly welcomed. (Collaboration with EU institutes on this topic could be framed under the recently extended EU–US Scientific and Technical Agreement.)
- Collaboration with international organizations, including JARI and the EU, is very good. This project could benefit from more regular updates (at least quarterly) with the NHFCCSCC.
- Partners are hydrogen fuel and fuel cell suppliers, international SDOs. Partnerships appear well-coordinated, with full participants.
- Collaborations are appropriate.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- The project is doing excellent work to determine analytically valid approaches to recover electrode performance following exposure to hydrogen. The project effectively incorporated DOE lower targets for platinum loading into the investigation quickly. In-line monitoring success will facilitate fuel quality and confidence.
- The implementation of hydrogen technologies needed to establish the hydrogen fueling infrastructure requires the technologies being developed in this project. Active participation in ASTM helps keep this activity well-focused and keeps the testing techniques established by ASTM aligned with the needs of the industry. While the code community is debating the question of "how to guarantee that the fuel quality as established by SAE J2719 is delivered to the vehicle," the development of an in-line detector is very relevant to answering the question and providing a possible technology to satisfy this need.
- This effort is critically needed for enabling deployment of fuel cells in transportation.
- Hydrogen quality and international harmonization of hydrogen quality assessment tools are necessary to deployment of fuel cell technology.
- The round robin and the cocktail work are highly relevant and have the potential to be high-impact. The sensor work may have acceptance issues with station operators.

Question 5: Proposed future work

This project was rated **3.7** for its proposed future work.

- Future work is well-thought-out and includes significant collaboration. It is good to see plans for international round robin testing and to see consideration given to testing to SAE standards. It is excellent to see the project begin incorporating recirculation effects.
- The proposed future work and direction are good. Some thought should be given to the sample technique for the in-line detector to make sure the overall system response time is appropriate for a four-minute fill. It is important to consider this system notion rather than just the detector itself. This system design needs to, of course, consider detector temperature and pressure of operation as compared to the fill protocol as specified by J2601.
- Proposed future work is a continuation of improving test methods and hardware, improving standards, and expanding international collaboration. The proposed work is needed to harmonize standards and provide technical data and evaluation tools.
- It is recommended that the project include, from the outset, international partners in the efforts (workshop and subsequent experimental activities) on hydrogen storage system cleanliness. Interaction with JARI and the EU should be strengthened, including the identification of commonly agreed loading cycles (stressors) representative for automotive applications, in addition to the harmonized test protocols which should be expanded from MEA to stack level.
- The proposed future work makes sense.

Project strengths:

- The fundamental notion presented by this technology could prove to be very valuable to hydrogen fueling infrastructure to guarantee fuel quality meets the desired specification for each fill. The current state of this technology is appropriate and showing promise.
- Strengths include technical data development and strong international collaborations.
- The project addresses many issues that are critical path to a hydrogen infrastructure.
- Every year this project reports progress in evaluation of fuel quality and the effects of contaminants.
- The project has a clear focus.

Project weaknesses:

- Attention needs to be given to the sample system when assessing the response time and the quantity of hydrogen needed to sample for detection.
- The lack of reference and apparent support of SAE J2719, which is the fuel standard for the US, is not understood. ISO documents are not adopted in the US as state regulation.
- This project could benefit from more regular national outreach/feedback opportunities.

Recommendations for additions/deletions to project scope:

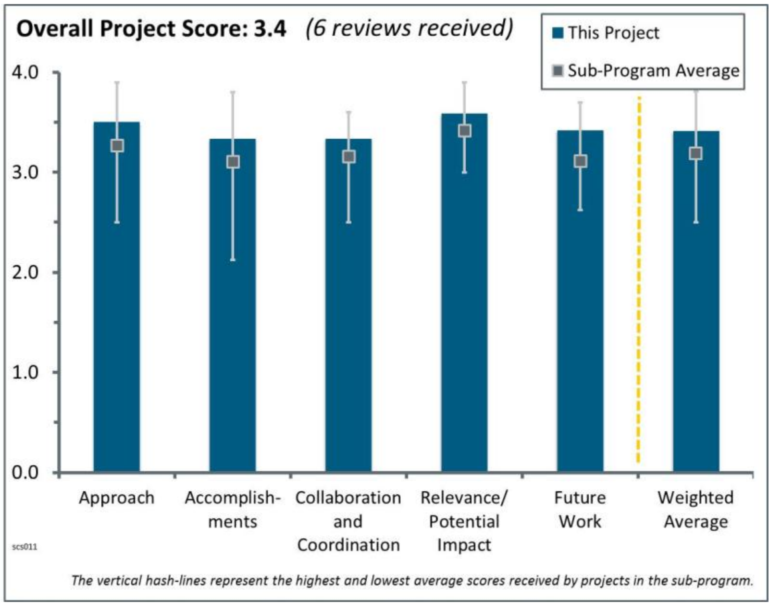
- The project should increase the pace and scope of the round robins to quickly validate the ASTM test methods. The team should consider applying the impurity sensor in a manner suitable for the state regulators to do spot testing at stations during the metrology/inspection visits.
- SAE J2799 compliance testing might be a good future addition.
- The project might extend international harmonization of test methods to include commonly agreed representative automotive loading cycles. The team should extend testing from MEA to stack level.

Project # SCS-011: Hydrogen Behavior and Quantitative Risk Assessment

Katrina Groth; Sandia National Laboratories

Brief Summary of Project:

This project provides a science and engineering basis for assessing safety (risk) of hydrogen systems and facilitates the use of that information for revising regulations, codes, and standards (RCS) and permitting stations. The project goals are to develop and validate hydrogen behavior physics models to address targeted gaps in knowledge, to build tools to enable industry-led codes and standards revisions and safety analyses, and to develop hydrogen-specific quantitative risk assessment tools and methods to support RCS decisions and to enable a performance-based design (PBD) code-compliance option.



Question 1: Approach to performing the work

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

- The chosen approach has struck a fine balance between theory and practice. The approach has a strong focus on quantitative risk assessment (QRA) and impact on RCS through participation in National Fire Prevention Association Standard 2 (NFPA2) and the International Energy Agency's Task 31, etc.
- Use of coordinated activities in applying research and development (R&D) in RCS, QRA methods and tools R&D, and hydrogen behavior R&D increases relevance and usefulness of project output.
- Based on input/questions at the merit review, there appears to be room to expand literature research and verify whether previous research has covered some of the topics under investigation in this project/program.
- It is unclear how benchmarking from Sandia National Laboratories (SNL) leads to an 18% increase in station readiness. It is not certain that there are code officials who agree with this number, nor is it clear how the QRA information is currently being used/applied to safety, codes and standards (SCS).
- The project draws on efforts and deliverables from a number of sub-projects that were presented individually in previous AMRs. In that respect, it is not straightforward to judge on this question. However, for the main activity of the project, namely the development of Hydrogen Risk Assessment Model (HyRAM), the approach is scientifically sound. It is unclear how the information provided on slide 5 (metric) relates to the overall project. This seems a standalone activity aimed at providing a post-hoc justification of the work for HyRAM. Such a justification is not needed.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The project has the following accomplishments: (1) demonstrating performance-based design in NFPA 2, (2) helping socialize the PBD approach with industry and authorities having jurisdiction (AHJs)—which is excellent work, and (3) developing a curved flame model that better represents physics and shows lower heat flux as the flame curves up, allowing for reduced separation distances. The project addressed past reviewer comments very well.

- Metric benchmark development is good accomplishment, but based on "hydrogen-targeted California sites," a 0% to 18% improvement sounds like giving too much credit to this effort alone when applied to reality. It is not clear which "sites" are targeted (because it depends on context of station owners' willingness, site selection process, AHJ requirements and use of codes and standards in jurisdictions, additional requirements, etc.). Regarding the curved flame model, the maximum height at which this model is applicable is not stated. Ground-level release downstream heat flux may exponentially change at higher heights, owing to atmospheric influences not accounted for (wind, etc.), so this information may be helpful in adding to model presentation and interactions with AHJs/design engineers. Other accomplishments are good progress!
- The project is well-directed to apply QRA to PBD, but it is not clear how the developed QRA tools are being verified and validated, nor how they compare with other QRA tools used by industry—not necessarily for hydrogen but for oil and gas as an example. One should bear in mind that operators do not typically approach universities/research laboratories; they turn to some established consultancy organizations such as the DNV GL Group in Europe and Bakers Engineering in the United States. Hence it is not clear how all these can feed to the evolving hydrogen industry, how the knowledge and tools will be transferred to industry, and how industry can be convinced to use these tools. The R&D work on curved flame model is based on solid fundamental research and should lead to simplified correlations that can be used in QRA. The presentation provides insufficient details about the maturity of the overpressure model; essentially, this is a collection of previously developed simplified models by SNL and others. Care should be taken about the range of the validity of each model and its integration/selection in an overall QRA. It is also of concern that hydrogen ignition probability is derived from industry data on hydrocarbons. Research and accident databases have shown that the probability of hydrogen ignition is much higher than with hydrocarbons – this is also consistent with the fundamental combustion-related properties of hydrogen.
- The information provided in the slides and during the AMR presentation demonstrates that progress has been made for HyRAM, as well as in some aspects of behavior and consequence modeling. For the latter, however, it is difficult to identify the amount of progress that has been realized in the last year. (The last slide in the set reserved for the reviewers only identifies one milestone in fiscal year [FY] 2013; all others are in the future.)

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The project has excellent international collaboration. Commendable work is being done with AHJs and the international community. It is good to see collaboration with H2USA. There is direct input into developing RCS.
- Effective collaboration exists with some limited industrial organizations. The data exchanges with Air Products, Linde, and SRI are encouraging and beneficial to the project aims. Collaboration with HySafe should benefit activities in this area on both sides of the Atlantic and eventually avoid duplication of efforts. Obviously, there are additional benefits in publicity and knowledge sharing. Collaboration with more operators of refuel stations in addition to Linde and car manufacturers should be pursued in the future. It is not clear what value the collaboration with Tsinghua University is adding to the project. There is no doubt that the institution is top-class, but the quoted academic does not appear to have a track record in the subject and is relatively unknown to the community in terms of jet and jet fire research.
- Collaboration with U.S. entities seems to be purpose-oriented and effective. The scope, intensity, and impact of collaboration with non-U.S. partners cannot be judged on the basis of the information provided.
- The project should consider collaborating with the Energy research Centre of the Netherlands (ECN) and German counterparts in addition to HySafe.
- Two private companies (Linde and one other) have provided feedback, but their participation and interaction was not highlighted significantly enough to warrant a higher score.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- This work is critical to modifying relevant documents to allow reasonable requirements while maintaining safe hydrogen filling stations.
- Once validated and proven easily accessible, the HyRAM toolkit will be a major enabler for a risk-informed PBD for a number of hydrogen applications. As such, it clearly directly contributes to the DOE program goals.
- The potential impact is excellent overall, but critical uncertainties remain, as described in the presentation.
- There is scope for improvement. However, it will have positive impact on the DOE R&D goals and objectives in the hydrogen and fuel cell area.

Question 5: Proposed future work

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

- The proposed future work is relevant to DOE R&D goals and objectives in the hydrogen and fuel cell area and in line with the original objectives of DOE funding for SNL. The plan for FY 2014 is basically a continuation of the work reported here. The proposed development for liquid/cryogenic hydrogen experimental capability should be of particular relevance to the immediate needs of the industry and contribute towards filling some knowledge gaps in the area.
- The proposed future work follows logically from the past and ongoing activities. Although external financial support for future work is obviously useful, this should not be a *conditio sine qua non* (should not be indispensable).
- Proposed future work is consistent with DOE Hydrogen and Fuel Cell Program goals, industry needs, and needs of developing RCS.
- The project should continue working with industry partners to address the heavy near-term needs.
- There are more critical uncertainties than there is DOE funding.
- Planning/mapping activities/goals could be better defined in the presentation.

Project strengths:

- Addressing challenges with siting hydrogen stations at gas stations is an immediate need. It is good that the project highlights that PBD can be an alternative.
- Project strengths include facilitating use of the safety data and information, and demonstrating evidence of supporting market growth (a number of sites that can readily accept hydrogen).
- The work absolutely has the potential to affect code in a positive way (reducing quantity-distance restrictions, thus making fueling stations fit better in current footprints). The largest hurdle is going to be getting code officials to understand this QRA approach and to adopt it. The current project does not have a planned goal for this, however.
- The project has excellent industry partners and is working on key areas.
- The project has a solid science-based approach towards the establishment of a powerful tool for facilitating (1) improvement of RCS and (2) PBD. This avoids subjectivity in the assessment and contributes to enhanced confidence of AHJs in the application of this approach, which will in turn promote deployment of hydrogen systems.
- The project has very good relevance to RCS development. There is a strong focus on QRA and PBD. The work is well-publicized and -disseminated.

Project weaknesses:

- There is a lack of resources for diverse objectives and a multitude of gaps that require future work.

- Critical uncertainties remain, but the project is filling gaps as well as possible, utilizing partners and proposing future work.
- It is not clear whether the claimed accomplishment about having a toolkit is finalized. The project hosted a meeting, received input, and updated the model, but it is not clear who can currently access the toolkit and use it for their application. This was ostensibly part of the goal.
- H2First is currently not making fast progress, which may hinder project progress/value overall, with many stations being funded for construction in the coming two years.
- It is not clear how validation and verification of the models is handled. Collaboration with more operators of refueling stations in addition to Linde and car manufacturers should be pursued in the future. There is no clear plan for how the models/QRA tools developed will be exploited by the end-user community.

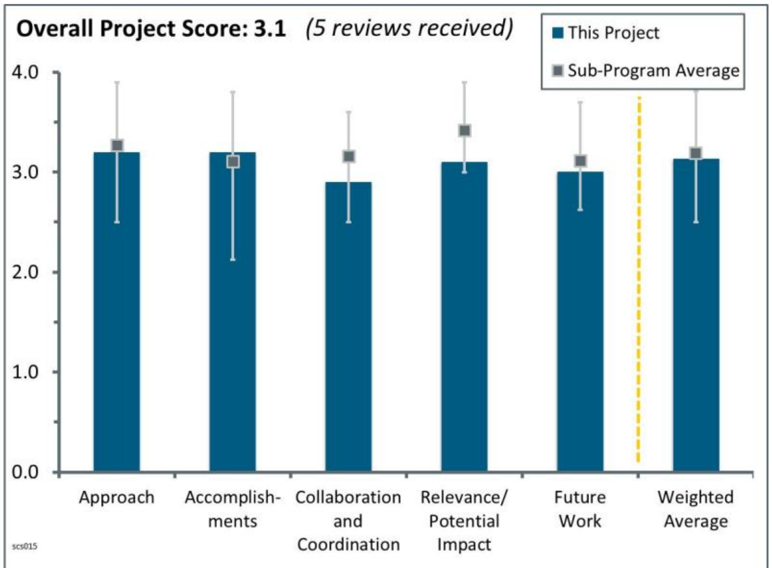
Recommendations for additions/deletions to project scope:

- The project should: (1) consider what underground liquid hydrogen storage can do to mitigate separation distances, (2) expand collaborations (ECN and Germany), and (3) explain this project's future potential by defining the maximum percentage of targeted gasoline stations that could be available to house/host a hydrogen station, based on findings and codes and standards limitations.
- The project should clearly identify future participants (who the AHJs that are going to use this toolkit are) and the timeline to complete the teaching and hand over the toolkit. Targets need to be better clarified/defined with clear metrics. The project should clarify who is working on approving the alternative compliance (PBD) from a codes perspective and how this is being planned/mapped.
- This may not actually constitute an addition to the project scope, but from the information provided, an explanation is lacking on how data from (validated) behavioral and consequence modeling are actually transferred to the QRA module, realizing that the model outcomes are affected by assumptions for initial and boundary conditions that may quite well differ from those in the actual case considered.
- The liquid hydrogen work should be explicitly included in the plan for out-years.

Project # SCS-015: Hydrogen Emergency Response Training for First Responders

Monte Elmore; Pacific Northwest National Laboratory

Brief Summary of Project:



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

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 (ER) information to first responders. Objectives for fiscal year 2014 include: (1) developing and implementing a national hydrogen ER training resource with downloadable training materials that are adaptable to the specific needs of first responders and training organizations, and (2) exploring mutually beneficial collaborations with other programs and organizations to enhance first responder training content, techniques, and delivery.

Question 1: Approach to performing the work

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

- The online and in-person approach is balanced and useful.
- The approach for a national template is good.
- The project is getting "old," with little new to report. It might be time to look for ways to end the project and/or transition resources to a new project that can look for fresh ground to cover.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- The project has made great progress and is headed in the right direction.
- The training events are encouraging. However, no training appears to have been done in 16 months. It would have been better to see more training events and refresher training.
- Overall progress since 2004 is good, but progress in the last couple years has stalled. There are notable successes in terms of a few training classes provided, but this is anecdotal and sparse at only two per year. It is not certain that the effort is worthwhile. A good indication would be from: (1) feedback from class participants, and (2) the ability to charge for the service. Then it will have demonstrated value.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The team seems well-connected to relevant stakeholders.
- There are documents within standards development organizations (SDOs) being created for first responders. It was not mentioned in the presentation, but if needed, aligning these is recommended.
- There does not seem to be collaboration in the setting up of the training materials for ER—at least, it is not apparent from the information provided. The presentation and presenter indicate collaborations, but the identified partners are recipients of the ER training rather than collaborators. It is unclear to what degree

feedback from the training recipients or from the partner organizations organizing the training is included in revising/updating training materials. The opening up to the European Union through the interaction with HyResponse is welcome.

- The collaboration and outreach do not appear to be reaching the target organizations. Several things are not clear: (1) what, if anything, is being done to reach out to the local fire departments and police departments; (2) whether the project team is working through the trade organizations; and (3) whether the project team is working with and through the state public safety departments.
- Collaboration is shown with “name brands,” but it is not clear that these organizations can really move this forward. A collaboration with a future owner of this material is needed.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- This work will be relevant for the next decade.
- The project definitely contributes to enabling further deployment of hydrogen technologies.
- This activity is highly relevant to a hydrogen infrastructure.
- This does not add critical value to the DOE goals of establishing a hydrogen infrastructure. If this program went away, few would notice, and the need would be met elsewhere.

Question 5: Proposed future work

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

- Apart from finalizing the national program, the proposed further work seems to be targeted at increasing the number of recipients of the ER training. The project should actively exploit the experiences collected in the “lessons learned” included in http://h2tools.org/lessons/ for inclusion in the ER training materials. Vice versa, the project should check on a continuous basis any exposure of emergency responders to a hydrogen-related event and collect/evaluate their experiences for updating the training materials and for inclusion in the H2tools hydrogen-lessons-learned (H2LL).
- The future work does not appear to be creative in getting people interested in training, nor does it appear to have a sense of urgency.
- There need to be more specific goals for future work. As-is, the project will not add much on-going value.

Project strengths:

- A very thorough program was created with an appropriate approach.
- The project addresses a demonstrated need.
- The material is useful and helped fill a void, but it needs to transition to a long-term home.

Project weaknesses:

- It seems that the project could use more outreach for participants. The reviewer hopes the increased funding will contribute to increased awareness and participation.
- If H2USA is intended as the link to industry feedback, then the project leads should have a more consistent message that demonstrates this link.
- Weaknesses include a lack of funding, the need to make training materials more accessible and coveted for stakeholders, and the lack of a more proactive approach and stakeholder engagement strategy.
- This cannot be the long-term forum to provide this information. An “owner” or “sponsor” should be found who is willing to take over this information and present in the future.

Recommendations for additions/deletions to project scope:

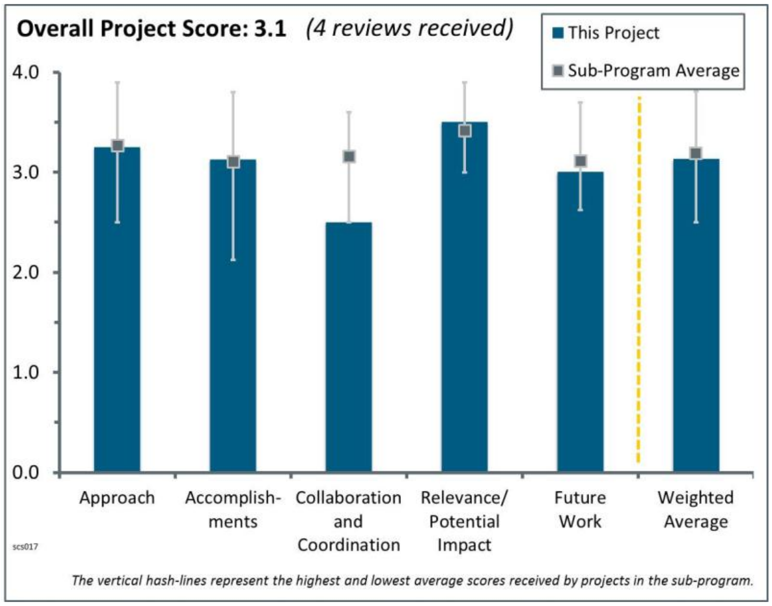
- The project should make two additions: two-directional interaction with H2LL from H2tools and interactive virtual reality tools.
- The project needs more creative outreach, more training with the goal of increasing the number of students each quarter, and refresher training.
- The project should ensure training material aligns with new SDO documents and modify the material appropriately.
- The project should find an organization willing to own this going forward and/or start charging for the service to make this cost-neutral.

Project # SCS-017: Hands-On Hydrogen Safety Training

Salvador Aceves; Lawrence Livermore National Laboratory

Brief Summary of Project:

The overall objective of this project is to develop a hydrogen safety training program and instructional materials for laboratory researchers and technical personnel. During 2013/2014, the goal is to develop classroom materials for a hands-on training course that includes comprehensive instruction on components, system design, assembly, and leak testing.



Question 1: Approach to performing the work

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

- Hands-on training in hydrogen safety is of tremendous practical use, and having such a course is a definite need for the Fuel Cell Technologies Office (FCTO). As part of this, handling of hydrogen at high pressure is extremely important. This course, however, seems to be more pressure-based than hydrogen-based. This is not altogether a bad thing, as non-hydrogen compressed gases are often utilized in the same process as is hydrogen gas, and learning how to handle them is important. However, it seems that it is too large a part of this training program.
- The project is certainly a good use of available resources in Lawrence Livermore National Laboratory (LLNL) expertise. The class length is long. It is nice that there is a website for referral.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- The attendance/participation is consistent. The use of hands-on training aids is very good.
- Again, the development of the hands-on training instruction seems too generic. While learning handling of high-pressure gases in general is useful and should not be eliminated, there needs to be some focus on hydrogen and how it differs physically and chemically from other gases, and what special care is necessary. One hopes this will be remedied during review by the Hydrogen Safety Panel (HSP). While the presenter’s statement—that the all-inclusive nature of the class (covering all compressed gases) is needed because no one else is doing it—may be accurate, focus should not be lost.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.5** for its collaboration and coordination.

- The HSP review of the classroom work was a good approach, and appears that it will continue for the new current training. It is not clear how review by “laboratory safety personnel” will provide assistance to a potential industrial hands-on training class, nor whether this refers to LLNL safety personnel or a wider group.
- The reviewer agrees with comments during the presentation about getting this out to a larger audience—universities, other laboratories, etc.
- As noted over at least two DOE Hydrogen and Fuel Cells Program Annual Merit Reviews (AMRs), coordination with other entities doing similar work would be greatly beneficial.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- Relevance to the success of the overall FCTO is large in that the training promotes hydrogen safety and reduces the possibility of a catastrophic high-pressure hydrogen event; such an event would be tragic in itself, and could also derail the DOE Hydrogen and Fuel Cells Program. It is important that focus remains.
- Safety is always important but seems especially so with hydrogen, given its imminent commercialization and use by the general public.

Question 5: Proposed future work

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

- The project should focus on finishing the hands-on safety class! The web-based class should need only minor updates, if any. This type of program is needed, but progress is slow. The project team should check with industry/universities to see whether there is a demand for such training, as the number of people being trained is trending lower.
- Development of the promotional materials is certainly a valuable addition—the project could even consider the social media route!
- It looks like a sound path forward.

Project strengths:

- LLNL has extensive expertise in high- and very-high-pressure work with gases. The web-based class was quite good and appears to have been well-received.
- This project is important for safety in the industry. The approach—having the web-based and hands-on safety class—is nice.
- It seems there is a need for this type of training.

Project weaknesses:

- The project should get a project vision and find out how to transition out of the laboratory. This valuable work is "hidden behind a fence."
- LLNL is a good organization for developing this training but not for disseminating it.
- There is some concern about the degree of focus on hydrogen gas and hydrogen systems. While the more generic high-pressure material is important as well, it should not overwhelm the effort.
- The progress seems slow for over four years of work, and outreach is insufficient.

Recommendations for additions/deletions to project scope:

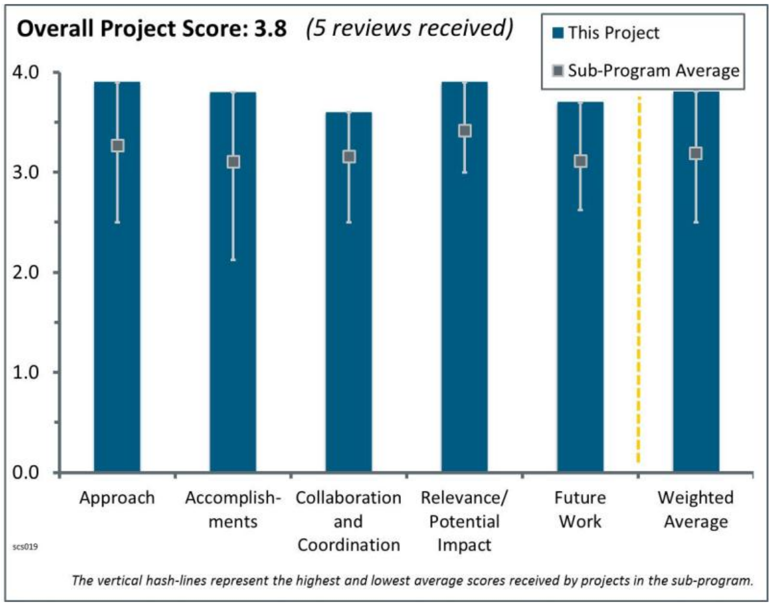
- The project team should certainly consider the suggestions from AMR attendees of performing a "train-the-trainer" and passing it off. The project should get this onto the web portal with the HSP and the emergency response material as a resource.
- An exit strategy and long-term vision to provide to the industry are needed.
- The project should either be funded adequately to complete the objectives or allowed to unwind if there is no industry need for training.
- The project should include teachings on how hydrogen is different. Sanity checks on the course curriculum should be kept going by including periodic reviews by either the HSP or other experts.

Project # SCS-019: Hydrogen Safety Panel and Hydrogen Safety Knowledge Tools

Nick Barilo; Pacific Northwest National Laboratory

Brief Summary of Project:

This project provides expertise and assists in identifying safety-related technical data gaps, best practices, and lessons learned through a hydrogen safety panel. The panel also helps integrate safety planning into U.S. Department of Energy (DOE)-funded projects. Safety knowledge tools are a collection of information and lessons learned from hydrogen incidents and near misses, with a goal of preventing similar safety events from occurring in the future. The tools also capture a vast and growing knowledge base of hydrogen experience and make it publicly available to the hydrogen community and stakeholders.



Question 1: Approach to performing the work

This project was rated **3.9** for its approach.

- The Hydrogen Safety Panel (HSP) part of this project is spot on and receives a score of 4.0. The motivation and work of the panel is to ensure that DOE-funded programs adhere to a high degree of safety, identify gaps, and report to DOE. Changing the approach to reviewing early in the project life will yield much more powerful results, making sure that the project operates with the highest degree of safety for as much of the project duration as possible. The knowledge tools part of this project is also very good and receives a 3.5. A one-stop shop that is portable-device-enabled in today's mobile environment will make the work of DOE and others in regulations, codes, and standards (RCS) readily available in a convenient format. Moving this tool to a portal makes this no longer just available to Apple devices (iPhone, iPad, etc.) but enables the use of all devices (Android, etc.). This is perfect. This activity needs to embrace collaboration with others who are working in similar areas (the Fuel Cell & Hydrogen Energy Association, the National Renewable Energy Laboratory, etc.) to strengthen the product and remove duplication of effort.
- The project does an excellent job working with DOE and developing strategy to have the needed tools in place for next stage of commercialization. The interaction on real projects is the key to success.
- Expanding use of the expertise on the panel from last year is not only good utilization of a valuable resource but also shows growth and flexibility as hydrogen and fuel cells are commercializing.
- The size and diversity of the HSP are a positive. Safety does have to be treated as a continuous process. The reviews and incidents and best practice databases are complementary functions in disseminating safety information and getting current information out to new projects before a problem exists.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.8** for its accomplishments and progress.

- The HSP portion of the project receives a 4.0. Over and above the required safety reviews, some of these activities are exactly the type in which the HSP should be engaged, e.g., reaching out to other projects, such as the trip to Flint Michigan to visit a fueling station and repair garage. This activity helps to extend the panel's impact and at the same time brings back to the HSP experiences of those who are deploying the hydrogen technologies in the early stages of the infrastructure rollout. Branding the HSP is a great idea and should help a great deal to improve the visibility of this resource. The HSP has responded well to previous reviewers' comments for outreach. The tools portion also receives a 4.0. It is a minimally funded project (~0.3 full-time equivalents [FTEs]), but even so, its accomplishments are good. The creation of the one-stop shop mobile device tool via a portal will help the community access this information in a more convenient manner, and it is already showing this success through the number of hits received to date. In addition, this project motivated the first live webinar presented at the Fifth International Conference on Hydrogen Safety (ICHS5) meeting in Brussels—which proved to have the largest attendance of all webinars held by DOE to date. This activity has done its due diligence to answer the question: "Do we have the right tools?" The workshop held earlier this year had an impressive array of appropriate people. The output from this workshop should prove to be very powerful. Much has been accomplished with this minimal level of funding—the community is getting a very good bang for the buck.

- The reach of the panel—providing input to the project, identifying issues, and offering solutions—has been outstanding this past year. The project should keep on that path. The branding is excellent; it will be interesting to see results next year—good to include on Linked In! (Other laboratories/projects should follow the lead.) There has been a positive outcome of more frequent meetings; the project should continue the meeting frequency. Regarding safety knowledge tools, the Electronic Safety Resource Tools Planning Session provided 136 unique ideas for resources! That is a wonderful outcome and includes *new* ideas that are contemporary and take advantage of current methods to disseminate knowledge and share learnings. Another excellent accomplishment is the *NFPA Journal* article (May/June 2014) and emergency response education/training with the U.S. Fire Administration.
- It is excellent to see early project engagements of the HSP on early design reviews. Early stages of branding the panel should lead to increased awareness and use, leading to better safety knowledge in future projects.
- Safety is the most important goal of any new technology. This project continues to provide comprehensive safety information, and the project improves upon previous methods for getting the information out to wider audiences.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- It is commendable that coordination includes general outreach, such as a webinar and two papers at an ICHS conference, as well as specific outreach to HSP participants, national laboratories, the International Energy Agency (IEA), and project developers and reviewers.
- Much effort is being made to reach out and collaborate—and not just on the receiving end but also in acquiring information.
- The HSP section receives a 4.0. This activity does not really lend itself to embrace collaboration in a traditional sense. The tools section receives a 3.0 and is intended to be an all-encompassing collection (or access tool) for the community; however, there are some significant gaps in the collaboration that have led to gaps in the resources being utilized and/or duplication of effort. This team needs to reach out and include in this work activities of FCHEA and NREL.
- NFPA, the International Code Council, and the U.S. Fire Administration are included under this section, as are the IEA, Joint Research Conference Sandia, as HSP affiliates as well as authorities having jurisdiction (AHJs) on a case-by-case basis.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.9** for its relevance/potential impact.

- The HSP portion receives a 4.0. The basic motivation is to ensure that DOE projects are performed as safely as possible and are executed in keeping with hydrogen safely best practices—spot on! The tools portion also receives a 4.0. These tools enable a one-stop shop to make the vast safety, best practices,

permitting, etc., available to the professional needing this information in an easy comprehensive manner—spot on!
- The project very effectively takes on Hydrogen and Fuel Cells Program goals and objectives and puts real-time solutions directly into the hands of those who are deploying hydrogen technologies.
- The wealth of knowledge and expertise of this panel is highly valuable. In working together with other projects (NREL's, for example), it is a powerful resource!
- This is a comprehensive safety assessment and outreach program—very nice.

Question 5: Proposed future work

This project was rated **3.7** for its proposed future work.

- Development of the tools, outreach plans, and direct engagement with project developers and those seeking to review projects is all excellent. The presenter referred to a desire for the hydrogen safety web portal to be a one-stop shop for credible, reliable safety information. This is also objective of the FCHEA hydrogen and fuel cell safety report website. People who are looking for safety information when considering a hydrogen or fuel cell project are likely to look to FCHEA for information. The project lead should consider ways to utilize the FCHEA website as an outreach mechanism.
- Proposed future work is a continuation of the same necessary activities and improved education and outreach. This is necessary work.
- The HSP portion of the work receives a 4.0. This is right on track. The tools portion also receives a 4.0. Given the minimal funding this project has, the future plans are appropriate.
- For the white papers (and other input to safety, codes and standards), the project should coordinate or work with organizations and working groups such as FCHEA and the U.S. DRIVE Partnership's Codes & Standards Technical Team. (This may be happening now, as some panel members are also members of these other groups.)

Project strengths:

- The HSP portion has proven to be an excellent resource for the DOE programs and the hydrogen community at large. Indeed, imitation is the best form of flattery—the International Association for Hydrogen Safety's HySafe is working to create a similar tool under the auspices of the Fuel Cells and Hydrogen 2 Joint Undertaking. Regarding the tools portion, the notion of a one-stop web-based tool accessible from all the mobile devices now in general use is very good. This should prove to be a very valuable resource to those deploying hydrogen technologies.
- The team has done a great job organizing an approach, workflow, and execution to meet the real barriers that exist today.
- This project is showing real innovation and forward thinking in how best to utilize this valuable resource!
- Strengths include outreach with insurance groups and AHJs to better understand user needs.

Project weaknesses:

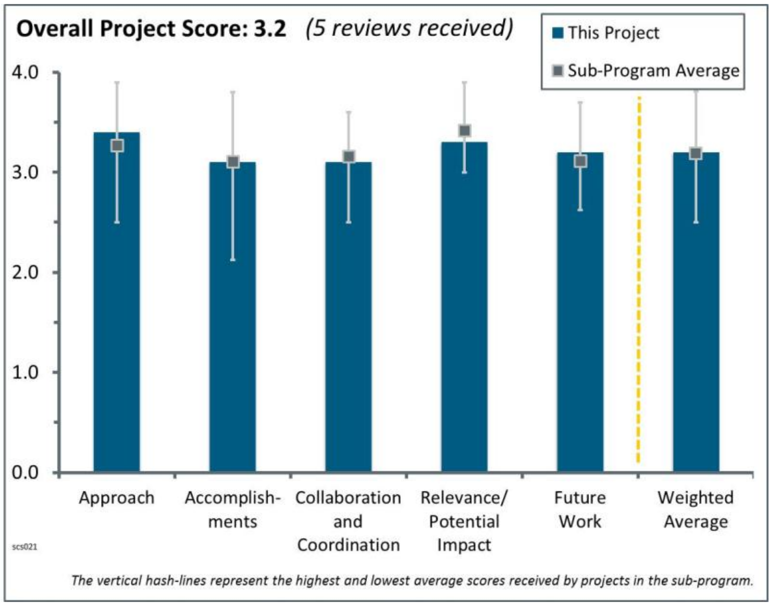
- Excellent tools are being developed. This reviewer wonders whether more could be done to promote the tools, such as more interface outside the hydrogen community. The article in the *NFPA Journal* is an excellent start.
- This is not so much a weakness as a suggestion: it would be good to see the HSP continue to look for areas where its enormous expertise can provide value. Regarding the tools portion, the team needs to work harder at being more inclusive with collaborators and the efforts of others so as not to duplicate effort or create gaps.
- This is not so much a weakness of the project but a general observation: there seem to be some repeat efforts in similar arenas, so DOE needs to align these.
- It may be just a temporary glitch, but the reviewer was having difficulty with the incidents database the day before the review. The search was not working properly.
- Funding for the hydrogen safety panel at over six times the safety knowledge tools portion is questionable. The funding for the HSP seems excessive for the scope of work.

Recommendations for additions/deletions to project scope:

- The project should consider working with FCHEA to avoid duplication of effort with fuel cell safety report website goals. The project should begin to consider long-term possibilities of safety reviews for projects without DOE funding.
- The project should revisit HSP tasks and future work and adjust the budget accordingly. The funding amount is large and does not seem to be used efficiently.

Project # SCS-021: National Renewable Energy Laboratory Hydrogen Sensor Testing Laboratory

William Buttner; National Renewable Energy Laboratory



Brief Summary of Project:

Sensors are a critical hydrogen safety element and will facilitate the safe implementation of the hydrogen infrastructure. Through this project, the National Renewable Energy Laboratory (NREL) sensor laboratory tests and verifies sensor performance for manufacturers, developers, end-users, and standards development organizations. Information collected on sensors through testing is used to support codes and standards (C&S) development and improve sensor performance with manufacturers.

Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- Independent evaluation of sensor performance is critical to providing confidence and verified sensor performance. Indeed, the collaboration with the sensor testing facility in the Joint Research Centre (JRC) helps to leverage resources and to make sure that the data obtained has a high degree of confidence. Attention is given to ensure this knowledge is provided to the C&S development activities. The attention to client confidentiality is necessary and excellent.
- The approach followed by the project correctly covers a number of relevant technical activities, as well as interaction with industrial stakeholders and with standards development organizations and regulators.
- Sensors to detect hydrogen leakage seem to be a worthy cause. Present sensors have reliability, durability, and cost issues.
- The project provides independent assessment of hydrogen sensor performance. Efforts are now qualifying sensors for specific applications. This activity is somewhat integrated with other efforts but tells only part of the story.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- With effectively one full-time equivalent (FTE) of effort, this project’s accomplishment are substantial, particularly when it is recognized that the new facility just came online and the new testing laboratory was recently moved and approved for operation. During this laboratory move and configuration, the principal investigator remained active and produced a number of relevant publications, lectures, and presentations (12) and co-authored a book. There needs to be some caution in measuring the output for this funding period since the laboratory is just now coming back online. Though some claimed accomplishments refer to work that is forthcoming, the accomplishment noted is one of establishing the relationship—this, in and of itself, is indeed an accomplishment (it shows initiation of many projects). The reviewer looks forward to seeing concrete output in the coming years with the new collaborations/partnerships and the new facility. The absence of concrete output from these new relationships is the reason for a score of 3.5 instead of 4.0. This rating is simply a result of the drop in concrete results due to the move and laboratory restart.

However, the work on hydrogen sensing by looking at O_2 displacement —the Global Technical Regulation (GTR)-driven investigation—is particularly impressive. These results are critically important technically and critical to ensuring that the GTR and other codes do not make the same error that Phase I of the GTR did. This is outstanding!

- The project clearly contributes to the high-level DOE goal of safe deployment of hydrogen technologies. Progress has been made in the integration of the experimental project activities in the Energy Systems Integration Facility.
- The focus appears to be having sensors on vehicles, but stationary applications may be more relevant. For example, many state building codes require fire (and carbon monoxide) detectors in a home. It would not be a stretch to assume a hydrogen sensor will be required in a private domicile garage.
- There is some correlation between this project and the DOE barriers listed on slide 2; however, this project is focusing on very specific technology rather than the broader analysis to determine whether this is the right technology to focus on. While much work has been done, there seems more yet to do than progress already realized. This is expected to be the case in basic research projects. This project seems more basic research than facilitating deployment.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The collaboration/partnerships with the private sector (five partnerships and agreements) are very good—this presumably refers to sensor manufacturers to be tested. The government collaborations are all appropriate and good; particularly impressive is the continued relationship with the testing facility at the JRC. It is recommended that outreach be made to the Asian community to secure collaboration there. The new facility in Japan (HyTrec or I2cner at Kyushu University) and facilities in China should be investigated as potential collaborators.
- There is good outreach through publication of a book, technical reports, and peer-reviewed journals. Collaborations with Europe, the International Partnership for Hydrogen and Fuel Cells in the Economy (IPHE), Parker Aerospace for the aviation market, and the JRC in the Netherlands are all commendable. Collaborations with Bundesanstalt für Materialforschung und –prüfung's (BAM's) Fuel Cells and Hydrogen Joint Undertaking, as well as NREL, and H2Sense participation, are noteworthy. The project should have close coordination with fuel cell electric vehicle original equipment manufacturers (OEMs). The presenter responded to previous review questions by saying sensors are critical because other hydrogen detection technologies do not exist. At this point, hydrogen-specific sensors listed to Underwriters Laboratories (UL) 2075 do not exist either. It is not clear that the car manufacturers are saying what they really need is a cheaper reliable hydrogen sensor. Perhaps it would be beneficial for DOE to look at this project in the context of the overall aim of safety. Hydrogen-specific sensors may or may not be the ultimate solution. Perhaps there should be more research in alternative technologies that can achieve an equivalent level of safety to the UL-listed hydrogen-specific sensors that do not exist.
- There is a purpose-oriented interaction and collaboration with a number of relevant entities, including non-U.S. ones, as demonstrated by an extensive record of joint publications and presentations.
- The collaboration does not appear to be suitable to going forward. Collaboration with instrument manufacturers and building code officials may be a better approach for both requirements (and features) and acceptance.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- This project supports hydrogen vehicle repair facilities (sensors mandated by the International Code Council [ICC]). The expected significance is a hydrogen sensor for a turn-key safety system. Regarding NREL support of the Department of Transportation's National Highway Traffic Safety Administration on hydrogen safety requirements for Federal Motor Vehicle Safety Standards, the testing showed oxygen

depletion is not a suitable detection method for vehicles. It is not clear whether this assertion has been vetted with vehicle OEMs. Slide 3 says sensors for safe hydrogen deployment are mandated by National Fire Protection Association (NFPA) 2 (Sections 10.3.19.1 and 3.3.219.2.2). There is no Section 10.3.19.1 in NFPA 2 (2011 published edition). This may be a typographical error, as there is a Section 10.3.1.19.1, which states that "dispensing equipment shall be provided with gas detectors, leak detection, and flame detectors such that fire and gas can be detected at any point on the equipment [52:9.2.1.14]." It is worth noting that hydrogen-specific sensors are not mentioned. NFPA 2 Section 3.3.219.2.2 is a definition, not a requirement. The NFPA 2 code does not mandate hydrogen-specific sensors. The ICC's International Building Code (IBC) does have more specific requirements. It is very important to note that IBC requires that sensors be listed in accordance with UL 2075. There is no mention of UL 2075 in this presentation. If there are no hydrogen-specific sensors capable of being listed in UL 2075, it might be better to focus work on updating the code text rather than continued long-term testing of sensor technologies.
- This activity is relevant. The potential impact for the stationary market would be positive.
- Point sensors in hydrogen applications are inappropriate and indeed can lead to a false sense of security and safety. The issue is not with the sensor but with being able to identify a leak in an environment where one cannot know the location or direction of a leak. Sensors are required by code, however (NFPA, ICC). With this said, it is good to see this project investigating wide-area detection—which, if it can be made cost-effective with suitable detectability in space and in concentration,—will solve these concerns.

Question 5: Proposed future work

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

- The direction of this work remains focused on the growing needs of sensor technologies—particularly wide-area detection (monitoring). The continued collaboration with the laboratory at JRC remains a very good aspect of this work. The interaction with the industrial community to help ensure safe operation and appropriate use of sensors is good and necessary.
- The proposed future work is aggressive and all-encompassing. It appears to be encroaching upon the work done by the Nationally Recognized Testing Laboratories (NRTLs). It is not clear how the project is addressing this or addressing the liability associated with testing designs generated by commercial interests.
- The proposed future work is a logical follow-up of past and ongoing activities.
- On slide 19, the project lead refers to alternate means of hydrogen detection as "hypothetical," then goes on to describe significant challenges and barriers in the development of a suitable hydrogen-specific sensor. The tone of this slide, as well as the slides describing responses to last year's reviewer questions, suggests bias toward hydrogen-specific sensors at the expense of suitable alternatives.

Project strengths:

- This project is largely spot-on with good output for a one-FTE effort. The direction is good, the output appropriate, and so forth.
- A strength is the international cooperation allowing exploitation of synergies between laboratories resulting in higher testing throughput.
- There appears to be good international collaboration on basic research.
- The project is working to address real challenges in both the near and far term.
- The scope is a strength.

Project weaknesses:

- This project could use more direct collaboration with automotive OEMs. The project could benefit from a more balanced approach in communicating strengths and weaknesses of hydrogen-sensing technologies.
- It would be good to see this project reach out to the Asian hydrogen community and establish an appropriate collaboration.
- It seems that the project is waiting for participants and project collaborators to come to them.
- It is unclear how "lessons learned" from collaboration with industrial partners (subject to confidentiality) are fed back into the future work program.

- The apparent focus is on vehicles, and the OEMs do not appear to be receptive to hydrogen sensors. The apparent limited focus on stationary applications is a weakness.

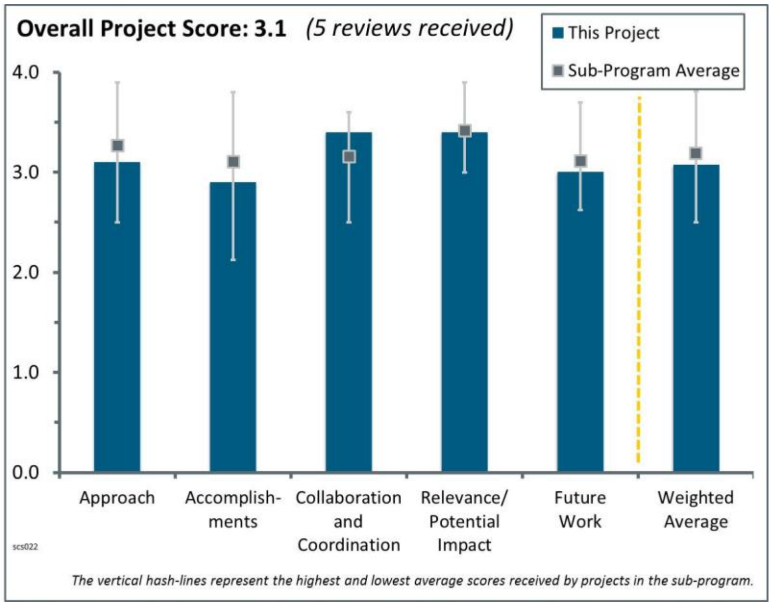
Recommendations for additions/deletions to project scope:

- It is suggested that less attention be spent on vehicle sensors and more on stationary applications. It is suggested that potential conflicts with NRTLs be addressed. It is suggested that an approach be generated to address the liability associated with testing designs generated by commercial interests.
- It would be good to see representation and participation in an independently facilitated industry workshop to discuss the needs of automakers, the code requirements, and what it will take for hydrogen-specific sensors to be listed to UL 2075. If this is not feasible, consideration should be given to revising the ICC's IBC to remove a requirement that is impossible to meet.
- The project should have active outreach to relevant stakeholders.
- Wide area monitoring is now included in the project (although no results have been presented so far). The new effort on hydrogen fuel quality detection should be aligned with related activities in other sub-programs of the DOE Hydrogen and Fuel Cells Program (e.g., Hydrogen Production and Delivery, and Safety, Codes and Standards).

Project # SCS-022: Fuel Cell & Hydrogen Energy Association Codes and Standards Support

Karen Hall; Fuel Cell & Hydrogen Energy Association

Brief Summary of Project:



This project supports and facilitates development and promulgation of essential codes and standards by 2015 to enable widespread deployment and market entry of hydrogen and fuel cell technologies and completion of all essential domestic and international regulations, codes, and standards by 2020. The project ensures best safety practices underlie research, technology development, and market deployment activities supported through U.S. Department of Energy (DOE)-funded projects.

Question 1: Approach to performing the work

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

- The general approach has good disciplines and good objectives, and the activities follow a logical flow. The coordination of hydrogen safety information through reports, websites, and meetings is very helpful. Besides the tracking matrix, it would be helpful to identify the methods and approaches that are specifically conducted by the project to accelerate the efforts by code development organizations and standards development organizations (CDOs/SDOs). The project has a wide focus on multiple industries (transportation, stationary power, portable power, etc.), and monitoring these is helpful, but the project may want to identify priority sectors to manage the project's limited resources.
- The approach is similar to the approach used by the U.S. Fuel Cell Council (USFCC) in that there are working groups of industry members. However, there is no longer a high visibility at the organizations generating the standards. For example, the USFCC used to have a representative at the SAE Fuel Cell Standards Committee to contribute and function as a liaison. The approach also consists of a coordination call between the trade organizations and the SDOs. The attendance and input from the SDOs is limited. A more proactive outreach to the SDOs may be warranted.
- Working as the liaison to set up interactions between businesses and DOE and actively participating in several of the national codes and standards organizations demonstrates that the return on investment (funding) is being maximized with this group.
- The Fuel Cell & Hydrogen Energy Association (FCHEA) leads working groups that address the major safety, codes, and standards (SCS) activities for all types of hydrogen/fuel cell areas of interest (portable and stationary power and transportation).

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- The information provided on their website (http://www.hydrogenandfuelcellsafety.info) has up-to-date information on the status of various codes and standards and links to a variety of hydrogen safety information. Quick observation: April 2014 is missing from the archive page. The website is not all-

encompassing of everything hydrogen but does a reasonable job capturing most of the SCS tasks of U.S. interest.
- The project is doing good work in coordinating and helping standards to be developed and information to be disseminated. Both the national and international interfacing are valuable.
- The project has made accomplishments in communicating and coordinating information regarding various hydrogen codes and standards. The specific contributions of the project should be highlighted in future reviews. It is unclear whether the project is simply monitoring or actually involved in accelerating the standards.
- The accomplishments listed are dated and reflect previous activities conducted as USFCC. A more proactive approach, as conducted by other trade organizations, may be warranted.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- This project has excellent collaboration with multiple SDOs/CDOs and other organizations.
- One of FCHEA’s main reasons for existing is collaboration. They are well-tied-in and do this well.
- This is a trade organization. A more visible presence with the SDOs and the assisting of the SDOs in getting experienced personnel active in the generation of the product safety standards would accelerate the process and improve the quality of these standards, which are the supporting documents to the building and fire codes.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The project performed by FCHEA has a high degree of relevance to the DOE Hydrogen and Fuel Cells Program’s (the Program’s) SCS objectives. FCHEA’s support/facilitation role in coordinating the groups that develop standards and communicating/sharing safety and standards information is very important. FCHEA's role of linking industry to the Program is an important one.
- The project provides an important role in coordinating and progressing the critical hydrogen codes and standards for commercialization. The focus of the project has a direct link to a majority of the DOE research, development, and demonstration objectives.
- The work is relevant, but the current activity does not appear to optimize the potential of the membership within a trade organization.

Question 5: Proposed future work

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

- The project’s proposed work appears realistic.
- The proposed future work is adequate as it stands, but it is not nearly as aggressive as the work conducted in the past and does not impart a sense of urgency with the CDO and SDO management and working groups.
- The future work was outlined but appeared to be an extension of stated accomplishments rather than a clear focus on the next critical items needed for commercialization.

Project strengths:

- The project team has many years of experience promoting and coordinating efforts that connect DOE to industry and facilitating SCS activities. The project has a unique role in representing industry.
- The project has demonstrated very good meeting disciplines and coordination. It is also serving an important role in communicating the progress of hydrogen codes and standards.
- A strength is the access to and support of the membership of a trade organization.

Project weaknesses:

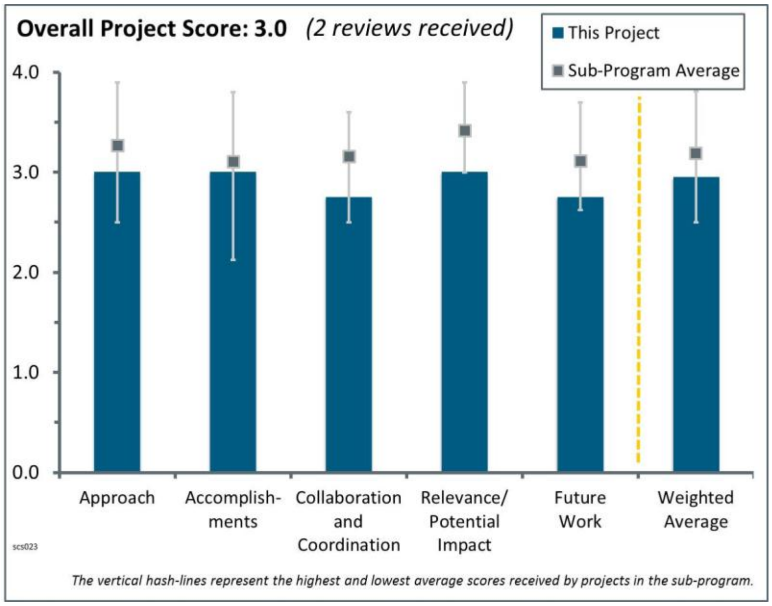
- A weakness is the lack of leveraging the membership to help populate the various SDO working groups to generate high-quality, timely product standards.
- This project needs more visibility and a cohesive strategy.
- The project could improve in identifying its specific contributions and influence to accelerating hydrogen standards.
- The project does not actually develop standards, uncover best practices, or perform research and development that validates standards.

Recommendations for additions/deletions to project scope:

- No changes are needed.
- The project should pursue opportunities to receive the voices of key stakeholders in various industries regarding their barriers to commercialization. The project seems very diverse across many hydrogen industries and appears to be missing key barriers for the transportation market (e.g., an infrastructure path to saleable hydrogen, such as flow controller, was not identified). Also, the project should include direct input and feedback regarding hydrogen field issues into the SDOs/CDOs.
- The project should become more engaged with the activities of all the SDOs and leverage the membership to generate high-quality, timely product standards.

Project # SCS-023: Hydrogen Leak Detector for Hydrogen Dispenser

Igor Pavlovsky; Applied Nanotech



Brief Summary of Project:

This project's goal is to make a low-cost, robust, durable hydrogen sensor. The reliability and maintenance burden of the leak detection systems at hydrogen dispensers will improve with sensor immunity to dust, poisons, and organic vapors. Sensors developed under this project will demonstrate stable, repeatable performance across wide temperature, pressure, humidity, and hydrogen concentration ranges.

Question 1: Approach to performing the work

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

- The approach was to take two microresonators (one open and one closed) and utilize the change in local gas density in the presence of hydrogen to affect the frequency of the microresonators over a wide temperature range. The idea is proven and validated. This is very nice work for a Small Business Innovation Research Phase I project.
- It would be good to see some interference gases added to the testing. Interference to common household solvents, natural gas, gasoline, etc. should be evaluated.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- The team demonstrated repeatability and high accuracy at up to 2% over a wide temperature range. The project has great use of thermal modeling to troubleshoot the power usage to reduce heat load. It would be interesting to see the results from the sensor that was shipped to the National Renewable Energy Laboratory (NREL).
- The reviewer would like to see results of the independent NREL testing. These testing data should be used to evaluate the suitability of this technology for a Phase II award.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- The independent testing at NREL is a good collaboration. At least one partner that uses hydrogen (fueling stations, original equipment manufacturers [OEMs], etc.) should be added if Phase II is funded.
- The team seemed to work well with NREL, but the collaboration with the Northeast Gas Association is less clear. The project might need to highlight that function better.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- The cost, durability, and calibration stability make this sensor very impactful to the commercial hydrogen rollout and would be very interesting to a station developer or even automotive OEM.
- Development of low-cost robust hydrogen leak detection would help in the faster adoption of hydrogen technologies.

Question 5: Proposed future work

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

- It seems that there will be testing at NREL, but it is unclear if that will happen if Phase II is not funded, although it would be useful. It is not very clear that this would need the whole of Phase II funding ($1 million), as this seems as though it just needs a little bridge funding for the sensor to be commercialized. Maybe that aspect could be expanded a bit.
- The proposed future work is dependent on Phase II funding. Interference testing should be added to this before field trials. The sensor-to-sensor reproducibility should be evaluated. Long-term durability testing should be added.

Project strengths:

- The technology is simple and seems to be low-cost.

Project weaknesses:

- There are no interference data. The entire response to 2% hydrogen is <2% of the baseline. It is not clear what the sensor drift will be. The PI claims that the team plans to calibrate once every two years. However, there are no long-term data to show that this is even feasible.

Recommendations for additions/deletions to project scope:

- Phase II should be funded only if the NREL testing shows promise for this technology.

2014 — Market Transformation
Summary of Annual Merit Review of the Market Transformation Sub-Program

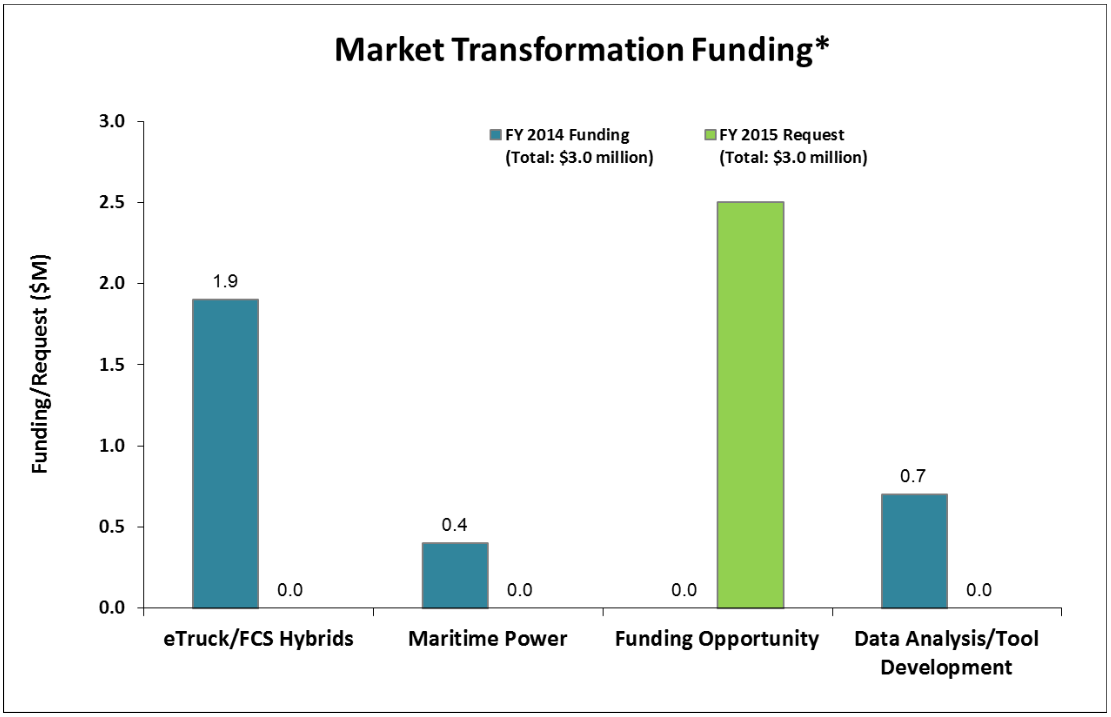
Summary of Reviewer Comments on the Market Transformation Sub-Program:

The purpose of the Market Transformation sub-program is to spur market introduction and growth for domestically produced hydrogen and fuel cell systems. By supporting initial commercialization in key early markets, this sub-program helps to identify and overcome nontechnical barriers to deployment and to reduce the life cycle costs of fuel cell power by helping to achieve economies of scale. The current focus of the Market Transformation sub-program is to build on past successes in lift truck and emergency backup power applications (part of the U.S. Department of Energy's [DOE's] American Recovery and Reinvestment Act of 2009 [Recovery Act] efforts) by exploring the market viability of other potential and emerging applications. Six projects were reviewed this year, and these projects are highly leveraged, with more than half of the funds provided by DOE's partners. This substantial commitment of external resources shows the high level of interest in exploring applications and markets where the hydrogen and fuel cell industry can expand and the technologies can play a valuable role.

Generally, reviewer comments about the sub-program were positive, noting that the focus on material handling equipment and emergency backup power has been extremely successful, as has the focused work in the state of Hawaii. The Market Transformation sub-program's coordination with agencies is commendable and allows the sub-program and the agencies to leverage funding to achieve mutual and individual goals, although it was suggested that increased collaboration with private companies could be beneficial. Some reviewers suggested that the sub-program could benefit from a general market transformation strategy that pinpoints longer-term niches. Reviewers also asked for insight into the process of deciding which markets are pursued and which are postponed.

Market Transformation Funding:

With the market successes that have been achieved by fuel cells in lift trucks and backup power applications as a result of prior fiscal years' and Recovery Act funding, the focus of FY 2014 funds was on a new application: battery/fuel cell medium-duty hybrid tucks that will demonstrate a value proposition for parcel delivery fleets, airport ground support, and specialty vehicles. As shown in the chart below, another application (i.e., shore power) will be a focus that will be leveraged through partnerships with other federal agencies and stakeholders. Although not reflected in the budget figure, DOE invested $42 million under the Recovery Act to enable the deployment of more than 1,000 fuel cells for early market applications, such as forklifts and backup power. The Market Transformation sub-program budget for FY 2014 was $3 million.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area.

Majority of Reviewer Comments and Recommendations:

The Market Transformation sub-program's projects were rated average to high, and overall ratings ranged from 3.0 to 3.6, with an average score of 3.3. The projects were judged to be relevant to DOE activities and employ good or adequate technical approaches. Reviewers recommended that future data collected and analyzed from all deployment activities be used to develop business case reports that can be used to support further market expansion.

Stationary Applications (Micro Combined Heat and Power): This project received an overall score of 3.3. Reviewers commented that this project was clearly relevant and could help build significant market share for hydrogen and fuel cells in the near term. They also observed that this project was well designed for collecting and analyzing data, and that the project had recovered well from failures of initial units. Some reviewers suggested that feedback should be solicited from host organizations about their experience with the system, cost/benefit, worthiness of using the system without DOE support, and what system changes are needed.

Airport Ground Support Vehicles: This project received an overall score of 3.1. Reviewers reported that the plan to complete this project is reasonable, with a number of go/no-go decisions that will help mediate the risk of this project. However, they mentioned that the summer 2014 schedule seems very aggressive and will need to be monitored. One important comment was that it is unclear how project partners have been integrated into the area of safety planning, and that the project also missed an opportunity to collaborate with the Hydrogen Safety Panel in early project design activities.

Landfill Gas-to-Hydrogen: This project received an overall score of 3.2 for its efforts to validate the business case and technical feasibility of using landfill gas (LFG) for hydrogen production and to share lessons learned that may be applicable for other candidate waste streams. Several reviewers commented that this project showcases an opportunity to produce hydrogen that is viable for use in fuel cells from LFG, which is often an unrealized asset. However, a reviewer commented that the project lacks cost information on the impact of new gas cleanup equipment and system design.

Hydrogen Energy Systems as a Grid Management Tool: This project received an overall score of 3.6 for its efforts in modeling, testing, and validating potential applications for hydrogen energy systems to address grid stability issues. Reviewers stated that the project is worth continuing. The reviewers made several suggestions: better align barriers addressed with the project's objectives and approach; seek more private industry participation; and document processes, challenges, and solutions so future projects can benefit.

Maritime Fuel Cell Generator Project: This project received an overall score of 3.6 for its efforts in developing, designing, and testing a first-of-its-kind hydrogen fuel cell power generator for maritime applications. Reviewers stated that the project has done an outstanding job of coordinating efforts between the fuel cell supplier, the fuel cell customer, the infrastructure support, and the relevant regulatory agencies. Also, reviewers stated that the project shows notable leveraging of other government agency funding and provides a meaningful deployment of hydrogen technologies. Reviewers commented that any schedule slip on the design review will surely result in a delay of the entire project.

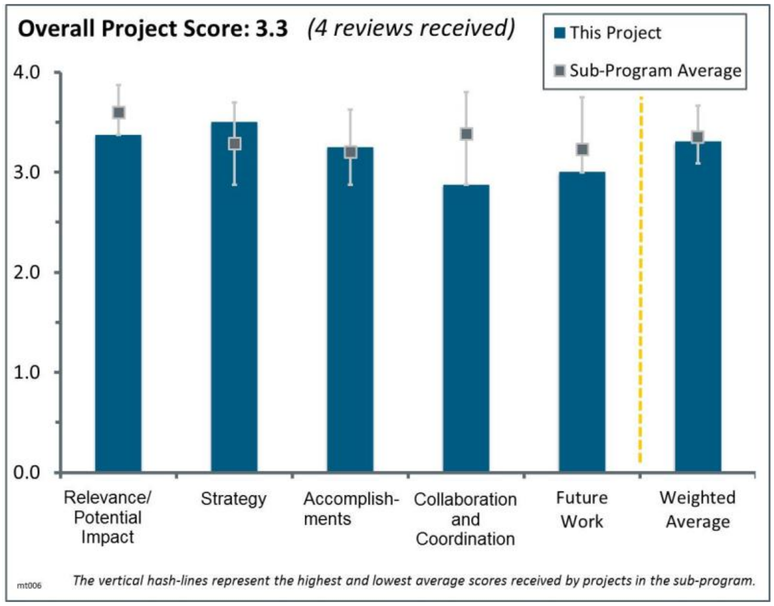
Fuel-Cell-Based Auxiliary Power Unit for Refrigerated Trucks: This project received an overall score of 3.0 for its efforts to design, develop, and demonstrate hydrogen fuel cell power for refrigerating trucks. Reviewers stated that the potential impact will be large, given the number of refrigerated trucks on the road and the number sold each year, if a business case can be realized. Reviewers stated that this project could meet a need of the trucking industry, save fuel, reduce greenhouse gases, and create a market for fuel cell technology. Also, it was stated that the funding and/or time does not seem sufficient for full integration (e.g., electrical integration with the transport refrigeration unit), and that the reason for 400-hour demonstrations was not defined.

Project # MT-006: Fuel Cell Combined Heat and Power Commercial Demonstration

Kriston Brooks; Pacific Northwest National Laboratory

Brief Summary of Project:

The overall objective of this project is to demonstrate combined heat and power (CHP) fuel cell systems, objectively assess their performance, and analyze their market viability in commercial buildings. Possible system improvements are identified through long-term data collection. The project provides independent assessment of operations, economics, and environmental impact and develops a business case for the continued use of CHP fuel cell systems.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The project is a very good demonstration of fuel cells installed in real-world applications. Great data are being collected that will help prove that fuel cells are ready for “prime time.” The results of the project can help improve acceptance of fuel cells based on unbiased performance data collected.
- The project clearly showed the advantage of a phosphoric acid fuel cell (PAFC) versus BASF’s polybenzimidazole (PBI) and the advantage of small CHP in various applications. The project showed which buildings were better than others for CHP applications.
- The project helped introduce CHP systems at consumer locations. One hopes the users will recognize the many benefits of these grid-independent systems. The data collected from these applications have provided valuable insight into their effectiveness, reliability, etc.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.5** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- This is a great showcase of the technology in an unbiased test, as well as a great mix of fuel cell applications/users to broaden the public’s knowledge base on the fuel cell technology. The published results will be very useful to the community.
- The project was designed for four different sectors and sized to supplement existing utilities. Continuing the study over five years has been good since it allowed the inclusion of the M5 units.
- The project has a well-thought-out technical plan.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The demonstration was excellent. The project has overcome some industry problems to nearly complete the demonstration and keep many of the fuel cells operational. The project team found a way to get the most data available out of the fuel cells. The large number of data collected over the ~five-year demonstration is fairly unique, as most demonstrations are not this long. It is beneficial to the industry that the government was able to fund this long-term demonstration.
- Analysis of the data is very good, showing the efficiencies, availability, and cost in different markets. The environmental costs should be articulated through publications in journals as well as in mass media. It seems that the PBI-based stacks deteriorated faster than expected, while the PAFC stacks proved more reliable. It is somewhat disappointing that the PBI stacks were failing—it is indicative that the technology needs maturation. It would be desirable to document the weaknesses of the PBI stack so that corrective research and development can be pursued. Color coding of “M5” and the original “CE5” seems confusing. Per slide 20, the M5 (PAFC) are producing less power and less heat and are less available than the CE5. These findings seem at odds with the results in slide 21. Slide 28 shows the life cycle cost of ownership. It would be interesting to see the cash flow curve.
- The project has made excellent progress and shows good transformation from PBI to PAFC. This is a good recovery of a program.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The project team found a good mix of users including collegiate, commercial, and recreational users. The publishing of the results will further the industry as a whole.
- Collaboration was mainly with ClearEdge and the host organizations. It is not clear what the host organizations think about their experience with the system, nor whether the cost versus benefit is worthwhile to repeat with their own dollars. If not, it is not clear what would need to change.
- The project needed better cooperation with ClearEdge to identify stack technical issues earlier.

Question 5: Proposed future work

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

- The project will be ending this year, but the team has an exit strategy to wrap up the data collection and publish the results.
- PAFC systems have been studied over many years and have a good reliability record. Other fuel cell types should be included— polymer electrolyte membrane fuel cell- or solid oxide fuel cell-based, even if the scales are different. Quantifying down-time contributors is worthwhile. Cash flow curves would be interesting, along with identifications for system improvements with the greatest impact.
- The project needs to highlight more “good” applications versus “bad” applications and publish these.

Project strengths:

- This is a solid demonstration of CHP fuel cells that is yielding important data on the performance and degradation of fuel cells over a ~five-year time period. This information could be used to raise awareness of the feasibility of fuel cell systems for commercial use.
- CHP systems have been deployed and are generating data for public dissemination. Good analysis is coming from the data.
- The project had a good recovery from failures of initial units.

Project weaknesses:

- Other fuel cell types are needed in the study. Feedback from host organizations would be good.
- The project should have identified issues earlier.

Recommendations for additions/deletions to project scope:

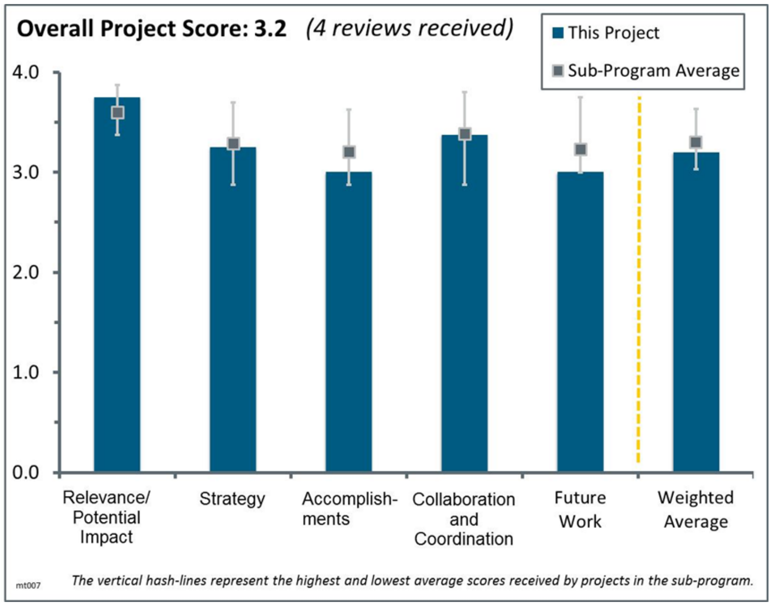
- This is an excellent project, and as long as the reports capture the data that was presented, the final reports will be very useful to the industry.
- Mass media collaboration would be beneficial. Other types of fuel cells should be included in the study.
- The project should evaluate cost versus benefit of avoided food spoilage costs due to power outages.

Project # MT-007: Landfill Gas to Hydrogen

Shannon Baxter-Clemmons; South Carolina Hydrogen and Fuel Cell Alliance

Brief Summary of Project:

The objective of this project is to validate the business case and technical feasibility of using landfill gas (LFG) as a "distributed generation" option for hydrogen production. The project will survey commercially available equipment to draw conclusions regarding economic viability of the LFG-to-hydrogen approach for potential end users, demonstrate technical viability of current systems to produce sufficiently pure hydrogen for use in motive or other applications, and confirm that there is no adverse impact on fuel cell systems that operate on LFG-sourced hydrogen.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- LFG is a source of renewable energy and a terrific source for hydrogen and fuel cells, provided the gas can be cleaned up cost-effectively. This project serves to demonstrate the use of LFG for a business that has committed to the use of fuel cell-powered forklifts. Successful operation and the business case study are a benefit to the host company as well as to the U.S. Department of Energy, which is sponsoring this project as a business case study.
- The effort to solve the LFG-to-hydrogen approach is an important endeavor to harness current wasted assets. This solves an environmental issue and an energy issue. Doing so in an operational environment such as BMW is noteworthy.
- This project showcases an opportunity to produce hydrogen viable for use in fuel cells from LFG, which is many times an unrealized asset. If successful, this technology could create a hydrogen source from garbage, which could allow for hydrogen stations to be built at landfills.
- This process could provide renewable hydrogen from a variety of sources for a variety of transportation and other applications.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.3** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- SCRA has used commercial equipment for the clean-up and conversion of the LFG to hydrogen. The facilities have been set up and will be tested at an existing plant where fuel cells are already used with delivered hydrogen. The clean-up equipment is designed to deal with siloxane-free LFG. Problems with nitrogen removal have been addressed by bringing in a new vendor and equipment. Funding issues due to unanticipated clean-up needs have been resolved. Project stall because of inadequate nitrogen removal points to underestimated challenges in clean-up process design.

- The original strategy to complete the tasks was excellent, but the project has hit several barriers over the course of the project. The team has been able to overcome those barriers, and if the demonstration is successful, the entire project will be a success.
- The approach is sound and logical. What the reviewer would have liked to see coming from this project is a “return” to the feasibility study/business case analysis done in fiscal year 2012 and an update with actual information and costs for an “actual” business case versus the projected case.
- The only reason for a not-perfect score is lack of concrete information on the impact of new revised gas cleanup on cost-effectiveness.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.0** for its accomplishments and progress.

- The project has been turned around from what appeared to be a show stopper. A new vendor has set up equipment to deal with the nitrogen removal. Two tests have shown that the quality of the hydrogen meets the specifications for most contaminants, including sulfur. The levels of halogens, ammonia, and siloxanes in the gas should also have been analyzed to determine suitability for the fuel cell application. Data collection was limited, perhaps as a result of time constrained by problem resolution and fund exhaustion.
- The project has had great progress on all fronts. The reviewer was totally surprised, after many years in gas cleanup, with the excellent removal of gaseous impurities.
- The project has hit several barriers over the course of the project, but the team has been able to overcome those barriers. Unfortunately, creating solutions for those barriers has had an impact on the timeliness, and the demonstration period has been shortened. The timeline has slipped during the project, and the final demonstration will be close to the original end date of the project. A no-cost extension may be necessary to get useful data if any more milestones are missed. It seems that 2014 was a better year for the project, and a number of obstacles were overcome. The government point of contact needs to keep a close eye on the project during the month of August to ensure a successful demonstration.
- The main barrier of this project is to overcome the LFG cleanup. Progress has been made, but there still does not appear to be a universal or simple solution for cleanup prior to steam methane reforming.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- There is great collaboration with private and not-for-profit entities. During the past year, the team reached out to the Gas Technology Institute to obtain expertise to overcome barriers affecting the project. The team has also been able to get significant non-government funding to support this project, which is very beneficial.
- Multiple partners and collaborators have contributed to the project. Resolving the clean-up problem by the partners and collaborators is indicative of their commitment to successful demonstration of the project. Sign-up of an investor and potential adopter of the technology is a plus.
- The involvement of industry and government is quite impressive—a really strong attribute for the project.
- The project needs to operate more closely with gas cleanup suppliers.

Question 5: Proposed future work

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

- The equipment will be tested over the next couple of months, with only two to three weeks of tests. This is very tight considering that unforeseen events have tripped up the operations before, but it is acceptable considering the constraints of the vendors and funds.
- Future work includes determining timing results in a limited trial of hydrogen production and use in forklifts. Outside the scope, business case work ideally would have been an output of this project.

- The project will be closing out, and the success of the project hangs on a successful demonstration/refueling of the hydrogen vehicles.
- The project must redo business with new gas cleanup system.

Project strengths:

- The team has overcome a number of the barriers that occurred during the research. The team has been able to assemble a number of collaboration partners that have the correct skills to complete the project.
- Strengths include support from two potential technology adopters. Cleanup solutions and reforming are achieved with commercial equipment. Sulfur and carbon monoxide have been managed to meet quality specifications.
- Strengths include project collaboration and use within a real-world manufacturing environment.
- The project is well-planned and well-thought-out.

Project weaknesses:

- The process design was faulty, which led to time and funding inadequacies. Siloxane removal should be part of the cleanup calculations considering the feedstock is LFG. There was a lack of cost or energy use data presented at the review.
- A number of technical barriers affected the timeline and shortened the demonstration period of the project.
- A weakness is the inability to deal effectively with the gas cleanup problem.

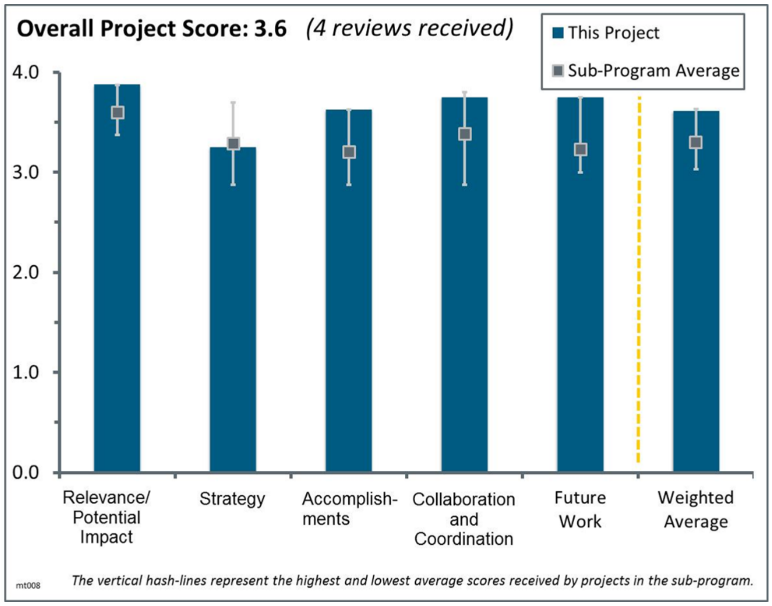
Recommendations for additions/deletions to project scope:

- It is strongly recommended that the project acquire overall plant performance data to permit detailed technical analysis (e.g., energy input/output, energy consumed at each component, utilities such as water, electricity, heat, etc.), as well as a business case study (capital and operating expenditures).
- The project should make sure the demonstration period for August 2014 is on track and good data are collected.
- The project should have been redoing the business case constantly.

Project # MT-008: Hydrogen Energy Systems as a Grid Management Tool

Mitch Ewan; Hawaii Natural Energy Institute

Brief Summary of Project:



Objectives of this project are to demonstrate the ability of electrolyzers to mitigate the impacts of intermittent renewable energy; to supply hydrogen to shuttle buses operated by County of Hawaii Mass Transit Agency and Hawaii Volcanoes National Park; to conduct performance and cost analyses to identify the benefits of an integrated system, including grid ancillary services and off-grid revenue streams; and to support the development of regulatory structure for permitting and installation of commercial hydrogen systems in Hawaii.

Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.9** for its relevance/potential impact.

- Proving the use of electrolyzers to stabilize the utility grid will have a significant impact on utility grid reliability and could help the implementation of more renewable power being injected into the energy mix. This aligns very well with the U.S. Department of Energy (DOE) mission.
- The project could have a tremendous impact on the availability to use renewables by mitigating the grid instability caused by those renewables.
- The principal investigator is spearheading a monumental effort in Hawaii—akin to work done by the California Fuel Cell Partnership. The Hawaii Natural Energy Institute's work is opening the market acceptance of hydrogen for an island that needs both energy security and environmental sustainability.
- This is relevant and useful for the program goals, but there is some discord between the identified barriers and the project objectives. The Barriers appear to revolve around a lack of knowledge, standards, and funding, while the objectives listed are more about the benefits of using fuel cells, electrolyzers, and hydrogen in various applications. Both are important and relate to DOE goals, but they do not seem to line up within the project. The project should review these and be more focused on one or the other—or both in a clearer manner.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.3** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The strategy presented to accomplish the tasks is excellent. The analysis justifying the electrolysis approach is useful by itself. The path to implementation in Hawaii will be the first of its kind, and the methodology and results should make future implementation easier.
- If barriers, objectives, and approach are more closely aligned, this project will receive a much higher score in this category. It would be good to know that the process of setting up the project/equipment is being well documented. It is not clear whether the objective of "supporting development of regulatory structure for permitting & installation" is being met by outlining the challenges, solutions, and details of the project

along the way. If this information is being recorded, then it cannot just meet this objective but also address the noted barriers. Similarly, it is unclear how the process of setting up the program and gaining additional funders and stakeholders has been recorded to meet and overcome that stated barrier. It appears progress is being made, but there is not clarity about documenting it for sharing and lessons learned.

- The success of the program is based on the ability to cycle electrolysis cells. If there are data to show this is not an issue, they should be shown; if not, data should be collected early, even if at the cell level.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- There was an announcement during the presentation that the Puna Geothermal Venture go/no-go is a go. The analysis justifying the electrolysis approach is useful by itself and should be shared throughout the industry. Documenting the path to get this far is a great case study example for others trying to implement hydrogen and electrolyzers in the utility grid system.
- This is excellent work in navigating local politics and procedures. Nothing talks louder than the demonstrated results.
- There has been great progress, particularly getting all the approvals.
- This strongly ties to several DOE goals, but it is not clear whether the project is really about demonstrating technology and applications or about improving knowledge and process for future projects. Most of the "guts" of the presentation was about the technology demonstration, getting it up and running, not on the process and knowledge improvements listed as barriers. It appears the project is doing both—and seemingly well, despite inevitable real-world challenges and obstacles. However, it would be good to see better clarification of what the main objectives really are and ensure documentation is taking place to help others in the future. This project could easily be accomplishing both of these (demonstration and process), but it is not clearly being laid out that way.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.8** for its collaboration and coordination.

- The project has a strong list of partners. It would be nice to tie in more private sector partners somehow.
- There is excellent collaboration. The project team has pulled together many strong players in Hawaii and has kept this project moving forward.
- It may be prudent to increase leverage of existing installations on the mainland. For example, learning and demonstration of electrolyzer systems could be expedited by leveraging National Renewable Energy Laboratory electrolyzer installations.
- Great cooperation has been achieved, particularly with Ormat Technologies and investors.

Question 5: Proposed future work

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

- The project seems to be on track for completion in 2015. This is a newer revised schedule and seems reasonable to accomplish the tasks that are remaining.
- The score is high as it relates to overcoming the listed objectives (technology demonstration), not the listed barriers, as it does appear to address barriers for setting up the equipment and related future work milestones.
- The project needs to get electrolysis cell data as soon as possible.

Project strengths:

- Collaboration between many entities keeps this project moving forward. The success of this project will be the first of its kind in Hawaii and will be a model for implementation at other sites. This success could lead to future implementation in other areas, with large amounts of renewables being connected to the grid.
- This appears to be a good project for demonstrating electrolyzers and renewable energy potential. It has gone through expected challenges and delays of real-world implementation but is showing progress and potential. The reviewer looks forward to reaching the full operation and analysis phase of the project.
- The project quickly identified the difficulty of getting all approvals.

Project weaknesses:

- It is not clear why the barriers are not more aligned with the objectives, as they do not appear to be directly linked nor addressed. The technology demonstration is impressive, but in further review and consideration, this misalignment is troubling. The solution may be as simple as reviewing the barriers to be addressed and editing them to reflect what the project really is seeking as its outcome.
- The project needs to have an alternative use for by-product hydrogen in case fuel cell electric buses (FCEBs) are not economic in the long term.

Recommendations for additions/deletions to project scope:

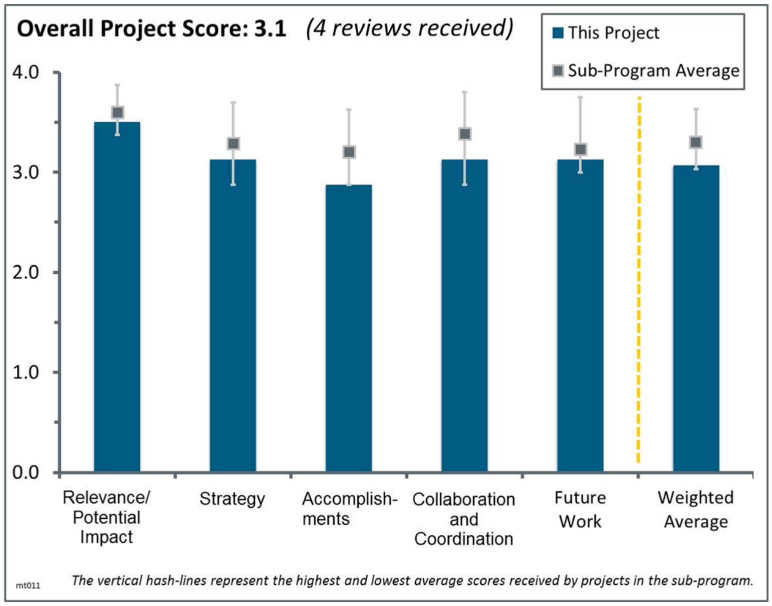
- The summary slide provides the best overview of what and why this project is important. It is worth continuing the project and seeing it through with the following notes:
  - Better aligning barriers addressed with objectives and approach of the project
  - Seeking more private industry participation
  - Documenting process, challenges, and solutions so future projects can benefit
- It would be beneficial to work with the utility companies to monetize grid benefits from electrolysis and install electrolyzers in distributed locations (e.g., Hilo, Kona). As renewables are distributed on the grid, benefits could be derived to the grid from distributed electrolysis. Such electrolyzers could operate during non-congested grid times and possibly receive lower electricity prices. It may be prudent to examine a utility-owned model for the electrolyzers.
- The project should continue on with the revised scope of work.
- Identify alternative use for hydrogen besides FCEBs.

Project # MT-011: Ground Support Equipment Demonstration

Jim Petrecky; Plug Power

Brief Summary of Project:

The overall objective of this project is to create a cost-competitive and energy-efficient fuel cell for airport baggage tow tractors to reduce consumption of fossil fuels, lower carbon emissions, and decrease energy expenditure. Specifically for 2013/2014, the project is working to develop the 80-V fuel cell product for baggage tow tractors to be tested in the Charlatte CT5E cargo tractor, perform a factory acceptance test to demonstrate equivalent tractor operation for battery versus internal combustion, and install and implement hydrogen at Memphis–Shelby County Airport in Tennessee.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The project appears to be aimed at a good market and is worthy of the resources to explore this area further. The identified barriers should be correlated to specific elements of the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan.
- The project is looking for opportunities to displace incumbent technologies. This is a necessary endeavor. Most of the discussion is about displacing diesel, but a comparison against battery-powered units is also needed, assuming that is viable as well.
- This project can help develop a new product area for the fuel cell industry that can help grow the industry and manufacturing in the United States. This new project may also help reduce greenhouse gas emissions in areas of concern.
- This is a great project that continues to expand the scope and value of fuel cells for material handling equipment.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.1** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- This is a perfectly designed project, with first obtaining requirements, then teaming up, design, alpha testing, beta testing, and deployment.
- The plan to complete this project is reasonable with a number of go/no-go decisions that will help mediate the project risk, but the summer 2014 schedule seems very aggressive and will need to be monitored. The barriers seem to be mainly engineering and manufacturing hurdles that the project team should be able to overcome.
- The project has a sound and logical approach.
- It does not appear that the project has worked through what requirements apply to this new application but is instead taking a figure-it-out-as-we-go approach. Without an early understanding of the applicable requirements, the project risks approval delays and/or potential safety issues. Additionally, there could be

significant value for a first-of-its-kind project to identify the applicable requirements to help future ground support equipment projects avoid delays or having to develop the list from scratch.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- The progress seams steady but slow. The beta builds will begin the summer with commissioning in September. That seems to be an aggressive schedule but should be able to be accomplished. Careful tracking of the progress should be performed to make sure production is on track.
- There has been good progress with alpha and beta testing remaining.
- The project is 50% through its schedule but has completed only 20% of the work.
- It would appear that the project should have been in deployment or nearing it at this point. The project is still awaiting the final go decision, which frankly could be delayed even further.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The project team has an excellent group of collaborators. If the demonstration is successful, the correct players are involved with opportunities for wide-scale implementation. Fedex Express and Charlatte are leaders in their respective industries, and if the project is successful, these two companies should be able to pull the fuel cell technology into the market.
- There is a solid list of partners. The project team might consider involving the Federal Aviation Administration in some role as the project moves forward.
- There is great teaming. The project perhaps could add some airlines to the team.
- The project has a number of partners. However, it is unclear how Plug Power has integrated these partners into the project in the area of safety planning. The project also missed an opportunity to collaborate with the Hydrogen Safety Panel in early project design activities. This could have been beneficial for the project and DOE program as a whole.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- A good project is based on scheduled alpha and beta testing and deployment.
- The schedule should be closely monitored. The work seems reasonable, but there has been some slow progress to date, and there needs to be much progress between now and September to keep the project on track. This project could be very successful if the schedule is kept.
- On Slide 22, Future Work – Budget Period 1, Task 1 is identified as “Definition of Requirements.” The presentation did not identify any specific details on this activity. Priority should be given to formally identifying all of the applicable requirements for this type of activity.
- The project will be “stressed” to complete all objectives within the current timeline. Developing a beta unit has taken longer than anticipated, resulting in the demonstration phase being squeezed.

Project strengths:

- This appears to be a good fit for the technology.
- The project is looking for a captured fleet-type market. Dealing with weather factors is an important consideration for this technology in this environment.
- This project is developing a product that could fill a need and be implemented at a number of sites across the country. A successful project could lead to a new fuel cell market opportunity. The fuel cell could fill a need in the fuel cell industry.
- This is a well-designed project, particularly the pretesting before deployment.

Project weaknesses:

- The presenter stated that one of the goals of the project was ultimately to support similar activities in California. However, the California location was dropped as part of this project. The presenter suggested that this was due to siting and timing issues (requiring a two- to three-year permitting process). It is not clear how the issues that prevented this deployment in California will limit the application at other airports. It is important for the project to identify (and DOE to understand) the impediments to the adoption of fuel cells in this market.
- Compression of schedule due to the long design phase is a weakness.
- It may be challenging to meet the September goals based on the current schedule.
- The project needs to get air quality quickly, including dust and sulfur compounds at the airport. There is also a need for more market analysis and economics.

Recommendations for additions/deletions to project scope:

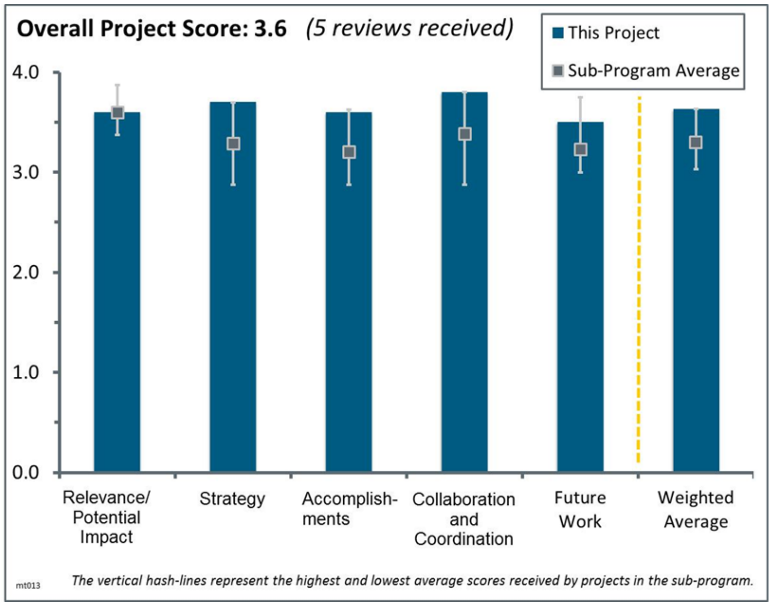
- The project should identify a requirements basis for implementing fuel cell technologies in ground support equipment and provide recommendations for how the California barriers could be addressed to open up that market.
- The project should add economics.

Project # MT-013: Maritime Fuel Cell Generator Project

Joe Pratt; Sandia National Laboratories

Brief Summary of Project:

The objectives of this project are to lower the technology risk of future port fuel cell deployments by providing performance data of hydrogen-powered polymer electrolyte membrane fuel cell technology in this environment, to lower the investment risk by providing a validated business case assessment for this and future potential projects, to enable easier permitting and acceptance of hydrogen-powered fuel cell technology in maritime applications by assisting the United States Coast Guard and the American Bureau of Shipping in developing hydrogen and fuel cell codes and standards, to act as a stepping stone for more widespread shipboard fuel cell auxiliary power unit deployments, and to reduce port emissions with this and future deployments.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- This is an outstanding project that can be leveraged to develop applications for many and varied but similar applications. Right now, today, diesel generators might be cheaper than maritime fuel cell generators, but this project might be that first pathfinder activity.
- This is a great project that leverages other government agency funding and provides a meaningful deployment of hydrogen technologies. The application has the potential to spill over into the general goods shipping industry, which is vast in globalized world commerce.
- Seeking mobile power solutions is an interesting niche.
- It is not certain that this application has a particularly wide market.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.7** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The approach is solid. Working with the state of Hawaii to advance the technology in many applications is very worthwhile. Building out from a single infrastructure makes economic sense for early adoption.
- The project is outstanding all the way around and a very well-planned effort.
- The project has a good approach to getting the requirements for hydrogen safety, but the team needs to do more quickly on the environmental requirements for the fuel cell, salt, water, drop, tilt, vibration, etc.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- A complete and professional project through and through; the team's written and oral presentations were top-notch.
- There has been good progress on hydrogen safety issues, but the project needs to move quickly on fuel cell environmental requirements.
- To date, the project appears on track. The critical element is the detailed design forthcoming this summer. Any delays in it or the project build will create future problems.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.8** for its collaboration and coordination.

- The project has done an outstanding job of coordination among the fuel cell supplier, the fuel cell customer, the infrastructure support, and the relevant regulatory agencies (not an easy job).
- The team is absolutely perfect and inclusive.
- The project has an impressive mix and apparently very strong collaboration to date. The project might consider talking to the U.S. Federal Emergency Management Agency (FEMA) about the utility of this power solution for their emergency and disaster response planning and support.
- There are numerous role players, each with key assignments delineated in the presentation.
- Hydrogenics is a fantastic company, but their focus is mostly electrolysis and not so much fuel cells. It is unclear that Hydrogenics is the right partner for this. Ballard already has a number of similar systems—hydrogen fuel cells in a container. They have been produced for Ballard itself as well as for DanTherm in Denmark. It may be prudent to include them in the program, as they have already gone down a significant cost reduction curve. If not, the project team should comment on the selection process.

Question 5: Proposed future work

This project was rated **3.5** for its proposed future work.

- The project has a good work plan.

Project strengths:

- The project is working on portable and distributed power generation, which, conceptually, has many potential applications.
- The whole effort is a project strength. This reviewer enjoyed the presentation to the point of taking minimal notes. No comments are necessary.
- The project is quickly addressing hydrogen safety issues and involving required stakeholders.

Project weaknesses:

- The timing is a weakness. The late award to Hydrogenics has put the schedule in jeopardy.
- This reviewer can identify no weaknesses whatsoever.
- The project needs to get fuel cell requirements.

Recommendations for additions/deletions to project scope:

- Liquid hydrogen should be considered as a fuel. The boil-off could be used directly in the fuel cell, and longer trips could be covered by this power generation type—potentially months in length, getting the interest of Pacific shipping. The technology could also be used by forward bases in U.S. Department of Defense applications—if liquid were used.

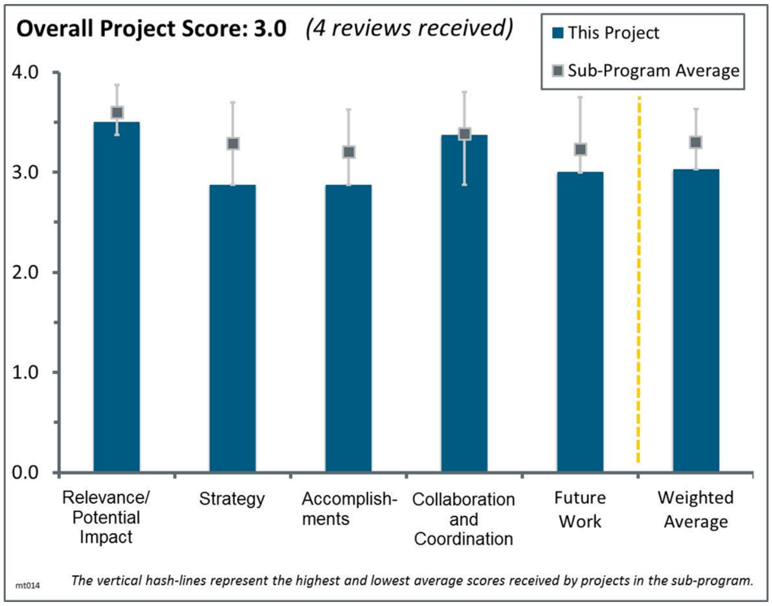
- Once the unit is built and operating, the project might reach out to FEMA for some late project collaboration.

Project # MT-014: Fuel-Cell-Based Auxiliary Power Unit for Refrigerated Trucks

Kriston Brooks; Pacific Northwest National Laboratory

Brief Summary of Project:

The overall objective of this project is to demonstrate the viability of fuel-cell-based transport refrigeration units (TRUs) for refrigerated Class 8 trucks. This project will demonstrate the value of a fuel-cell-based auxiliary power unit to replace diesel as the power source for the TRU to address environmental mandates, operate quietly, and be cost-competitive and energy-efficient.



Question 1: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- This project could meet a need of the trucking industry, save fuel, reduce greenhouse gases, and create a market for fuel cell technology.
- Looking for displacement of conventional fueled generators continues to be the best path for fuel cell introduction. This is already a winner on the emissions front, so getting the cost and value proposition remains a challenge for deeper adoption.
- The potential impact is huge *if* there is a business case, given the number of reefer trucks on the road and the number sold each year.
- This application seems to be a good extension of all the positive gains from the fuel cell material handling equipment market.

Question 2: Strategy for technology validation and/or deployment

This project was rated **2.9** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- There is a well-laid-out plan, and with the two subcontracts, the project will be able to compare and contrast the performance of two major U.S. manufacturers. The competition between the two companies may result in better products.
- The approach is sound, but as always, the cycle time to get these types of items ready for market introduction could and should be accelerated.
- Either funding and/or time does not seem sufficient for full integration (e.g., electrical integration with the TRU). The reason for 400-hour demonstrations was not defined in the slides. The timeline in the backup section does not seem to match with the approach timeline. And if the approach timeline is to scale, the section for defining the power rating of the system seemed too long.
- There is not enough being done to develop and demonstrate a convincing business case. The project seems more like a one-off (or two-off) demonstration that is unlikely to go anywhere unless the business case can be developed.

Question 3: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- In the first year of the project, subcontracts have been awarded, power requirements have been defined, and the business case has been developed. These actions will lead to a strong foundation for the out-years of the work. This seems like significant progress for the first year of the project and puts the project on a good track for the future work.
- With respect to accomplishments, the project is still in early stages. Getting the prototype testing completed is very good. The slides did not sufficiently describe why and how the systems were under development. Perhaps technology readiness levels could have been added to explain the development phase needed or the similarities to/differences from other products such as a fuel cell forklift power plant. The proprietary nature of the business case is understood, but perhaps there are assumptions that make a two-year payback period possible and what the size is of a “large” fleet that makes this feasible. It would have been good to see more definition on the data logging and power definition accomplishment (i.e., whether the sample profile was taken from the number of trips and averaged). The difference between alpha prototype testing and Level 1 prototype testing is not clear. It would have been helpful to see more which partner was leading the accomplishments.
- Within the first year, the project appears on course.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- Two of the major U.S. fuel cell providers are sub-contractors on this project. The sub-contractors each have partners as part of their teams, which consist of the progressive companies that will help create a market for a successful product.
- The list of contributors and industry leaders makes this a very attractive team. Having two producers also ups the nature of the work.
- Key players are participating, with strong industry/end user involvement.
- The project is coordinating well with two sets of customers.

Question 5: Proposed future work

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

- The upcoming work consists of developing the fuel cell auxiliary power unit and testing the units in the field with the project partners. The schedule seems reasonable, and the barriers seem to be overcome through engineering and manufacturing techniques.
- The actual demonstration and analysis was not included in the future work slide, but that seems like a critical aspect of this project. It would have been helpful to know what other value propositions were expected.

Project strengths:

- A strength of the project is system developers and customers. Another strength of this project is the logical (and important) extension from a forklift application to the refrigerated truck market.
- This project has good project partners working on achievable development and demonstration goals. The first year of progress has been successful, with no major barriers slowing down the research.
- The project is finding ways to displace incumbent technology.

Project weaknesses:

- There is still a distributed generations in the auxiliary power unit because it needs to be there for backup. This is not a strong indicator for the technology.
- A weakness of this project is that either the time or schedule is prohibitive of a fully developed solution. The demonstration and analysis aspect of this project was not discussed as much as expected.

Recommendations for additions/deletions to project scope:

- There are no recommendations for additions or deletions to project scope at this time.

2014 — Systems Analysis
Summary of Annual Merit Review of the Systems Analysis Sub-Program

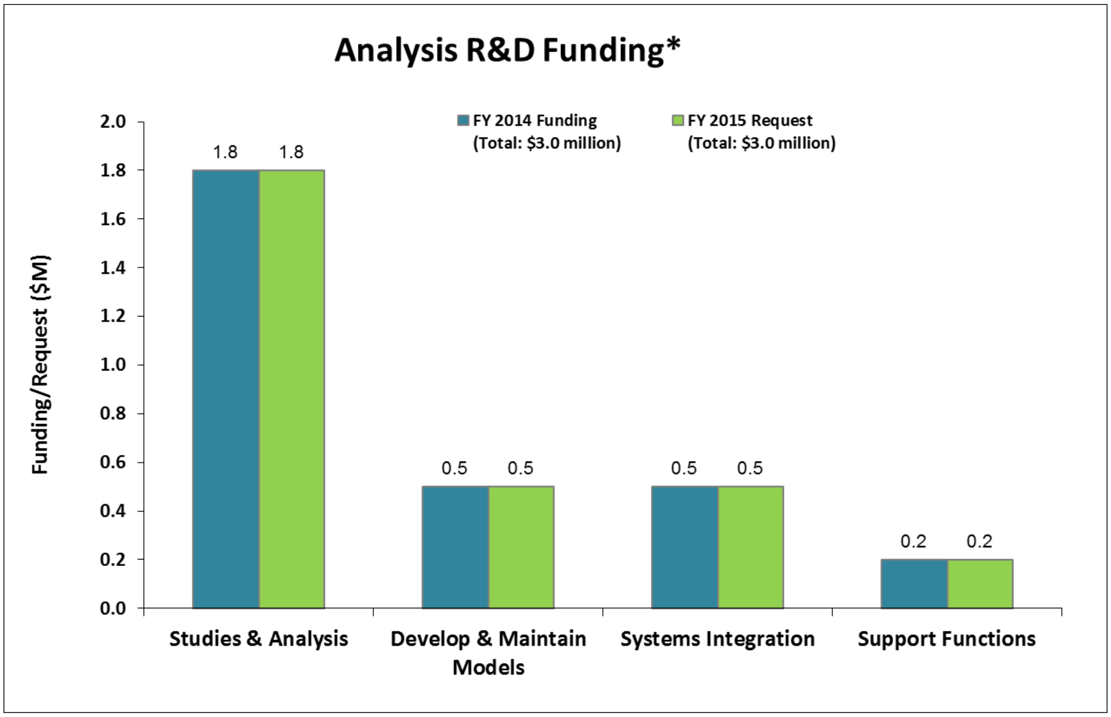
Summary of Reviewer Comments on the Systems Analysis Sub-Program:

The reviewers considered the Systems Analysis sub-program to be an essential component of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program's (the Program's) mission. The projects were considered to be appropriately diverse and focused on addressing technical barriers and meeting targets. In general, the reviewers noted that the Systems Analysis sub-program is well managed, demonstrates the ability to address immediate analytical needs and overall objectives and plans, and is specifically focused on supporting hydrogen infrastructure development.

Some reviewers commented that the sub-program is effective in providing analytical support and key insights for the Program's research and development (R&D) efforts, and that it is helpful in appropriately directing R&D efforts to address key barriers. Reviewers also commented that the analysis and model portfolio is balanced and making good progress toward understanding the issues, challenges, and opportunities related to achieving the sub-program's technical targets. Some reviewers commented that the models, tools, and financial analyses are helpful in understanding the current status of the technologies and near-term challenges; in particular, they pointed out that the investor workshop is creating awareness among the investment community. Reviewers noted that the Systems Analysis sub-program's collaboration with industry, national laboratories, and academia is strong and provides valuable information and feedback.

Systems Analysis Funding:

The fiscal year (FY) 2014 appropriation for the Systems Analysis sub-program was $3 million. Funding for the sub-program continues to focus on conducting analysis using the models developed by the sub-program. In particular, analysis projects are concentrated on infrastructure development for early market fuel cell introduction, the use of hydrogen and fuel cells for energy storage, life cycle analysis of water use for hydrogen production, employment impacts of developing infrastructure to supply hydrogen for fuel cells, and the petroleum and greenhouse gas (GHG) emission reduction benefits of seven future pathways. The FY 2015 request level of $3 million, subject to congressional appropriation, provides greater emphasis on analysis of hydrogen for energy storage and transmission, early market adoption of fuel cells, continued life cycle analysis of water use for advanced hydrogen production technology pathways, levelized cost of hydrogen from emerging hydrogen production pathways, impacts of consumer behavior, cost of onboard hydrogen storage options and associated GHG emissions and petroleum use, and hydrogen fueling station business assessment.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area.

Majority of Reviewer Comments and Recommendations:

The maximum, minimum, and average scores for the Systems Analysis projects were 3.6, 3.0, and 3.3, respectively.

Infrastructure: The three analysis projects reviewed in this topic area received a favorable average score of 3.3 for assessing the costs of hydrogen infrastructure development and understanding the hydrogen infrastructure costs compared to other alternative vehicle infrastructure. Reviewers acknowledged that the projects enable a better understanding of station configuration, hydrogen station components, the trade-off between consumer refueling time and vehicle range, and the cost of dispensed hydrogen at various dispensing pressures. The suggested next steps include more in-depth collaboration and consultation with hydrogen component supply companies to calibrate the projected component costs, and reviewers recommended that additional consideration should be focused on the cost of precooling and the cost of compression during station operation.

Model Development and Systems Integration: Two projects involving model development were reviewed (one for assessing the employment and economic impacts of deploying fuel cells and hydrogen infrastructure, and one for life cycle analysis of water use for hydrogen production), receiving an average score of 3.4. These projects received favorable reviews and were regarded as well aligned with the current sub-program goals and objectives.

Reviewers commented that the JOBS and economic impacts of Fuel Cells (JOBS FC) model provides valuable economic and job creation information for project funding justification, as well as informs policy makers regarding regional and national benefits of the technology. Reviewers recommended that the project be expanded to include a comparative analysis with other conventional and alternative fuels and fueling infrastructure, and to include information on the “net impact” of potential job displacement.

For the project *Life Cycle Analysis of Water Consumption for Hydrogen Production Pathways*, reviewers acknowledged that expanding the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET) model platform to include water use life cycle assessment addresses critical and relevant sub-program issues associated with hydrogen production, and that the comparative evaluation to conventional fuels is significant. Reviewers pointed out that the updated model would be important to policy and decision makers as competition for water resources and energy management increases. Reviewers commented that the future work for the project is robust, but that the project team should ensure the project uses standard methods and protocols with respect to existing water use work.

Programmatic Benefits Analysis: The analysis project reviewed in this topic area received an average score of 3.1 for assessing the costs and GHG emissions for multiple hydrogen production pathways. The reviewers commented that the analysis project to assess the Program's benefits (in terms of cost and reducing GHG emissions and petroleum use) for multiple *future* hydrogen pathways was relevant to the Analysis sub-program's objectives and illustrates the merits of hydrogen as an alternative transportation fuel for light-duty vehicles. Reviewers commented that the future work for the project should include updating the assumptions to a common analysis time period, adding transition analysis to the pathway group, clearly defining the difference between the current analysis and past/future studies on this subject, and examining sensitivity cases relative to decarbonization of the transportation fleet.

Studies and Analysis: Three analysis projects were reviewed and received an average score of 3.3. The projects covered a range of topics, including energy storage, fuel cell cost analysis, and the application of tri-generation fuel cells for infrastructure development in the Northeast. In general, the reviewers felt that the projects supported sub-program goals, but they also agreed that the results of the analysis projects need to (1) include cost and economic evaluations for the tri-generation systems, (2) expand the breadth of the technologies examined for energy storage, and (3) incorporate involvement from additional stakeholders.

For the *Impact of Fuel Cell System Peak Efficiency on Fuel Consumption and Cost* project, the reviewers commented that the project was well executed and useful for generating fuel cell efficiency projections compared to competing vehicle technologies, the impact of fuel cell improvements on cost, trade-offs between fuel cell cost and efficiency, and hydrogen storage costs. The reviewers suggested that the project should compare the fuel cell efficiency to other hybrid and alternative technologies and include additional collaboration with industry stakeholders to improve cost and performance information.

For the *Tri-Generation Fuel Cell Technologies for Location-Specific Applications* project, the reviewers commented that the project is well designed. They suggested that the project be expanded to include other sources of fuel, such as natural gas from a pipeline, and that the economics of the tri-generation system be included in the scope.

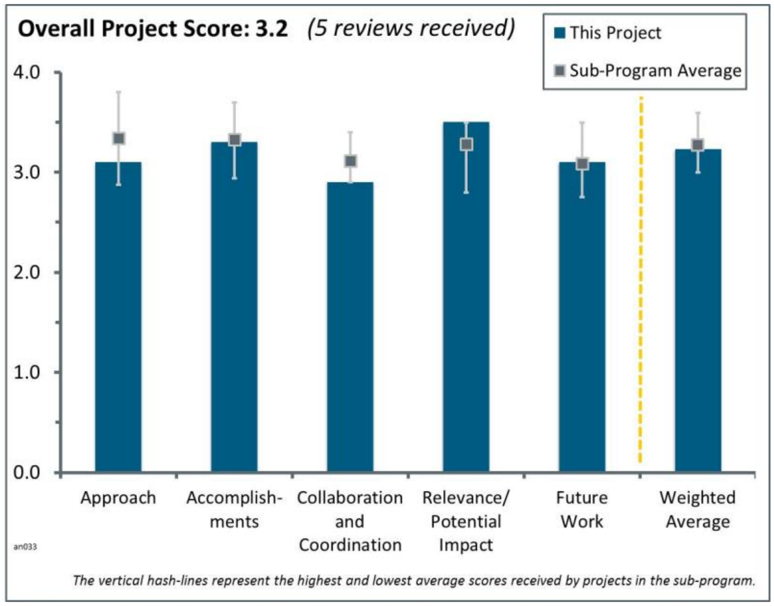
The reviewers of the *Electricity Market Valuation for Hydrogen Technologies* project commented that the study was exceptionally conducted and that introducing hydrogen production technologies to the electrical market is an interesting option to generate a revenue stream. They noted that the project should consider exploring the feasibility of the concept, how these technologies compare against the incumbent technologies that compete in this market, and the market size for this application.

Project # AN-033: Analysis of Optimal Onboard Storage Pressure for Hydrogen Fuel Cell Vehicles

Zhenhong Lin; Oak Ridge National Laboratory

Brief Summary of Project:

The overall objectives of this project are to: (1) develop a method to optimize onboard hydrogen pressure in fuel cell electric vehicles (FCEVs) by integrating a wide range of factors, (2) conduct case studies and provide useful insights for the industry and research and development planning, and (3) identify the optimal pressure that reduces system cost and increases market acceptance of hydrogen FCEVs.



Question 1: Approach to performing the work

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

- The integration of Oak Ridge National Laboratory's system analysis tools with other national laboratories' well-established models provides an excellent approach to the main objectives of this project.
- The approach is excellent; however, it focuses on optimization only. In reality, single objective optimization is not the right approach. The project needs to do multi-objective optimization, considering the importance of different vehicle parameters with individual weights—for example, range, cost, fuel efficiency, and infrastructure availability. Such relative optimization gains can be obtained via vehicle choice models. The ultimate goal is to get fast acceptance of vehicles by the market, and cost alone is not the driver. People buy cars for performance, cost, cargo capacity, etc. Changing the pressure of the tanks will affect all of those parameters. The direction of improvement may thus be actually in the opposite direction of the analysis' findings.
- The approach of considering the marginal cost of hydrogen dispensed at various pressures compared to the consumer value of refueling time is a reasonable method of considering optimal hydrogen storage pressure. The proposed value equation and parameters of interest can be used to understand consumer value of various hydrogen pressures. The presentation did not provide sufficient understanding of how key parameters (i.e., hydrogen station cost, consumer value of time, and refueling annoyance factor) were derived or whether these values were validated or peer reviewed. The presentation provided too little information on the details of the approach and where the values and parameters used in the analysis came from. This makes an assessment of the validity of the findings difficult. There are insufficient details provided about the hydrogen optimal pressure model and how it has been validated and/or peer reviewed. A baseline consumer value of time of $100/hour coupled with a refueling annoyance factor of 3.5x—yielding an overall consumer cost of refueling time of $350/hour—seems very excessive. It is unclear whether a value of $350/hour of consumer time has been validated against existing literature.
- Given all the parameters that could be optimized in the model, it is a good idea to build a user interface to allow the user to choose the appropriate inputs for different scenarios instead of trying to optimize everything at the same time. It is difficult to understand why onboard storage cost increases as a function of pressure. Vehicle manufacturers are set on a 10,000 psi storage tank. Therefore, the cost/size of the tank will be the same regardless of the pressure at which it is refilled. It is not clear why the marginal station cost line is always flat. At 700 bar, there should be a much higher cost given the additional compression and cooling required to meet the 3 minute fill-up time requirement. It is also unclear whether what is being proposed is a station that only refuels at one particular pressure or a station with the flexibility to dispense hydrogen at different pressures/costs. It would be good if the model could optimize pressure from the point

of view of the station owner and not only from the consumer's point of view. From the slides alone, it is unclear how the refueling inconvenience cost is calculated.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- The project provides a useful analysis of clustered deployment strategies versus region-wide infrastructure deployment. The project has developed appropriate analyses of optimal hydrogen dispensing pressure considering different values of consumer time, hydrogen station deployment strategies, and driving intensity. The findings of the project would be strengthened through better validation of the underlying assumptions, including the assumptions of consumer value of time, hydrogen cost, and hydrogen station cost. The analysis should consider lower overall values of consumer time. It appears that the lowest value of consumer time (including a baseline time value of $50/hour and annoyance factor of 3.5x) is $175/hour. Though it may be useful to consider an annoyance factor, lower overall consumer time values should be investigated.
- This project has made significant progress to include the development of a friendly user-interface, the analysis of optimum pressure under cases reflecting zero-emission vehicle (ZEV) scenarios, and the sensitivity analysis. It is very interesting to see that the new results suggest that 700 bar may not be the optimum pressure in ZEV scenarios under a cluster strategy.
- The principal investigators (PIs) have accomplished the majority of the work. They seem to be on schedule. It is not logical that the marginal station cost curve is flat—the (PIs) should review compression and cooling assumptions for high-pressure dispensing. The incremental cost curve should not be flat, but instead show a step increment when additional cooling and compression is needed to refuel at the appropriate pressure in under 3 minutes.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- More collaboration with car original equipment manufacturers (OEMs) or vehicle choice modelers would be necessary for adequate analysis.
- The project notes useful collaborations with other industry, academic, and national laboratory researchers. Greater collaboration with Argonne National Laboratory (ANL), including the incorporation of its latest findings on hydrogen station costs, particularly station costs for various dispensing pressures, would strengthen the analysis.
- There is good collaboration with other national laboratories and industry. Researchers will benefit if they consider additional collaboration work with automotive OEMs.
- The U.S. DRIVE Fuel Pathways Integration Technical Team (FPITT) has not had a chance to comment on the latest version of the model. It is suggested that a beta version of the model be shared with potential users before public release. Although there seems to be collaboration with Hydrogen Analysis model (H2A) developers and the National Renewable Energy Laboratory, there seems to be some disconnect. The vehicle miles traveled used in these analyses were "1.3 k mile/y" (presumably 13,000 mi), while H2A and the Macro-System Model use 15,000 miles/year. Also, it is unclear if this project would benefit from incorporating the work that ANL is doing on station cost at different fueling pressures to optimize precooling.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The project enables a better understanding of the trade-off between consumer refueling time and the cost of dispensed hydrogen at various dispensing pressures. The project provides a more thorough understanding of the cost of dispensing hydrogen at various pressures, which may aid DOE in understanding how to develop an adequate network of hydrogen refueling stations given a fixed amount of investment capital available, particularly in the initial rollout years for infrastructure deployment.
- This is a very important analysis, although ultimately DOE has limited leverage on its acceptance by OEMs. The OEMs have full expertise of what trade-offs to make for successful vehicle sales.
- Understanding the relationships between hydrogen pressure and range, costs, and consumer acceptance is key for the early development of the hydrogen market.
- It seems that the whole industry moved to 10,000 psi to achieve a range comparable to internal combustion engine vehicles without consideration of other factors such as cost to consumer or station cost. It is good to see an analysis of the trade-offs among different refueling pressures. Even if automakers continue to target 10,000 psi, that does not mean that the consumer cannot refuel at lower (cheaper) pressures.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The proposed future work will add great value to achieving the main objectives of this project.
- The presenter notes a wide range of future activities that would aid in DOE’s understanding of optimal hydrogen dispensing pressure. The proposed future work appears quite extensive, especially considering the time and budget remaining in the project. It would be useful to better understand the actual expected future work versus potential future work if additional funding is provided. Future work should consider better validation of underlying assumptions and modeling and peer review of the analysis, which would strengthen the findings. Future work should consider a wider range of consumer values of time, particularly lower values of time. Future analyses should consider refueling annoyance factor as a sensitivity analysis while maintaining an analysis that does not incorporate an annoyance factor.
- The proposed future work seems good. Seeking comments from the FPITT on the user interface is recommended. It would also be good to see an option in the model for the station owner to minimize cost. Also, the project could integrate ANL’s modeling efforts on optimal cooling for different refueling pressures.
- It seems like the major questions have been answered, and the project does not warrant another year of funding.

Project strengths:

- The project’s understanding of the subject is a strength, as are the modeling capabilities and user interface.

Project weaknesses:

- Perhaps this is not a weakness, but it seems that the cluster strategy is unrealistic; getting a relatively large number of people who live within a few miles from each other to all buy FCEVs at the same time seems unrealistic. The project could use more interactions with other hydrogen modelers to check assumptions.
- It is not clear if this will actually be implemented by stations. H2USA has a cost/utilization model that will likely be more widely adopted than this tool. Consideration should be given as to how this can complement H2USA activities. Otherwise, this is duplicative and competitive with H2USA.

Recommendations for additions/deletions to project scope:

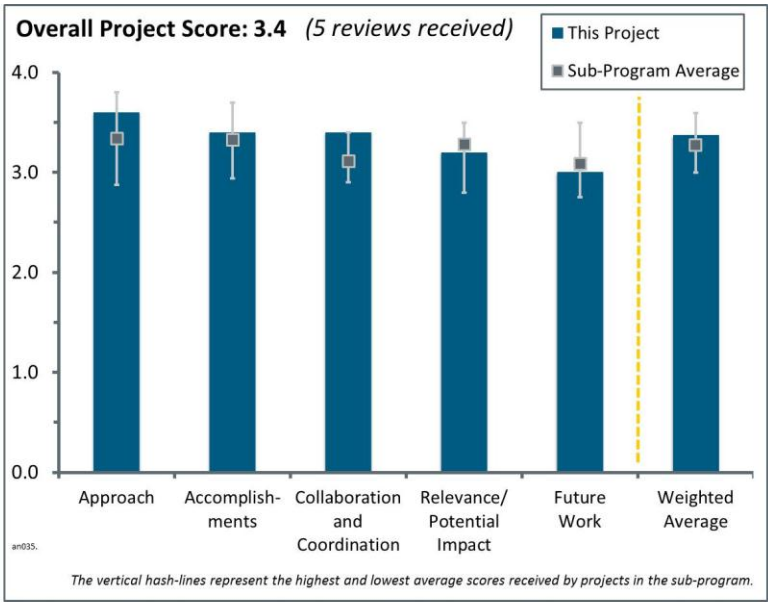
- This project appears complete.
- The researchers should incorporate contributions from modelers such as those working on the Automotive Deployment Options Projection Tool (ADOPT) and the Market Acceptance of Advanced Automotive Technologies model (MA3T). Also, they should incorporate reviewers from automotive OEMs.
- The project team should conduct analysis from the point of view of station owners and better define what elements are considered in the "station cost" analysis. It is unclear whether additional cooling and compression are considered for stations dispensing at higher pressures. Given that the marginal station cost is flat, it seems that the additional costs of precooling and compression have not been considered. The team should check its assumptions with ANL and describe how the refueling inconvenience cost is calculated.

Project # AN-035: Employment Impacts of Infrastructure Development for Hydrogen and Fuel Cell Technologies

Marianne Mintz; Argonne National Laboratory

Brief Summary of Project:

The objectives of this project are to analyze the economic impact of hydrogen and fuel cell deployment, to provide input for evaluating research and development and deployment targets, to develop a consistent framework for evaluating economic impacts of hydrogen infrastructure deployment, to compare alternative hydrogen station rollout scenarios, to develop robust and user-friendly tools with appropriate functionality, and to provide web-based training and support to enable economic impact analyses of hydrogen infrastructure deployment.



Question 1: Approach to performing the work

This project was rated **3.6** for its approach.

- Identification of the economic impact and job creation for alternative fueled vehicles and alternative fueling will be a critical policy consideration. The project work is user-friendly and will be accessible, standardized with a spreadsheet-based framework, and relevant with outputs providing economic impact and job creation information.
- This is fantastic work!
- The approach seemed straightforward, and the project seems in line with the objectives. However, it is unclear how partial jobs were dealt with. Some activities are responsible for only a part of a job. The speaker mentioned that jobs create indirect jobs and the effect ripples through the economy. It is not clear when to stop counting.
- This is a good model for analysis, but it is not clear who the target audience is.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The project is ready for launch.
- The project is making good progress on the stated goals of determining the number of jobs and in what sectors they occur. At least, these are the goals that the reviewer assumes the project is trying to achieve.
- There is very good progress with the development of the spreadsheet, default input fields, and model calculations. Additional work to conduct a peer review with additional industry developers to confirm input defaults and output calculations would be of value.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- Collaboration with institutions was excellent, but additional work will be needed to complete a peer review with industry developers to confirm input defaults and output calculations.
- There is good collaboration with other institutions.
- The project team has made a thorough effort to solicit feedback from stakeholders.
- More collaboration from policy analysis experts—for example, professional economist organizations—would have been expected. While inputs are great from stakeholders, economic analysis and jobs impact are a relatively new analysis pathway, and wheels might be reinvented unnecessarily.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- An understanding of the economic impact and job creation for alternative fueled vehicles and alternative fueling will be a critical policy consideration and is important for decision making. The approach is well supported and will be of high value. The project tool is user-friendly and will be accessible, standardized with a spreadsheet-based framework, and relevant with outputs providing economic impact and job creation information.
- This is a key area of research, which would inform policymakers regarding regional and national benefits of the technology.
- This could be used by certain policymakers to justify funding hydrogen infrastructure; however, the number of jobs estimated by this model is very low. There should be some analysis on how much money is spent in order to create one job.
- Perhaps job creation will happen in any alternative fuel sector. For example, electric vehicles will give a big boost to electricians, and they will spend their money at Starbucks. It is not clear whether the goal is to rank which alternative fuel will give the most benefit to the economy as a job creator. If electricity requires fewer jobs to be created, it is not clear whether this is better or worse. It is not clear whether this project is just quantifying effects or making value judgments. This has the potential to be very useful in quantifying numbers for reports and showing lawmakers the potential impact that hydrogen has on the economy.
- It is not apparent how effective this tool will be and who it is targeted to. The researchers need to convince fueling companies, station owners, etc. to construct new stations and install hydrogen dispensers at existing ones—it is not clear whether job creation is the driving force for this. The project needs to show payback, economic impact, and other data that would encourage station development.

Question 5: Proposed future work

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

- While the work is of high value, relevant, and well executed, additional work will be needed for review by developers. In addition, there should be consideration to expand the project to include a comparative analysis with other conventional and alternative fuels and refueling technology, and to include information on potential displacement of jobs to identify net changes.
- The project should take feedback from stakeholders to plan future work. There are still many variables and areas to add/investigate to make the tool more effective.
- The proposed future work does not seem to justify the proposed budget of $200,000. The planned activities seem to require less effort than those in the past year, which had a smaller budget.

Project strengths:

- The strengths include the following: relevance for policy and decision making, development of a tool that is user-friendly and will be accessible, and development of a tool that provides consistent and standardized results tailored to specific regions for accurate project assessments.
- The strength of this project is that it is straightforward, and the project is accomplishing its goals in a systematic fashion. This creates the political imperative to continue hydrogen work.
- Any model or analysis tool is needed, as the industry is finding footholds and proving itself to be a worthwhile technology to pursue.

Project weaknesses:

- Beyond creating good numbers for policymakers, it is not clear what the broader impact is. These jobs are created whether or not the project is completed.
- The weaknesses include the following: need for additional technical and economic peer review by hydrogen station developers, need for development of a comparative analysis with conventional and alternative fuels and refueling, and need to include displaced jobs for net results.
- The number of jobs created is low. Therefore, this could have a negative impact on hydrogen infrastructure given the amount of investment.

Recommendations for additions/deletions to project scope:

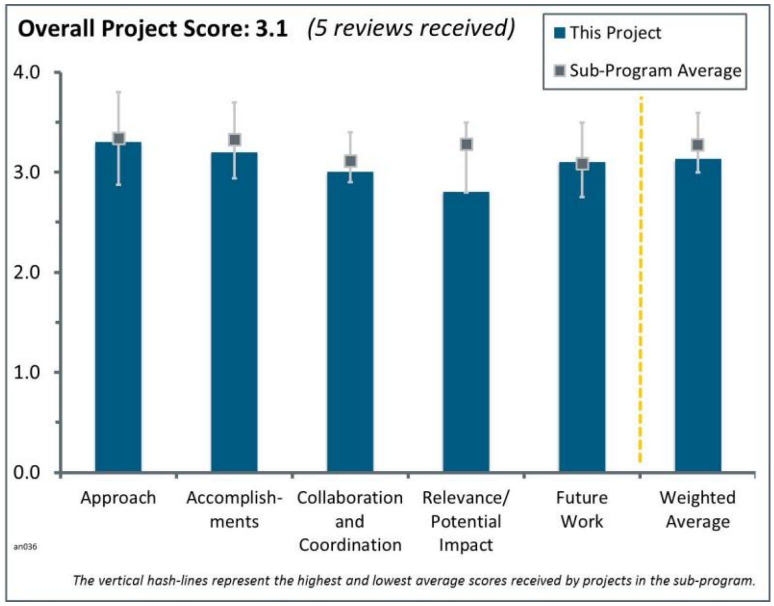
- The team needs to market this tool to the right audiences. The analysis should include adding a single pump at existing stations, as that is a direction some companies are taking in California.
- The project should reintroduce displaced jobs (even if in a cursory manner). Also, if at all possible, an indication should be added as to the skill levels required by the work for possible extrapolation to the associated salaries.
- The fiscal year 2014 budget should be lower. The project seems close to completion without much extra effort required, unless progress has been overstated in these slides.
- The project should do the following: provide resources for additional technical and economic peer review by hydrogen station developers; expand the scope for development of a comparative analysis with conventional and alternative fuels and refueling; consider expanding the scope to include displaced jobs for net results; consider where the spreadsheet tool will be located and promoted to provide effective public use by policymakers for objective decision making—placing the spreadsheet in a comprehensive alternative fuel transportation site/location would be advantageous over placement of the tool in a hydrogen silo site/location where it may be overlooked.

Project # AN-036: Pathway Analysis: Projected Cost, Life Cycle Energy Use, and Emissions of Future Hydrogen Technologies

Todd Ramsden; National Renewable Energy Laboratory

Brief Summary of Project:

The objective of this project is to conduct cost and life cycle energy and emissions analyses of complete hydrogen production, delivery, and dispensing pathways using the Macro-System Model (MSM) to evaluate hydrogen cost, energy requirements, and greenhouse gas (GHG) emissions. The project provides detailed reporting of assumptions and data used to analyze hydrogen technologies, enabling consistent and transparent understanding of results, and obtains industry review of the input parameters and the models used.



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Question 1: Approach to performing the work

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

- The project utilizes a highly structured approach to perform life cycle analyses of energy and emission costs associated with different hydrogen production and delivery scenarios. The approach appears to be similar to other projects (e.g., AN-044) supported by the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program and, as such, has a common basis for comparison with other analyses.
- The analysis is very rigorous, detailed, and well documented. It addresses the important objective of understanding the costs and impacts of advanced technology in a mature market. We need to know more, but we need to know this. Appropriate modeling tools and data were used. The key barriers have to do with the transition. It is understood that transitional analysis is not within the scope of this study, however.
- Analysis tools supporting the project are well developed and integrated in the overall analysis.
- The project uses the MSM to analyze hydrogen production pathways; it therefore uses a number of key models to provide valuable analysis of hydrogen production. It is not clear, though, how DOE is using this modeling data to identify critical areas for future research highlighted by the models as critical bottlenecks. The assumptions seem reasonable, but it is hoped that as fuel cells and stations roll out, these can be changed to match the situation on the ground.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.2** for its accomplishments and progress.

- Excellent progress has been made. The current technology analysis is complete and published. Most of the pathways of interest have been completed, and results were presented.
- The life cycle analysis for various hydrogen supply pathways is imperative to DOE’s overall efforts towards understanding the environmental and cost impacts of future energy supply options. Objective analysis is instrumental for DOE, industry, and other stakeholders in defending the merits of hydrogen energy in comparison to other energy vectors.
- The group has published a comprehensive report, and a large amount of good work has been performed. Where bottlenecks in costs are identified, DOE should consider directing research to address these.
- The accomplishments and progress are consistent with the level of support.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- Collaboration with other institutes is excellent—the project incorporates models and tools developed not only at the National Renewable Energy Laboratory but also Argonne National Laboratory and Sandia National Laboratories. The project works closely with Alliance Technical Services and the U.S. DRIVE Partnership to coordinate input used in their studies.
- In building on many years of previous work, broad collaboration is not necessarily required. The collaboration for this project is aligned to the project size and scope. The analysis is indirectly supported with the maintenance and continuing development of multiple models and tools from others areas of DOE, and this should be noted.
- For this study, it is not so much a question of collaborating with others but of making appropriate use of the models and data developed by others, and this was done.
- The project is reviewed by industry through the technical team, but the principal investigator should consider closer interactions with similar efforts worldwide to give the validation additional strength.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.8** for its relevance/potential impact.

- Project analysis clearly shows the merits of hydrogen as an alternative transportation fuel for light-duty vehicles and that hydrogen can be supplied through a number of hydrogen supply pathways. From an industry perspective, the analysis does highlight some concern on what are viewed as “stretch targets” contained in the analysis in the area of polymer electrolyte membrane water electrolysis efficiency.
- It is critical that DOE use up-to-date data to do a thorough analysis of all hydrogen production pathways, provide feedback to the technical task, and update these analyses with real-world learning as new technology comes online and the hydrogen economy rolls out.
- With fuel cell electric vehicles (FCEVs) still in their infancy, the hydrogen fuel costs are highly speculative. A comparison of the “Total Cost per Mile” pie chart (slide 12) with the chart shown in the 2013 review shows significant differences, and thus one wonders how sensitive the results are to input—especially with respect to how accurate the input is and how much it varies from year to year. As good as the approach is, it is of limited use if the input data are not known with sufficient accuracy. The GHG versus fuel cost information on slide 20 shows different data from those contained in the 2013 review—especially for the 2020 baseline gasoline vehicle and the 2020 electric gas hybrid. The reason for the differences is not clear.
- Premises are critically important. The standard premise for a study should be that the context is a concerted effort to reduce GHG emissions across the economy. It is a step in the right direction (but not enough) to run a green grid case (or cases). The basic premise should be that hydrogen is being introduced in an economy that has taken meaningful steps (in the long run since this is a mature market analysis) to reduce GHGs. This would affect not only the grid but all aspects of industry and transportation. Admittedly, this is difficult to do. It would require estimating how much biofuel is being used and what its well-to-wheel emissions are. It would also require assessing how energy efficient the vehicles, buildings, etc. are. Yet if a fundamental goal of hydrogen is to reduce GHG emissions, for a long-run lifecycle analysis, its impacts should be placed in the context of a world in which GHG mitigation has been achieved throughout the economy. This should be the default assumption, and sensitivity cases should be run relative to the default.
- The relevance of this project is not certain. This analysis appears to have been done many times before. It is unclear what the value added is.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The future analysis does not contain the evaluation of current hydrogen supply pathways that are expected to provide favorable life cycle emissions and low cost. This includes the evaluation of biomethane as a renewable energy feedstock for hydrogen using steam methane central and distributed reforming pathways. Furthermore, the central hydrogen pathways for steam methane reforming (SMR) should include the tri-generation of hydrogen, steam, and power.
- The future activities are a logical progression from the work performed thus far.
- Given the scope of the study, the proposed future work is very appropriate and will be useful. However, there are concerns related to the premises of the study.
- The project will also look at future hydrogen pathways and provide a very useful ability to compare to current hydrogen pathways.

Project strengths:

- Using MSM to provide critical data to DOE and stakeholders is a strength.
- The project foundation is based on well-developed and industry-accepted models and tools.
- The project's rigor, appropriate use of models and data, and careful documentation are strengths.
- The project has a consistent approach in analysis across the board—allowing for an accurate comparison of different pathways.

Project weaknesses:

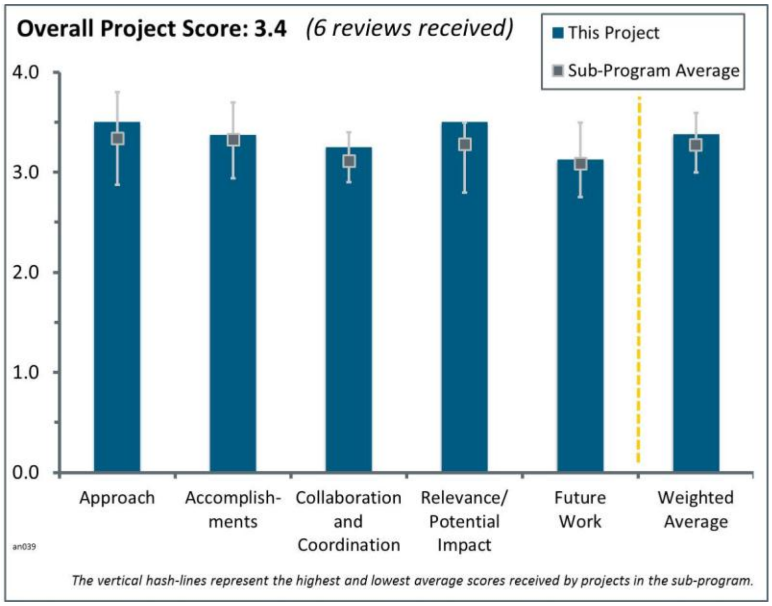
- The project may not be nimble enough to react to rapidly changing energy scenarios.
- Key input parameters and modeling assumptions underpinning the analysis should be updated for consistency and the latest information that reflects current market progress. Overall, there is a need to refresh the analysis assumptions to a common analysis time period (i.e., the project currently has a 2025 start-up year, but results are reported in 2007 dollars and based on 2009 energy costs) and assumptions (40-year analysis for central production versus 20-year analysis period for distributed production).
- The approach, while being consistent, is being used to analyze FCEVs that are insufficiently mature to yield accurate input data for comparison of the different pathways.
- The project needs to move into transition analysis and needs to reconsider premises concerning GHG mitigation throughout the economy.

Recommendations for additions/deletions to project scope:

- The project should continue to evaluate current and future hydrogen production pathways.
- The analysis should incorporate more immediate supply pathways, such as SMR of biomethane along with tri-generation of hydrogen, steam, and power for central steam methane reforming. Based on the set of assumptions, the project may want to make hydrogen stakeholders better aware of what the $2–$4/kg cost target really means in current and future dollars.
- The project requires better input information on costs and emissions for the different pathways being considered—perhaps the project should focus on developing high-fidelity data rather than expanding the project to consider different pathways.
- The project should add transition analysis, establish the decarbonization of the U.S. economy as the default assumption, and run sensitivity cases relative to that.
- Many aspects of this project appear to have been done in previous studies. These hydrogen pathways seem already to be well characterized by existing analyses and modeling tools developed by the national laboratories.

Project # AN-039: Life Cycle Analysis of Water Consumption for Hydrogen Production Pathways

Amgad Elgowainy; Argonne National Laboratory



The vertical hash-lines represent the highest and lowest average scores received by projects in the sub-program.

Brief Summary of Project:

The overall objective of this project is to develop water consumption as a new sustainability metric for evaluating the production of energy products. Life cycle analysis (LCA) is needed to estimate water consumption to provide a consistent accounting of water consumption of transportation fuels (including hydrogen). Argonne National Laboratory is expanding the Greenhouse Gas, Regulated Emissions, and Energy Use in Transportation (GREET) model to assess life cycle water consumption along the pathways of producing transportation fuels from various feedstock sources.

Question 1: Approach to performing the work

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

- The approach is good. The fact that water LCA will be integrated into GREET so that the model will become more comprehensive (and more complex too, unfortunately) is appreciated. This model could benefit from being more specific about the sources of fresh water—water consumption in areas where water is abundant is not as big a problem as consumption in places like California, where there is a persistent drought.
- This is a good strategy for water in the hydrogen cycle. It is a good strategy to compare with gasoline vehicles. It is a very ambitious approach. The project needs to show why and how water plays a role in hydrogen production.
- It is an excellent approach to use comparative analysis for identification of water use by fuel production and by per mile pathways. The approach is sound, but development of the data and review for various fuels and pathways will be challenging.
- The project addresses a critical need by adding water consumption to the GREET model for all fuels and is thus addressing many of the analysis barriers.
- The work has been carried out in an organized and effective manner.
- Generally, the approach to incorporating water analysis into GREET is good. It would be good to see more of the thoughts on how to combine water availability and water use to capture the regional nature of the water issue.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.3** for its accomplishments and progress.

- There is very good progress given the complexity of finding the appropriate data sources to integrate into GREET. The principal investigators (PIs) claim to have completed 70% of the original goal. The reviewer would agree and proposes that the project be extended to include GREET2.
- The work done so far is impressive. It is clear that progress has been made, and there are no faults here.

- Evaluation of the water footprint for the major fuels is a significant result. Quantification of water treatment impacts has a significant impact. The pathway comparison is a valuable development.
- Inclusion of steam methane reforming (SMR) and water electrolysis and conventional fuels is an accomplishment that will be very useful in the future as the United States moves to an independent, sustainable energy economy. The conclusion that most of the water in fuel production is used for irrigation for farming, used for cooling in thermoelectric power generation, and lost in evaporation in hydroelectric power generation is somewhat obvious. What is needed now is a metric by which we understand what water is really lost from the system. In all of the above cases, most of that water is returned to the supply; assuming water used for hydroelectric power is consumed in the western United States, it presumably falls as rain further east, and the irrigation water and cooling water are returned to rivers and streams, which allows the water to be re-extracted, treated, and re-used. Water for fracking is definitely lost, as it is injected into a deep aquifer now, but may be recycled in the near future. Water for hydrogen production is presumably lost at the point of manufacture but is returned as water at the point of consumption. This complicated picture needs a better representation in the model output.
- The comparison of LCA water impact is very nice. The tri-generation system does not need external water for hydrogen during steady state operation.
- Progress and accomplishments are in the initial stages for development, and additional work may be needed to collect additional data on fuels and pathways, to confirm data and assess accuracy, and to display results confidently as a comparative analysis that can be used to support policy for water conservation with energy management. Additional consideration should be made to determine if/how resources in both time and funding are adequate to complete the analysis.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- Researchers involved all the DOE technology offices that are needed to provide input to the model. Furthermore, they reached out to individual organizations and U.S. DRIVE Partnership technical teams for input.
- There is a good mix of collaboration with industrial collaborators and the U.S. Geological Survey. However, with such a complicated resource issue, more collaborators are definitely desirable.
- There has been some collaboration with industry and government. It would be better to see more involvement by industry for each of the pathways that have been chosen. Three real-world data points for two of the pathways (SMR, electrolysis) are a good start, but this needs to be expanded.
- There was reasonable collaboration with industrial users for large SMR and forecourt electrolysis. Water usage from forecourt SMR would be a useful addition.
- Coordination was good, but coordination with others from international markets and collection of data from industrial participants will be needed to resolve significant range discrepancies and confirm accuracy.
- Collaboration was not covered sufficiently to make comments.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- GHG and energy efficiency have lived in an LCA vacuum for a long time. Given all the water issues faced by industry, regulators, and communities, water consumption needs to be integrated as a functional unit when comparing energy sources and vehicle technologies. The 2014 National Climate Assessment states, “Changes in water availability, both episodic and long-lasting, will constrain different forms of energy production... Extreme weather events and water shortages are already interrupting energy supply, and impacts are expected to increase in the future... Producing energy from fossil fuels (coal, oil, and natural gas), nuclear power, biofuels, hydropower, and some solar power systems often requires adequate and sustainable supplies of water. Issues related to water, including availability and restrictions on the

temperature of cooling water returned to streams, already pose challenges to production from existing power plants and the ability to obtain permits to build new facilities."
- To make the correct decision for future energy use, we must have water consumption in the models, so the impact here is very high.
- This is a high-value topic that will have increasingly important policy and decision-making implications as competition for water resources and energy management increases.
- Developing an understanding of water is an important component of systems analysis. This project should be further supported to assist in an accurate assessment of the state of hydrogen and electrified vehicles regarding the environmental impacts.
- Incorporation of major pathways is important for the GREET model. Minor pathways are nice for completeness but are not as relevant as the major pathways.
- The relevance is not clear.

Question 5: Proposed future work

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

- The future work proposed will make important additions to the model. The comprehensive detail that the project will provide is appreciated; covering all the bases will make the output far less susceptible to criticism.
- The proposed future work is good. It is a staged approach, which makes sense because some pathways will be useable before the entire project is complete. It would be good to see more attention paid to documenting the sources of the information and assumptions. It would be best if these could be documented in a central location. The planned future work to reconcile the different water consumption concepts is very important, and it would be good to see a bit more definition around that.
- The inclusion of other pathways is important for completeness and to allow comparison of all options. Regional aspects should be addressed at "extremes" first to assess sensitivity to region. Impacts associated with purification are a good addition.
- It is encouraging to see that resource availability will be considered, although it is difficult to fathom how that will be measured within GREET. One of the bullet points under future work includes "develop water factors for vehicle materials." The reviewer is assuming that would feed into GREET2, which the reviewer would really like to see. It would be interesting to compare water consumption for different vehicle technologies. It is not clear how much water is needed to produce an internal combustion engine vehicle versus a battery electric vehicle. The PIs have a very clear understanding of where potential issues/areas of improvement lie and seem to be grappling with integrating a solution to these issues into the water model.
- Water from tri-generation plants should be considered as higher priority. It is a "no-water-needed" case for hydrogen or cooling.
- Comparative analysis for water use for fuel production and pathways will be very important and increasingly relevant, but attention is needed to collect and confirm data.

Project strengths:

- The analysis builds on a robust framework of life cycle analysis. It is structured in a logical manner, and the work is thorough.
- The PIs have the best understanding of how GREET works and are the best suited to incorporate new data into the model.
- The project represents a solid analysis. It builds on a strong background of the Hydrogen Analysis model (H2A), Hydrogen Delivery Scenario Analysis Model (HDSAM), and GREET model.
- The project adds water consumption to the GREET model, thereby strengthening the usage of the model.
- Fuel cells and hydrogen benefits are most important.
- Strengths include relevance, approach for development of a comparative analysis, and scope for production of fuels and use by pathways.

Project weaknesses:

- Definitions of water use are not always clear and meaningful (primarily for hydroelectric generation).
- Documentation of data sources for input assumptions and reviews of this project with stakeholders appear to be somewhat lacking.
- The project will always fall short in its representation of a complex problem, so the range of water consumption should always be included.
- The project is very ambitious, complex, and easy to misunderstand.
- Weaknesses include the tedious work needed to collect additional national and international data on water use, resolve wide ranges of data, confirm gross and net water use, and correct any miscounts or double counts of water use.
- Water issues are regional, and it will be difficult to portray these differences in the current version of GREET given that it does not have a mapping/geographic information systems component. GREET will become even more complex given the integration of a water component.

Recommendations for additions/deletions to project scope:

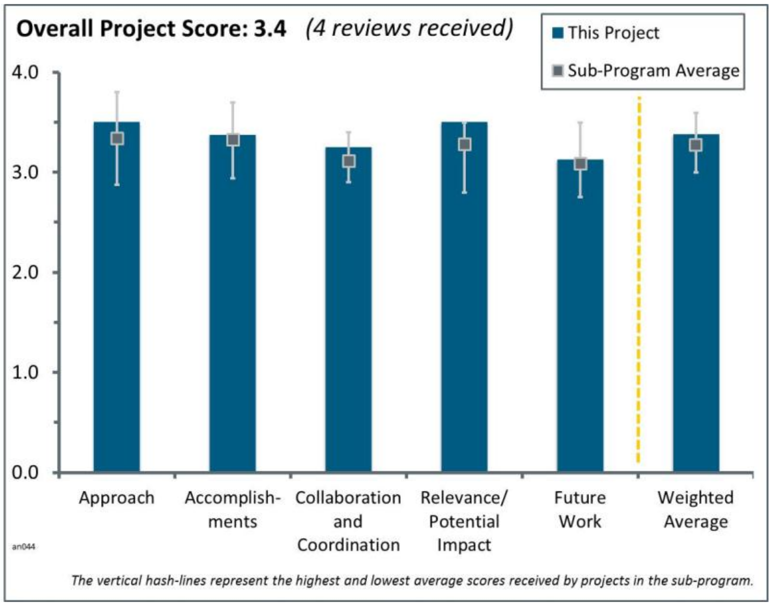
- It would be interesting to see the GHG, energy, criteria air pollutants, and water consumption of different fuels and vehicle technologies represented in one chart. Water consumption should be incorporated into GREET2, and ranges should be included in the results to show the variability of the data.
- This may already be part of the planned future work on reconciling water consumption concepts, but it would be beneficial to see a definition of the water impact that includes the regional availability, as well as the consumption. Alternatively, it would be valuable to incorporate into GREET the function of determining where water use occurs, leaving the impact analysis to others. Basically, defining the regional water use is very important because all water use is not created equal.
- The project should do the following: consider the cost of water treatment from produced water and the severity of pollution of various grades of produced water; include next-generation hydrogen production, such as algal and photoelectrochemical methods; in addition to consumption range, include statistics on how likely a particular level of water consumption is (e.g., evaporation in a dry year versus a wet year, maximum water for corn production, where and when, etc.)
- The project should ensure that work uses standard methods and protocols with respect to existing water-use work to facilitate comparisons with work done outside of DOE.
- The team should include comparison of hydrogen with other fuel options on the LCA basis.
- This is a relevant topic, but additional resources and time will be needed to collect additional national and international data on water use, resolve wide ranges of water-consumption data, confirm gross and net water use, and correct any miscounts or double counts of water use.

Project # AN-044: Impact of Fuel Cell System Peak Efficiency on Fuel Consumption and Cost

Aymeric Rousseau; Argonne National Laboratory

Brief Summary of Project:

The objectives of this project are to evaluate the benefits of aggressive fuel cell system peak efficiency compared to the current target of 60% from an energy consumption and cost point of view, and to evaluate the potential of technologies to accelerate petroleum displacement. Full vehicle simulations were performed to assess the vehicle energy consumption and cost of current and future fuel cell electric vehicles (FCEVs) compared to conventional powertrains as well as aggressive fuel cell system peak efficiencies.



Question 1: Approach to performing the work

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

- Use of the Autonomie model, which has been well vetted and is a well-regarded model for advanced vehicle simulations, is an excellent approach to understanding fuel economy improvements from advancements in fuel cell stack efficiency. The analysis of a range of vehicle classes and sizes and comparison to other vehicle types, such as conventional gasoline vehicles and hybrid electric vehicles (HEVs), helps provide a better understanding of how FCEVs perform in relation to competing vehicle technologies.
- There is very good connection with U.S. Department of Energy (DOE) goals. Greater than 60% peak efficiency is a good start to guide component development. Connecting with DOE advanced technology improvement work is a timely strategy. Technical Team feedback is a productive connection. The project makes good use of modeling tools and stakeholders, including the U.S. Environmental Protection Agency (EPA) drive cycle. Assumptions are realistic and hence beneficial.
- The approach is reasonable and uses appropriate data inputs (from Strategic Analysis, Inc. and EPA cycles).
- The project is well designed, but it is difficult to understand the objectives of the work given the description in the presentation. The principal investigator should revise all of the language on slide 3, define each scenario, and describe the DOE barriers that are being tackled. The barrier described in the project is "provide guidance on component targets and future [research and development] directions." That is not a barrier; that is an objective. This project basically compares current fuel cell efficiency (~60%) to a theoretical high efficiency that could be achieved in 2030. Both of these scenarios are then compared against current and future internal combustion engine (ICE) vehicles. It takes a while to understand that because in slide 3, the 60% fuel cell efficiency is referred to as a "goal," when in fact, that is the current efficiency of the fuel cell. It is difficult to understand what an "aggressive" fuel cell system performance means. Slide 26 states that the tank mass shrinks as the efficiency of the fuel cell increases. It is understandable why this analysis is being done, but if the assumption is that the tank size is optimized to reduce cost, this seems unrealistic given that automotive manufacturers are set on a 10,000 psi tank that can provide at least a 300-mile range, although more mileage, and therefore a larger tank, would translate into additional zero emission vehicle credits.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.4** for its accomplishments and progress.

- The analysis provides a very good understanding of the impact of fuel cell stack efficiency improvements on FCEV fuel economy across a range of vehicle sizes and classes. The findings of how fuel cell stack efficiency improvements lead to improvements in vehicle weight associated with the reduction in the need for hydrogen storage and other subsystems are significant and help show how fuel cell stack improvements lead to overall FCEV system improvements. The analysis provides an excellent look at FCEVs (across different classes) compared to conventional vehicles and HEVs.
- The team's use of probability for high targets is a good idea for productive simulation. For weight, the inclusion of multiple vehicle categories is useful for analysis. Multiple improvements with hybrids make a better impact. On-board hydrogen storage of 5.6 kg yields a range of more than 320 miles for all cars in this analysis, which is good news. The 60% to 65% efficiency improvement is very beneficial. Projected benefits justify continued DOE investment to 2030. Cost benefits results are confusing—the balance between capital expenditures and operational costs needs better understanding.
- It seems that the bulk of the work has already been completed. The project contributes to overcoming barriers outlined by DOE and meets the expectations outlined in the scope, but it does not add very much to the body of science. We already knew that higher-efficiency fuel cells would improve the economics of FCEVs and improve miles per gallon.
- It would be useful to have actual fuel efficiency projections. Relative numbers and gains are okay, but fuel efficiency projections can more directly feed into other modeling efforts.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The project benefited from consultation and collaboration with the U.S. DRIVE Partnership, including vehicle manufacturers. The project lists collaboration with industry and academia, but it is not clear how these actors were engaged or what information they provided. It seems likely that the project benefited from the data, modeling, and assumptions embedded into the Autonomie model, but as development of that model was outside this project, it is unclear how the model utilized industry and academic input on vehicle parameters.
- There are good collaborative efforts. Adding stakeholders such as battery electric vehicle (BEV) manufacturers, such as Tesla, is suggested.
- Collaboration with other government entities and laboratories seems appropriate. However, it is hard to tell whether collaboration with experts has been appropriate because there is no information about who was involved and how many people participated or whether the results of the Autonomie runs have been submitted to the "experts" to vet the validity of the results.
- The project could stand to benefit from direct collaboration with original equipment manufacturers (OEMs) for benchmarking analysis results. It is also unclear if the project findings and tradeoffs are in line with typical OEM behavior. For example, higher efficiency may yield larger cars while keeping tank size the same.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- This is one of the most important parameters of FCEV—as it has an impact on the size of production, distribution, and dispensing requirements per vehicle, as well as revenue per car and emissions footprints.

- The project provides guidance to DOE on FCEV research priorities by helping with understanding of the impact of fuel cell stack improvements on FCEV performance, including a comparison of FCEV performance to conventional and HEV.
- This is very important work to guide future improvements and funding. It will be nice to connect with the driving factors that improve system efficiency.
- This seems to be a good update to the Autonomie model, which is good to do on a regular basis to capture technology changes. The update is particularly important given that other models get input from Autonomie. However, the results contribute very little to the existing body of knowledge.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- Comparisons with competition and advanced hybrid concepts will be beneficial.
- The barrier described in the project is "provide guidance on component targets and future [research and development] directions." That is not a barrier; that is an objective. It is hard to tell whether the proposed future work is focused on critical barriers or will overcome them because no barriers are described. The barrier is the lack of understanding of how fuel cell efficiencies will improve in the future and what components will contribute to a reduction in cost and an improvement in fuel efficiency. With that in mind, the future work described in slide 16 seems appropriate. In particular, it would be nice to see future vehicle technologies compared against future HEVs and BEVs.
- Additional guidance on peak power and onboard hydrogen needs will be useful. Additional understanding of the comparison of FCEV costs to the costs of other vehicle platforms, including conventional vehicles and hybrids, will be useful. It is unclear what the need is for higher fidelity plant and cost modeling, partly because it is unclear what the limitations are on the cost assessment embedded into the findings in the project thus far.

Project strengths:

- Use of the Autonomie model, which has been well vetted and is a well-regarded model for advanced vehicle simulations, is an excellent approach to understanding fuel economy improvements from advancements in fuel cell stack efficiency. Analysis of a range of vehicle classes and sizes and comparison to other vehicle types, such as conventional gasoline vehicles and HEVs, helps provide a better understanding of how FCEVs perform in relation to competing vehicle technologies. The analysis provides a very good understanding of the impact of fuel cell stack efficiency improvements on FCEV fuel economy across a range of vehicle sizes and classes. The findings of how fuel cell stack efficiency improvements lead to improvements in vehicle weight associated with the reduction in the need for hydrogen storage and other subsystems are significant and help show how fuel cell stack improvements lead to overall FCEV system improvements.
- Analytical tools and stakeholder interactions are excellent.
- The project team has a lot of experience in vehicle modeling. Autonomie is a strong model.

Project weaknesses:

- The project may benefit from additional collaboration with industry stakeholders to improve cost and performance information, though it is unclear how much of this information from consultations with industry is embedded into the Autonomie model. It is unclear what the limitations are on the cost modeling in the findings to date (the presentation notes the need for more detailed cost modeling).
- The project should compare the project's subject technologies to tomorrow's advanced vehicles, such as Tesla and HEVs.
- The project could benefit from more frequent reviews from stakeholders to improve the presentation and clarity of the results, but the project itself is very well done. There is no information about the "experts" consulted.

Recommendations for additions/deletions to project scope:

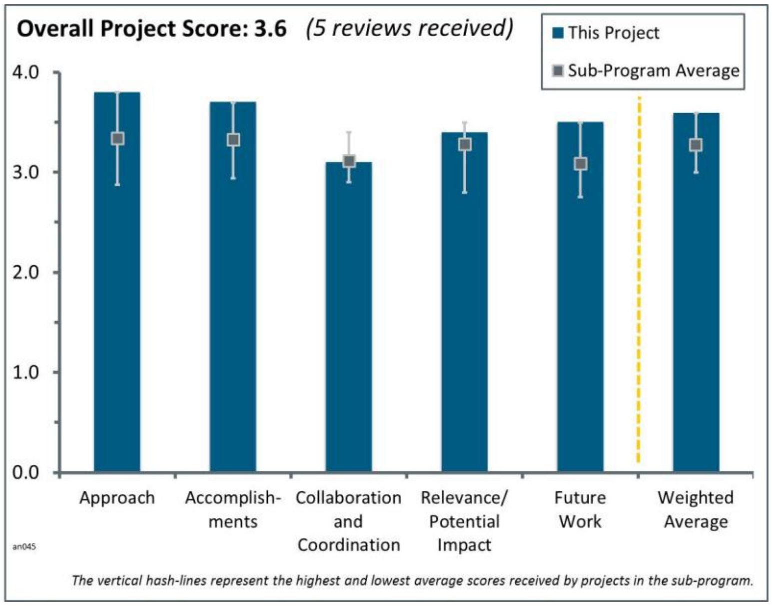
- It would be good to see more analysis like this—possibly expanded to provide projections for different vehicle classes, including heavy duty vehicles.
- This effort should be continued with hybrids. Feedback comments should be provided from U.S. and overseas fuel cell and hybrid developers.
- The description of the scenarios and cases could be improved by adding a slide with definitions. The project should explain some of the results seen in the tables. For instance, the numbers in slide 39 do not seem to make much sense—the dollar figure is higher in the "average" cost scenario than in the "high" cost scenario. An explanation of the results in the table would be useful. The same problem occurs in slide 41. The hydrogen tank cost "decreases" at higher efficiencies, not the other way around. The work is very strong, well executed, and technically solid, but it does not add much to the existing body of science. HEV and BEV should be added to the analysis.

Project # AN-045: Analysis of Incremental Fueling Pressure Cost

Amgad Elgowainy; Argonne National Laboratory

Brief Summary of Project:

The overall objective of this project is to provide a platform for comparing the impact of alternative refueling methods and fueling pressure on the cost of dispensed hydrogen. The impact of fueling pressure on the fill rate and refueling cost is evaluated. The modeling of hydrogen refueling stations (HRSs) incorporates the implications of the SAE International J2601 refueling protocol. Cost drivers of various fueling technologies and configurations are identified, and the potential of novel concepts to reduce refueling cost is evaluated.



Question 1: Approach to performing the work

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

- Development of the Hydrogen Station Cost Optimization and Performance Evaluation (H2SCOPE) model for optimization of compression, storage, and dispensing components provided an excellent approach to understand and consistently analyze hydrogen storage and dispensing configurations. The approach for conducting H2SCOPE model development based on physical laws and properties that were then validated against experimental data was excellent. The project provides a better understanding of hydrogen delivery and dispensing costs, particularly in reference to developing cost estimations for various hydrogen dispensing pressures.
- The development and the modeling structure in combination with key vehicle tank properties and existing refueling protocols provides an excellent approach to this project.
- The principal investigator has done excellent work as usual. This is how the reviewer would have approached the project.
- The principal investigator has taken a reasonable, well-planned approach to address numerous issues surrounding fueling pressure.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.7** for its accomplishments and progress.

- Development of the H2SCOPE model for hydrogen dispensing optimization and system configuration provides an excellent tool for understanding hydrogen station components and configurations. The H2SCOPE model was based on physical laws and properties and has been validated against published experimental data. The project provided excellent data on station costs and configurations to support various hydrogen fill times, including important information on hydrogen pre-cooling needs. Development of hydrogen compression, storage, refrigeration, and dispensing components is critical for improved hydrogen cost modeling. The project provides a thorough understanding of the cost of station components and the resulting hydrogen cost for hydrogen dispensed at various pressures to fuel cell electric vehicles.
- Significant accomplishments achieved by this project include the development of the H2SCOPE model and the evaluation of relaying time at different pre-cooling temperatures. These accomplishments will provide

very valuable information in understanding the impact of different hydrogen refueling pressures and the optimization of it.

- The assembly of a wide variety of data describing the various trade-offs around fueling pressure has been carried out thoroughly, and results were presented clearly. The H2SCOPE model is a major accomplishment that allows proper sizing and optimization at fueling pressures of interest. Results show potential for precooling at -30°C rather than -40°C. This could be a significant savings for forecourts. The graphs illustrating the wrap-up into final fueling costs show the pressure effects well.
- The material was processed and presented in a very professional and succinct manner.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- There is good work with manufacturers of compressors, refrigeration equipment, and others to acquire and analyze both performance and cost data.
- The collaboration with vehicle original equipment manufacturers (OEMs) and with hydrogen station equipment suppliers is key to the progress of this work.
- The project included very good collaboration with other national laboratory researchers, industry (through the U.S. DRIVE Partnership), and component supply companies. More in-depth collaboration and consultation with additional hydrogen component supply companies (and supply companies for other related industries, such as natural gas storage and dispensing equipment manufacturers) would aid in ensuring that the projected cost of hydrogen station components is accurate and reflects the variability of component costs across the industry.
- It would have been good to have more collaboration—it was not obvious that input was taken from compressor companies, for example. There are nonlinearities due to compressor availability—especially in the short term. For example, the type of compressor would be different past certain pressure thresholds (e.g., bootstrapping of last compression stage). This may push conclusions of the analysis to favor lower pressures.
- The project should spend some more time seeking input and distributing information to actual station designers, owners, and operators.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The project enables a better understanding of potential hydrogen station configurations and the cost of hydrogen station components, which will help improve dispensed hydrogen costing models. The project provides a more thorough understanding of the cost of dispensing hydrogen at various pressures, which may aid DOE in understanding how to develop an adequate network of hydrogen refueling stations given a fixed amount of investment capital available, particularly in the initial rollout years for infrastructure deployment.
- The relevance is high, but ultimately DOE might have little leverage over OEM decisions for optimal pressure. The project provides understanding for public consumption and some possible consensus views for OEMs to consider (in case they have not done their homework).
- The understanding of pre-cooling requirements at hydrogen stations and the impact on the cost of station refueling equipment are definitely addressed with this project.
- OEMs appear to have settled on 700-bar fueling, so these studies, while well done and scientifically relevant, may not have an impact on the marketplace.

Question 5: Proposed future work

This project was rated **3.5** for its proposed future work.

- Updating the Hydrogen Delivery Scenario Analysis Model (HDSAM) delivery cost model with the findings of this analysis will ensure a more accurate modeling of dispensed hydrogen cost. Integration of the H2SCOPE model with HDSAM to better optimize hydrogen station configurations will improve our understanding of hydrogen costs. Investigations of liquid hydrogen delivery components are a good extension of this work.
- Updating the analysis work with the latest version of SAE International J2601 and the MC refueling method will provide very valuable information to this project.
- Trade-off work with refrigeration and heat exchangers is a good addition. The HDSAM update is needed and should be pursued.
- It would be good to see much more collaborative activities with compressor companies, e.g., PDC Machines.

Project strengths:

- The project enables a better understanding of potential hydrogen station configurations and the cost of hydrogen station components, which will help improve dispensed hydrogen costing models. The project provides a more thorough understanding of the cost of dispensing hydrogen at various pressures, which may aid DOE in understanding how to develop an adequate network of hydrogen refueling stations given a fixed amount of investment capital available, particularly in the initial rollout years for infrastructure deployment. Development of the H2SCOPE model for hydrogen dispensing optimization and system configuration provides an excellent tool for understanding hydrogen station components and configurations.
- This project appears to have some potential to provide practical advice to station designers and is therefore a good project. Any added demonstration aspect would be a nice added benefit.
- The competent analysis and state-of-the-art filling model are strengths.

Project weaknesses:

- More in-depth collaboration and consultation with additional hydrogen component supply companies would be useful and would aid in ensuring that the projected costs of hydrogen station components are accurate and reflect the variability of component costs across the industry. Supply companies for other related industries, such as natural gas storage and dispensing equipment manufacturers, may yield useful cost information for components.

Recommendations for additions/deletions to project scope:

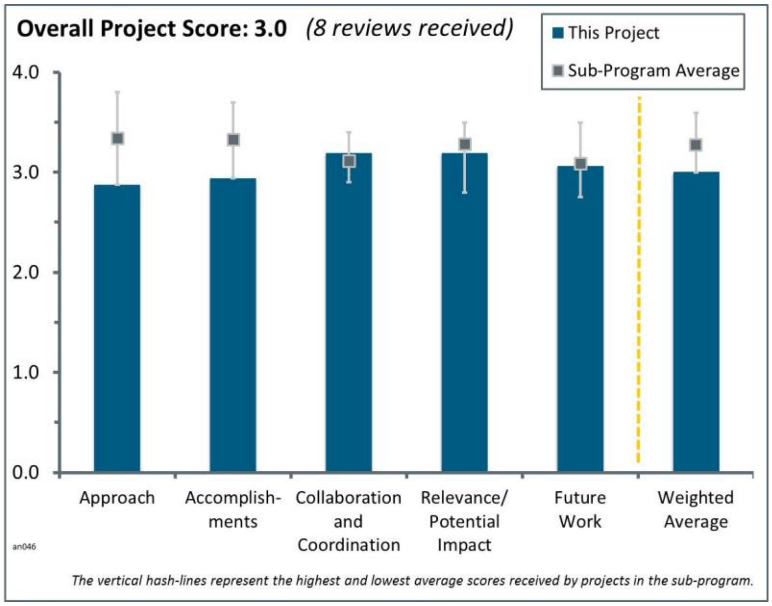
- The project should consider asking compressor makers about the impact of pressure on reliability.

Project # AN-046: Hydrogen Station Economics and Business (HySEB)—Preliminary Results

Zhenhong Lin; Oak Ridge National Laboratory

Brief Summary of Project:

The overall objective of this project is to develop a tool to analyze profitability, risk, and public–private partnerships in hydrogen station deployment. The Hydrogen Station Economics and Business (HySEB) model optimizes key deployment decisions to maximize profitability in consideration of investment risks, employing a clustering strategy.



Question 1: Approach to performing the work

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

- The project addresses three barriers: (1) future market behavior, in that it will suggest where to site new hydrogen fueling stations; (2) stove-piped/siloed analytical tools, in that it will combine a number of models and tools across a number of analytical platforms; and (3) insufficient models and tools, in that it will provide a new model based on existing tools and potentially a model validation tool as well.
- The use of cumulative cash flow is a good approach because it is one that investors can easily assess and use. The "Next-N-years" net present value (NPV) concept was confusing. NPV is usually a single value. The principal investigator needs to use metrics that are commonly used in the investment community and make the presentation more comprehensible. Rather than use the Next-N-years NPV metric, the investigator should clearly state, "Given a government subsidy of $X for Y years, the NPV of the project is $Z." Penetration rates based on projections are fine, but the market should also be modeled using both hybrid and plug-in hybrid electric vehicle penetration rates for comparison.
- The use of the station costs and clustering strategy from previous work at the University of California–Davis (UC-Davis) as the assumptions to their modeling work, in addition to the use of Hydrogen Analysis model (H2A), is a very good approach to address the main objectives of this project.
- So far we see the approach for trading off between emphasizing larger or smaller stations. There is concern that there is more to the story—for example, the model appears to have static stations. In reality, as demand grows, stations will expand to conform to demand. The question might be better stated to include an upgrade strategy rather than a strategy that places only new stations into a region. It is good to see leveraging of existing analysis—such as from UC-Davis.
- Analysis of a clustered station deployment is appropriate for the initial rollout of hydrogen refueling infrastructure. Analysis of different driving patterns and behaviors strengthens the analysis. It is unclear what assumptions and parameters are embedded into the HySEB model, and it is unclear whether the model has been thoroughly validated and peer-reviewed. It is unclear what assumptions were made regarding hydrogen station costs. The presentation lists costs as coming from a 2013 publication from Dr. Joan Ogden and from the H2A model. The H2A model is a costing tool that provides the cost of hydrogen on a dollars-per-kilogram basis given the input cost of hydrogen station capital—it is unclear where these station costs came from or if the costs are associated with particular published H2A case studies.
- The main weakness with the work is that the results are difficult to interpret or compare. It would be better if there was a simple definition of the objective function that the consumer/investor should seek to optimize. This should be the output of the model.

- The project goals are too broad and may be difficult to fully satisfy without broader engagement and participation of key industry stakeholders. Absent from the analysis is the amount of private and public support required to financially support the development of hydrogen fueling infrastructure proposed. It is good to see that the U.S. Department of Energy (DOE) is evaluating the merit of public–private partnerships as a means to develop early market hydrogen infrastructure. Considering public–private partnerships can be quite diverse, the "term-limited" public–private partnership evaluated in the analysis will face difficulties with private companies and individual investors because of the long buy-down period in the analysis.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **2.9** for its accomplishments and progress.

- Good preliminary results have been presented on this project, mainly in providing the analysis of how combining a cluster strategy with station-scale economy and travel patterns could significantly affect the system cost.
- The analysis provides information and findings on how station size and deployments can affect NPV, including analyses of different driving patterns. The analysis and findings are based on the HySEB model, which was developed for this project. The findings would benefit from peer review of the analysis and, in particular, a peer review of the HySEB model. It is unclear what assumptions are embedded into the HySEB model, and it is unclear either how the HySEB model incorporates the information from other DOE and academic modeling (Scenario Evaluation, Regionalization, and Analysis [SERA], Hydrogen Delivery Scenario Analysis Model [HDSAM], H2A, and the Spatially & Temporally Resolved Energy & Environment Tool [STREET]) or how HySEB compares to these models if it does not incorporate data from them. The findings of this analysis would be strengthened if the underlying model parameters and assumptions could be compared to existing modeling on hydrogen cost, station cost, and infrastructure siting.
- A new model (HySEB) has been developed to understand the economics of hydrogen fueling stations. Some preliminary results were presented that suggested that this analysis will be useful, but the modeling needs to react to the developing picture on the ground as stations are added, i.e., it cannot be using yesterday's situation to predict tomorrow's future.
- The analysis is very exploratory in nature, but the HySEB model appears to be already institutionalized. Considering that this is a new project, it is too early to place HySEB in the same category as other DOE and national laboratory models that have been fully vetted and developed over many years. One of the project goals is to address stove-piped/siloed analysis. Because public–private partnerships are not new and have been exercised across other industries and markets, perhaps a proven public–private partnership arrangement structure already exists that can be applied to hydrogen infrastructure.
- It was somewhat hard to tell what has been accomplished from the presentation. It appeared that a new model, HySEB, had been developed. However, it would be good to see a bit more time spent looking critically at the inputs to that model and the sensitivities of those inputs. The inputs were overly optimistic and based on a single analysis. Building the framework of a model is part of a modeling project, but the more important aspect is vetting the data inputs. That is arguably where the bulk of the work should be spent, and it is not obvious that much work has been done here.
- As it is a new project, we have not seen the approach considered for future work items. As the project end date is October 2014, it is not evident that there is sufficient time to perform those additional tasks.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The authors have collaborations with both industry (Ford) and academia (University of Tennessee). There also was evidence of heavy collaboration within the national laboratory circuit. This is good and definitely helps to overcome the barrier of siloed analytical capabilities. Because the analysis is heavily dependent on

an economic analysis and it was unclear whether this is being done, it would be good to see more economists involved from academia.
- There is very strong collaboration for this project with a great combination of academia, national laboratories, and automotive industry.
- The project notes significant collaboration across national laboratories, government agencies, academia, and industry.
- The project works with a good mix of university, national laboratory, and industry collaborators. As is always the case with these projects, the more input the better, but the project has made an excellent start.
- The project includes good collaboration with very well-respected project partners. To ensure private sector buy-in, project collaboration needs to expand to a broader representation of industry stakeholders.
- More collaboration would be useful with vehicle original equipment manufacturers (OEMs) and infrastructure providers. Stakeholder points of view will add relevance. It will also be important to see what financial performance metrics are of interest to stakeholders.
- Collaboration with H2USA is required, given it is developing a cost model as well. This might be much more highly utilized than this model. It would be good to have the nice work done here incorporated into the H2USA work.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- This is critical work—and it could not yield results too early. As investments are already in the works for infrastructure (e.g., California), feedback to inform station sizing trends will be critical. Any optimization that can improve station economics will reduce public subsidy needs and reduce investment payback time.
- The project is aligned with the DOE Fuel Cell Technologies Office's Market Transformation sub-program to help implement hydrogen fueling infrastructure in a real market environment. It is to be hoped that the project can address a solution to the inadequate level of federal incentives available to date for pre-commercial and commercial hydrogen infrastructure for light-duty vehicles.
- It is critical that DOE understands the real business situation on the ground and the number of subsidies and how long these must be applied to enable early adopters to be successful.
- This type of analysis work is key to the initial rollout of hydrogen station deployment and the economics around deployment.
- The link this analysis was making towards advancing the goals of DOE was not clear. It was potentially an interesting analysis, but more thought needs to be put into justifying how this will be useful to the overall goals. In other words, it needs to be made clear who will use this and how they will use it. Then, what overall benefit it will have needs to be indicated. Part of the issue stems from the fact that the metrics are not very clear, and there is no obvious way to compare the different options or to compare with a baseline case.
- The project helps with understanding the investment needs for initial hydrogen infrastructure rollout, including the level of public–private partnership required. The analysis helps provide an understanding of the needs and uncertainties associated with the initial deployment of hydrogen infrastructure. It is not clear whether the modeling provided by HySEB adds to DOE's understanding of hydrogen station cost and infrastructure rollout or whether HySEB is a new modeling of cost and infrastructure siting that does not reflect previous work conducted by DOE.
- There is uncertainty about the relevance if the project is not collaborating with the H2USA working group.

Question 5: Proposed future work

This project was rated **3.1** for its proposed future work.

- The proposed future work outlined by the researcher should provide additional value to the preliminary results obtained so far on this project.
- The topics are relevant next steps of this analysis.

- The project includes a good amount of future work, and the future project work needs to be prioritized. More analysis of public–private partnerships' cost-sharing mechanisms should be a top priority because it may eliminate the need for future work in other areas. One concern is that institutionalizing HySEB too early may risk the chance of abandoning the analysis in the future.
- It would be beneficial to have more alignment of the model with experiences of other fuel station systems, with incumbent/conventional technologies. It might be possible to do this through collaboration with companies that manage stations or invest in stations.
- Future work will expand on preliminary data; the project needs to be careful about sensitivity analysis and uncertainties in this new market.
- The proposed future work would be useful, but it appears ambitious considering the budget and remaining time left for the project. Future work should include further validation of the HySEB model, which the analysis and findings rest upon, and a peer review of the HySEB model would be particularly useful.

Project strengths:

- Analysis of a clustered station deployment is appropriate for the initial rollout of hydrogen refueling infrastructure. Analysis of different driving patterns and behaviors strengthens the analysis. The project includes significant collaboration across national laboratories, government agencies, academia, and industry.
- Collaboration seems to be the strength of the project. Many parties appear to be actively involved. It would be good to see this continued.
- The project begins to give an idea of how the economics of hydrogen fueling stations will play out.
- Credit should be given toward the evaluation of public–private partnerships as a potential means to develop early market hydrogen infrastructure.

Project weaknesses:

- The weakness is mostly that the product is difficult to use or interpret. If some time is spent clearly defining this, the project has potential.
- Dealing with the uncertainties while giving a realistic picture to guide policy and incentives needs to be handled realistically.
- The basic public policy partnership structure options should have been initially explored. Preliminary data analysis and presentation should be simplified for the introductory project presentation.
- The findings would benefit from peer review of the analysis and particularly from a peer review of the HySEB model. It is unclear what assumptions are embedded into the HySEB model, and it is unclear either how the HySEB model incorporates the information from other DOE and academic models (SERA, HDSAM, H2A, and STREET) or how HySEB compares to these models if it does not incorporate data from them. The findings of this analysis would be strengthened if the underlying model parameters and assumptions could be compared to existing modeling on hydrogen cost, station cost, and infrastructure siting.

Recommendations for additions/deletions to project scope:

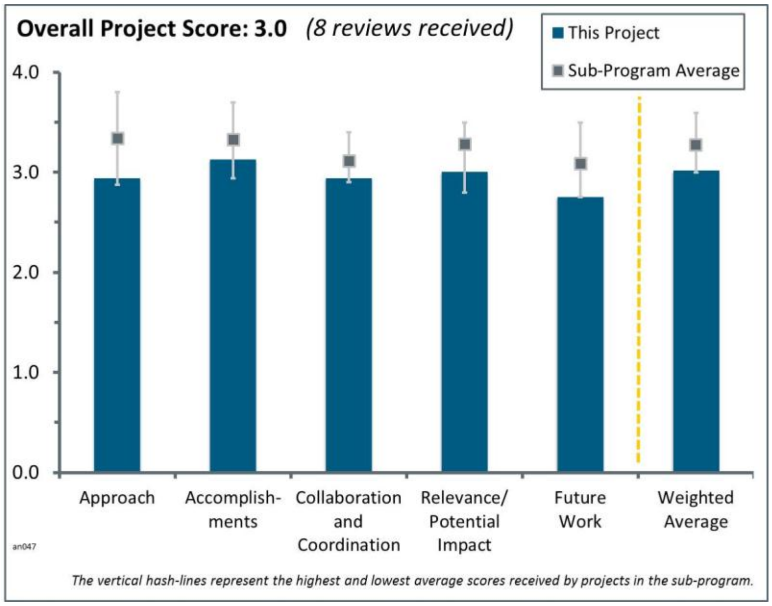
- Station upgrading strategies with demand growth should be considered.
- The project should obtain more feedback from new stations as they are deployed.
- The project introduces a new financial concept that requires more clarification and understanding. Graphs throughout the analysis are very "busy," and the differences between the analyses of the various buy-down periods are not clear. The "valley of death" between the small, medium, and large stations should be more transparent and built into the hydrogen station timeline. The analysis promotes a policy that incentivizes a guaranteed buy-down value over a period of time and will naturally incentivize larger and more expensive hydrogen stations. It remains questionable if that is really the right approach to building national hydrogen infrastructure for a fuel cell electric vehicle market that will take many years to realize an appreciable level of market penetration.

Project # AN-047: Tri-Generation Fuel Cell Technologies for Location-Specific Applications

Brendan Shaffer; University of California, Irvine

Brief Summary of Project:

The objective of this project is to assess the potential number and location of tri-generation (tri-gen) fuel cells producing electricity, heat, and hydrogen in an early fuel cell electric vehicle (FCEV) market scenario (circa 2015) in New York, New Jersey, Connecticut, and Massachusetts. The analysis considers the use of natural gas (NG) and anaerobic digester gas as feedstock. It also considers the viability of the tri-gen units serving as a local hub for hydrogen production.



Question 1: Approach to performing the work

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

- This is a well-designed and comprehensive view of the issue. The author has drawn from a wide variety of data sources to bring together a model that gives a clear picture of the challenges that may face tri-gen plants at renewable biogas locations. The project plan, design, and sources are clearly laid out. The objective to compare different types of tri-gen facilities is clear. The approach addresses all barriers.
- The project has a good approach and good ideas. It could be improved by adding a more realistic estimate of demand at sites based on vehicle miles traveled. Likely this will not change the fact that potential owners neither drive nor live near tri-gen facilities. Likely the heating load is a more binding constraint, as fuel may be transported to nearby stations that do have pass-by demand. Additional incorporation of underlying economic factors will add value.
- The approach employed seeks to overlap map locations of vehicle sales with wastewater treatment (WWT) sites and landfill sites as potential sites for feed gas for tri-gen of heat, electricity, and hydrogen (for future FCEVs). The approach of overlaying maps is an interesting visual technique to identify sites; however, the approach adopted is limited in its assumption that current sales of alternative fuel vehicles are an accurate predictor of future FCEV sales. The approach appears to be limited in flexibility—it focuses solely on tri-gen (heat, electricity, and the use of hydrogen for FCEV). If the approach loosened (e.g., to look at other sources of fuel, such as pipeline NG), the number of sites for tri-gen would increase dramatically. It was not made clear if the project was specifically constrained to wastewater, landfill, and FCEV attributes. If the intention was to show how to identify sites for tri-gen, then limiting them to a narrow set of constraints does not do justice to identifying the broader range of capabilities for tri-gen.
- There are good partners from the automotive developer side. There is good information on WWT and landfill gas (LFG) sites. Matching of heating and cooling loads is a good criterion. Partners from tri-gen developers will strengthen the study. The use of waste heat is another missing link in approach. The project needs to justify California versus the Northeast. The Northeast needs energy security also—post-Hurricane Sandy.
- The approach is reasonable. Linking geographic information systems (GISs) and alternative fuel vehicle sales to assess landfill sites and potential tri-gen locations is good. Use of alternative fuel vehicle data as currently implemented is problematic. The household income threshold of $75,000 as an indicator of the likelihood of being a FCEV early adopter is quite dubious. This number should be much higher. Perhaps the project could use family income as an alternative indicator. The application to the New York

metropolitan area is good (i.e., it is good to expand beyond just a California analysis as so many past analyses are focused).
- This is early in the project, and it is suspected that the investigators have not settled on their methodology. The current methods are very approximate. Heat loads for sites are not yet available. There is apparently no consideration of transporting the hydrogen from the site where it has been generated, even for short distances and even for mobile refueling equipment (this is an early market study). This is especially important because, as the investigators note, landfill and WWT plants are not typically good locations for refueling stations.
- Highly detailed, spatially resolved modeling efforts have questionable value when the economics of tri-gen operations are highly uncertain. Early adopters who can afford FCEVs are unlikely to get excited about driving to a landfill or water treatment site to fuel their vehicles. Hydrogen (or methane) from renewables (landfill and water treatment) is very costly and unlikely to be used in early markets. The analysis does not appear to have any assessment of resource size, i.e., how much methane/hydrogen the relevant landfill/water treatment facility can produce.
- Matching loads with potential sources of energy is a good idea, but it is unlikely that this project will get traction given that (1) state/local officials/permitting authorities (major stakeholders) have not been involved, (2) there have been no economic considerations included to calculate the economic disparity between the current system and the proposed tri-gen technology, (3) there is no calculation of the associated environmental advantages—it is important to calculate potential emissions/water consumption avoided—and (4) sources and consumer loads have not been matched. This study could use some additional collaborations to tackle the barrier of “siloed analytical capability,” which is not being overcome with the current approach. Also, it is unclear how “future market behavior” will be overcome as a barrier with the current approach.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- Given the stage of the project, the progress to date is excellent. However, there is a need for an improved analytical approach.
- Good progress has been made. It would be good to see more economic assessment to tackle the barrier of “future market behavior.” It is not clear how local geographies affect the economic assessment of tri-gen facilities.
- There seems to be tremendous progress for only 3% of project spending. The project has a good start but clearly needs to modify parameters (e.g., the $75,000 income threshold) and expand the study. The project needs to better define how large a station is. The presentation links vehicle population numbers to the associated station requirements, but this link is based on a hidden assumption of station size.
- The identification of the top 10 WWT and LFG sites is very good. NG can support biogas—it is a good strategy. The results and strategy for addressing the stranded assets issue with FCEVs during the initial period are very encouraging. The connection between FCEV locations and these sites seems to be improper. Population density is a better parameter.
- Given the fact that the project just started (the kickoff meeting was in February 2014), the progress is appropriate.
- Goals are fairly broadly defined, so this question is difficult to answer. If the first half is mapping and the second half something else, then progress is excellent. If the mapping portion is done, then there is a little more to do on this front.
- The ability to serve stations appears to be based solely on sites within a given distance. It appears to ignore factors related to the quantity of hydrogen that a given landfill or water treatment facility can produce. Heating loads are generally seasonal. It is not clear whether this is incorporated into the data.
- Data were acquired and mapped relatively quickly. However, the infrastructure scenarios will be incomplete if the appropriate techno-economic analysis is not carried out. The loads dataset needs to be cleaned up—thresholds should be identified for heat and electricity consumption needed by a building/group of buildings for which it would be ideal to provide tri-gen. For instance, there could be buildings with 70% heat load and 30% electric load—it is not clear how to ensure that the loads are met optimally (i.e., cheaper than the incumbent alternative). That could take much work and additional effort. It is not clear whether this can be achieved within the time frame and scope of the project.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- Industry partner and national laboratory contributions demonstrate clear collaboration with key stakeholders in the hydrogen arena. Toyota's involvement will help future goals of incorporating vehicle sales into the analysis.
- Collaborators include the National Renewable Energy Laboratory (NREL) and Toyota (car sales), but there are not any collaborations with a tri-gen company.
- Coordination looks pretty good with industry partners, as well as with NREL.
- Collaboration appears to primarily be with NREL. However, the extent of the collaboration is difficult to assess.
- It is too early to tell, really. The reviewer hopes there will be more outreach to industry to broaden the range of the premises and business models considered.
- Collaboration could be improved by bringing additional stakeholders to the table, including local and state officials as well as the regulatory authority that permits power generation projects to identify siting constraints and potential technology supporters. It would also be good to get NREL's input on the economics of the project and the design of the production and dispensing facilities.
- The principal investigator needs to find collaborators that can furnish economic data.
- The project needs to collaborate with stakeholders in the Northeast such as the Connecticut Center for Advanced Technology (CCAT). Other users of hydrogen should be added.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.0** for its relevance/potential impact.

- The relevance is very high. As stated in the scope, renewable, low-cost hydrogen will be in high demand.
- This is an excellent beginning.
- The project is relevant to assessing the viability and effectiveness of bio-based tri-gen facilities. Coupled with a study on the economics, this project would be useful to determine whether these could compete with NG in selected locations.
- Finding out the potential role of tri-gen with hydrogen production for FCVs is important. It could be a valuable source of hydrogen during the early transition, but this kind of analysis is needed to inform the issue.
- The approach is interesting and makes good use of mapping data, and it provides a tool for identifying future sites for cogeneration and production of hydrogen.
- The project is identifying actual potential infrastructure scenarios for long-term transportation needs and early market opportunities, which aligns with the Multi-Year Research, Development, and Demonstration Plan. However, the impact and likelihood of success are low, given that there is no environmental or socioeconomic analysis and that the right stakeholders have not been involved to increase the likelihood of implementation.
- The work is significant to only one path. There is a high-cost biogas pathway, and it is unlikely to have an impact in early markets with high risks because of the uncertain market size. It should be considered only for mature markets where it is likely to be implemented. Without economic estimates, this work has little relevance to addressing FCEV introduction.

Question 5: Proposed future work

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

- Considering the size of the project, the activities are well defined and should be readily achievable.
- The goals seem pretty broad, so the future work looks flexible. It is not really clear what the future work entails, exactly. The research can go in any direction, but it is not clear what that is.

- More attention should be paid to the economics, as market behavior is a barrier being addressed. It is not clear what the cost is going to be in the different geographies.
- The project needs to give more thought to the premises concerning operation of tri-gen installations and the use of the hydrogen, especially whether it can be transported or loaded into mobile refuelers. The researchers need to consult with tri-gen equipment manufacturers and hydrogen suppliers on these issues.
- The first priority should be the incorporation of economics in the work. Until the economics are clear, the other issues are peripheral.
- The project needs to add hydrogen use for forklifts and grid support and other parameters—it is not clear whether there are fleet vehicles located near a big LFG and WWT plant.
- The future work looks okay, but it is missing elements.
- The description of future tasks lacks detail.

Project strengths:

- The project is very organized, and the approach is clear. Data gathering has been a clear strength as well, with a variety of sources being leveraged to come up with this model.
- The use of mapping data to identify energy sources (e.g., waste gas methane and LFG) and energy consumption (heat, hydrogen, and electricity) is excellent.
- The project makes good use of geographic data and the Spatially & Temporally Resolved Energy & Environment Tool (STREET) model.
- The project has a reasonable approach that will lead to interesting results. The team seems to be applying the correct/useful tools to the analysis.
- This is a great idea to start with tri-gen. The project should have more stakeholders from the Northeast provide input, including an assessment of how quickly stations can transition to profitability—sell power to the grid or a WWT plant.
- Good GIS modeling capabilities are present. Energy loads and locations of potential sources of clean hydrogen are available.
- The subject is good. It is generating potentially important results. The project is led by those with direct experience in the tri-gen field.

Project weaknesses:

- The $75,000 income threshold is a dubious filter value.
- There are no economics.
- The constraints associated with the fuel source (landfill/waste gas versus pipeline gas) limit the siting and potential market for the tri-gen concept.
- The project should address capacity utilization as an added parameter. WWT and LFG can support over 100,000 FCEVs. The team needs to double check its math. The heat load of WWT plants should be considered. Perhaps the project should involve General Motors, FuelCell Energy, CCAT, Rutgers, etc.
- State and local officials (major stakeholders) have not been involved. There have been no economic considerations to calculate the economic disparity between the current system and the proposed tri-gen sites. There is no calculation of the associated environmental advantages—it is important to calculate potential emissions/water consumption avoided. Sources and consumer loads have not been matched.
- To truly do this analysis right, a more complete incorporation of economics is needed. A more explicit representation of the limitations of the analysis is needed. The team needs to know what the analysis does well, what it needs, and what will it never do. There are aspects outside the scope that the project will never fully answer, but these things need to be explained or addressed in some fashion. Because the progress so far has been in mapping, a better representation of demand should be incorporated. Also, if the team is using any numbers from the census, they should look at the $150,000+ numbers for early market launch.

Recommendations for additions/deletions to project scope:

- This is a great project. Having input from the NG industry, WWT, and LFG is recommended. The use of waste heat from external sources can improve siting economics of tri-gen systems and increase the speed of payback.

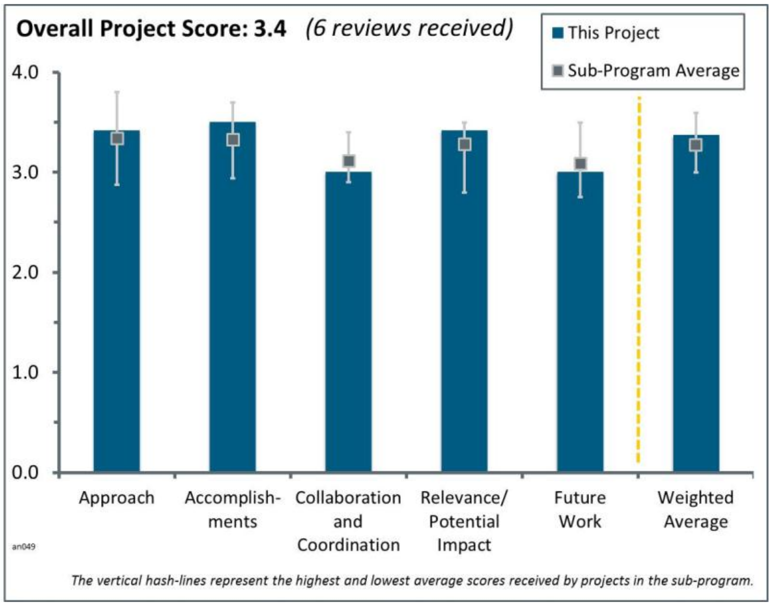
- The project should loosen up the constraints and consider pipeline sources of NG. The project should also consider increasing the six-mile-drive-time service coverage to ten miles—using this as a sensitivity variable.
- The project should consider transporting hydrogen by tube trailer or direct loading into mobile refuelers. The project should also consider short distance movements to better locations, in general.
- An economic analysis should be added, as should an assessment of the hydrogen production capacity of landfills and WWT.
- The project should involve regulatory authorities and local/state officials to provide input. Economic considerations should be included to calculate the economic disparity between the current system and the proposed tri-gen technology. The associated environmental advantages should be estimated—it is important to calculate the potential emissions/water consumption avoided.
- The project should do the following: consider hub arrangement for hydrogen generation and use; include more specifics in future plans; raise the $75,000 income threshold; quantify the levels of heating load; and assess the cost and the feasibility of various distances of heating sources to hydrogen generation (i.e., potential heating/electrical loads are listed at various distances from the landfill, but it is not clear at what distance it is cost-prohibitive).
- The project should add more on the systems-level analysis of the tri-gen systems. The costs and benefits of different sizes of systems are not clear. It is not clear what can we learn from the existing University of California–Irvine tri-gen system and what is transferable.

Project # AN-049: Electricity Market Valuation for Hydrogen Technologies

Joshua Eichman; National Renewable Energy Laboratory

Brief Summary of Project:

This work explores future market opportunities for hydrogen technologies and expands modeling capabilities for integration with the grid. The objectives of this project are to evaluate the ability of electrolyzers to bid into electricity markets, to assess the value proposition for grid integration of hydrogen technologies, and to include hydrogen technologies into large-scale grid operation models.



Question 1: Approach to performing the work

This project was rated **3.4** for its approach.

- The study was exceptionally well done. Appropriate methods were used to evaluate the potential for profitable operation of electrolyzers producing and selling (or not selling) hydrogen, providing grid services (or not), etc. The results provide clear guidance on the prospects for integrating electrolyzers with the grid while producing hydrogen for sale to the transportation market.
- The approach of the model was clear, and in particular, the authors' ability to convey the objectives of the project in a concise and logical manner was impressive.
- The project is well organized and logical. The objectives are particularly well stated.
- Introducing hydrogen production technologies into the electricity market is an interesting option to generate a revenue stream, particularly during the early stages of fuel cell electric vehicle (FCEV) penetration. The approach addresses all the barriers described. The approach would be even more interesting if it incorporated a trade-off analysis of hydrogen technologies versus incumbent technologies. Also, solid oxide fuel cells (SOFCs) could be analyzed. Bloom Energy has 50/100/200 kW commercial fuel cells that could be used for non-spinning reserve and/or supplemental reserve.
- The use of electrolyzers for frequency support is a good near-term opportunity.
- The project effectively established and validated water electrolysis efficiency for polymer electrolyte membrane (PEM) and alkaline electrolysis through equipment testing at the National Wind Technology Center (NWTC). Credit must be given that the analysis results of the energy supply concept were not compared to the full gamut of energy storage options. In a thorough review of the analysis, the results contain a high level of uncertainty.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- There is good progress. Results are surprising and insightful. However, it would be good to see how the technologies compare against an integrated gasification combined-cycle (IGCC) turbine or a single-cycle turbine in terms of economics. This project proposes a potential solution to the revenue issue being faced by hydrogen production technologies.
- The project has made good progress and has already developed some clear conclusions. It would be good to see a bit more work on the inputs of the model. Currently, it appears to be more like a sensitivity study, and

it would help to have a better feeling of realistic values based on other models at the National Renewable Energy Laboratory (NREL) that output these values.
- There is nice overall work in establishing an analysis framework to analyze hydrogen storage markets. It may be good to pare down the technology architecture to the highest-value market, which may be very valuable for DOE to understand at this early concept stage.
- The results of this study should lead to demonstrations and eventually to deployment.
- There are good modeling results.
- The project has produced a large body of analysis results. Proof of electrolyzer flexibility is largely asserted rather than proven by analysis/testing. The backup slides offer some values but are not well explained. Also, only some modes of electrolyzer flexibility were tested, with other aspects being out of work scope. The project should more clearly identify the relative value of each mode of flexibility to ensure that data are collected on the most cost-effective aspects. The bar chart graphical display showing the range of costs for each configuration is very effective in conveying the complex modeling results.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- The project has good partners and uses resources well.
- The project included dialogue with electric power and water electrolysis stakeholders.
- The principal investigators involved a number of good stakeholders, but it would have been good to see more regulators and potential adopters involved from the beginning to generate additional feedback and improve the likelihood of technology adoption. Another potential collaborator could be a manufacturer of voltage control devices (e.g., static volt–ampere reactive [VAR] compensators, static synchronous compensators [STATCOM], etc.).
- Collaboration appears to be primarily briefing results after the analysis has been completed. Perhaps earlier interaction occurred, but it does not show up in the presentation. Also, collaboration is more than just briefing results; it must also include incorporating feedback into the analysis. There is no specific evidence that this occurred.
- Collaboration is not a big part of this study.
- This is probably the weakest point of the project. Although the material has been “presented” to stakeholders, it would be beneficial to see more involvement by stakeholders in the development of the model. The project should move from an after-the-fact review to getting input up front from stakeholders on how the model should be designed.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The conclusion offered thus far, that it makes sense to have electrolyzers participate in the grid, is valuable. The further clarification that electrolyzers offer advantages for things such as frequency control and response time is useful. Finally, knowing that integration in the grid makes sense only if the hydrogen is being sold helps to guide future research.
- The study makes an important contribution by demonstrating how the responsiveness of electrolyzers allows them to provide the full range of grid services and that this adds substantial value that could be used to significantly lower the cost of hydrogen produced by electrolyzers.
- Using hydrogen for transportation and extending its use is a win-win. It will help reduce hydrogen costs for all applications.
- The project effectively leveraged the capabilities and resources available at NREL and linked new potential technology to current and future energy markets.
- Introducing hydrogen production technologies into the electricity market is an interesting option to generate a revenue stream, particularly during the early stages of FCEV penetration. The topic is relevant to a

number of stakeholders, and it could even help with the integration of other renewable energy technologies such as wind turbines by providing the necessary ancillary services to the grid.

Question 5: Proposed future work

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

- It would be good to see additional future work to explore the feasibility of the concept, including a proof of concept. Also, it would be good to see how these technologies compare against incumbent technologies that serve the same purposes.
- Publications should contain full disclosure of assumptions and sensitivity analysis on the results.
- The project needs to add other pathways—a tri-generation system can complement electrolyzers.
- It does not appear from the list of future work that this project is continuing further. The project team should look at how it can enhance this analysis and if it can further guide things such as the size of the system that would be integrated, etc.
- The proposed work is only generally described. Less than half the money has been expended, and two out of the three bullets for future work relate to writing reports/journal articles. The third bullet represents a potentially large body of work but is not described.

Project strengths:

- The project has appropriate methods and data. The strong study design is producing useful answers.
- The established analysis framework to study the integration of hydrogen production into the electricity grid is a strength.
- The team has a good understanding of the subject. The project addresses an important barrier—the poor economics during early introduction of FCEVs.
- The large analysis scope appears to be executed well.
- The modeling tools are a project strength.

Project weaknesses:

- The conclusions could be stronger and more clearly stated.
- The project needs to add the International Organization for Standardization (ISO) and the Federal Energy Regulatory Commission as stakeholders. Monetization of ancillary services is needed.
- The project needs additional input from potential adopters. It needs more analysis of other technology options. There is little information on large-scale electrolyzers.
- This early stage type of analysis requires more transparency on the underlying assumptions. There are many elements of this project that are not clear: (1) the selling price for power returned to the electrical grid; (2) the price mix assumptions for hydrogen injected into pipelines, dispensed to fuel cell vehicles, or sold to industrial processes; (3) what the market mix is; (4) the power cost to run the electrolyzer; (5) whether the duty cycle for the electrolyzer is 24/7 or off-peak; and (6) whether the water electrolysis unit operates on 100% renewable energy or a combination of wind and grid mix. The cost contained in the project analysis was limited to water electrolysis production unit, storage, and installation costs. Additional costs associated with hydrogen compression and grid integration in providing ancillary services can add significant costs to a business concept and have a material impact on the overall results. It is very difficult to conceive that a hydrogen production system of a mere 500 kg/day capacity can cost-effectively be integrated into multiple markets.

Recommendations for additions/deletions to project scope:

- This is great work. The project needs to add California and Hawaii scenarios—look at what the problems are in the grid, how they are currently being handled, and how hydrogen pathways can improve them.
- The project needs to know the potential size of the market. It is not clear how much hydrogen could be produced in this way.

- Commercial success of this hydrogen supply concept is dependent on electricity price arbitrage, so results presented should include sensitivity analysis to reflect uncertainty and boundary-level results, especially because the PEM unit efficiency assumed in the analysis was 30% higher than results achieved at the NWTC test center; the price taker assumed ideal operation with perfect day forward forecasts.
- More analysis of the meaning of the cost results is needed. Some configurations are clearly not competitive. Others appear more promising and thus merit highlighting with further specification of how they might be implemented.
- SOFCs should be included. Bloom Energy has 50/100/200 kW commercial fuel cells. The project should be compared against other technology options to establish a baseline—this could include IGCC, single-cycle turbines, etc.

Attendee List: 2014 Hydrogen and Fuel Cells Program

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Adams	Michael	University of Georgia
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Ahn	Sang Hyun	National Institute of Standards and Technology
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Sub-Program Comments Provided by Reviewers

Hydrogen Production and Delivery Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- The overview review does an excellent job of giving a highly structured presentation of the overall strategy.
- Yes, the presentation covered the program clearly.
- Yes.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes, this comes out clearly in the overview presentation.
- The goal of the DOE Hydrogen and Fuel Cell Program is to achieve less than $4/kg of hydrogen in the next six years. It was hard to tell from this presentation how realistic this is—if it is realistic, then the emphasis on near- and mid-term (five years) research is appropriate. If the near-term research is successful and infrastructure is in place to deliver hydrogen using near-term sources, the longer-term work on developing sustainable sources will have an eventual home. Under this scenario, longer-term work can be funded at a lower level, and the investments in U.S. Department of Energy Basic Energy Sciences (BES) can be leveraged to build commercially viable new technologies. If it is not realistic that the cost target can be reached, then an analysis of feasibility and areas for focus will help develop new funding priorities. It is essential that the near-term infrastructure for hydrogen delivery and production is in place using current sources of hydrogen in order for the new technologies to have a chance to succeed.
- No, the sub-program is failing to address the mid-term research and development (R&D) needs.

3. Were important issues and challenges identified?

- Slide 4 is an excellent summary of the important issues and challenges.
- The issues and challenges were discussed.
- Some of the important issues and challenges were identified.

4. Are plans identified for addressing issues and challenges?

- Yes, plans are identified for each issue/challenge.
- Yes, plans were identified. However, plan development with industry representing the stakeholders will be short-term focused. Perhaps it would be useful to have another layer of plan development to assess the balance between long-term and short-term investments.
- Some plans were identified.

5. Was progress clearly benchmarked against the previous year?

- Yes, the overview slides clearly indicate progress versus 2013.
- Yes, cost trends were presented and discussed.
- No.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- The overview presentation shows a very-well-structured roadmap of problems that need to be addressed.
- To some degree, the projects are addressing the relevant problems and barriers.
- Yes.

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- The overview presentation shows strong focus in addressing the identified barriers.
- Yes, this sub-program appears to satisfy FCTO's needs. There is a need for some examination regarding whether the resources available are up to the scope defined. Opportunities for leveraging related work could be evaluated as a means for extending the value of the Office of Energy Efficiency and Renewable Energy's (EERE's) investments.
- Yes, with the exception of focusing on mid-term needs.

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- There is lots of good-quality R&D on long-term needs, such as novel hydrogen pipelines and concrete/steel composite tanks. A weakness is failing to address the mid-term needs of the hydrogen and fuel cell space; in the near-term, most hydrogen will be produced on-site by electrolysis or steam methane reformers or with truck-delivered gaseous hydrogen. Very soon, however the industry will be shifting to liquid hydrogen delivery. This is already happening with the five Linde liquid hydrogen stations in California. In addition, most of the hydrogen used at warehouses for the growing fuel cell material handling equipment (MHE) market is trucked-in liquid hydrogen, and Praxair has announced a 50% expansion of its liquid hydrogen production capacity at Niagara Falls. Other industrial gas companies (IGCs) have excess liquid hydrogen capacity in the East. The sub-program should be focused more on reducing the cost of liquid hydrogen stations. There is a need to develop lower-cost and more reliable liquid hydrogen pumps; for example, to replace or reduce the need for hydrogen gaseous compressors. (It seems that Linde's use of its proprietary ionic compressors at its liquid hydrogen stations may drive up the cost and lower the reliability of these stations.) In addition, there is too much emphasis on electrolysis. Strategic Analysis, Inc. (PD-102) has shown that the likely cost of electrolytic hydrogen will range between $7.58/kg today and $5.79/kg in the future, indicating that there is little hope of reaching the U.S. Department of Energy (DOE) target of less than $4/kg with electrolysis. Another weakness is the absence of projects (with the exception of PD-091) to develop hydrogen from landfill gas or anaerobic digester gas, both of which can lead to zero-carbon hydrogen production.
- The key strength of the sub-program is the comprehensive view of the opportunity and the scope of the work that needs to be done to bring widespread use of carbon-neutral fuels to reality. The strength of the sub-program lies in the short-term projects, and the focus on forecourt and other needs to deliver fuels to consumers. The longer-term portfolio includes top talent, but it seems to be underfunded. It seems like resources were only available to support one of each kind of approach, rather than a best-of-breed approach. This is high risk in my experience. The working groups are a great way to benefit from broad experience, but they are more focused on knowledge aggregation than innovation. An examination of strategies to meet long-term needs given the resources available could be valuable at this point.
- It is sometimes difficult to assess the progress of some of the more long-term projects.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- The long-term projects in particular are very novel approaches to the identified barriers.
- The projects are more on the safe side. The need for short-term progress (in short-term and long-term portfolios) works against taking risks and being innovative.
- In some cases, the projects represent novel and/or innovative ways to approach the barriers.

10. Has the sub-program engaged appropriate partners?

- The sub-program is impressive in its engagement activities with industry. The partnerships with science programs could be stronger, both inside and outside of DOE. Hopefully a partnership with BES would be in place before a partnership with the National Science Foundation (NSF) is developed, because the missions are more naturally aligned.

- Yes, the sub-program has engaged appropriate partners for the topics chosen. If the sub-program were to focus on delivered liquid hydrogen, then there would have to be more collaboration with the IGCs and/or their liquid hydrogen component suppliers (such as liquid hydrogen pumps).
- It appears that the sub-program has engaged appropriate partners, given the progress that is shown.

11. Is the sub-program collaborating with them effectively?

- Yes, the collaborations described seem to be effective. What could be considered is the balance of voices to ensure that input on long-term as well as short-term research priorities is available for decision making.
- Yes, this appears to be the case.
- For the most part, the sub-program is collaborating with them effectively.

12. Are there any gaps in the portfolio for this technology area?

- There do not seem to be gaps, but there are too-hard boundaries at the edges. In particular, where hard science is needed to overcome technological obstacles, there seems to be little means to do what is needed.
- Perhaps there are gaps in some of the long-term approaches, but this is a difficult call to make because there are some highly immature technologies that are probably outside the scope of the technology area and should belong more in BES.
- Yes. The sub-program needs the following:
    - A project to reduce the cost of liquid hydrogen stations (trucked in liquid hydrogen with 700 bar gaseous dispensing).
    - More of an emphasis on landfill gas and anaerobic digester gas sources of hydrogen to generate zero-carbon hydrogen.

13. Are there topics that are not being adequately addressed?

- Broadly, the balance is thoughtful. There are details that can be improved, which is normal.
- It would be useful to see the infrastructure financial analysis that is needed to attract investors as part of this technology area.

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- The areas selected are appropriate.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- The materials work described in the reports could be augmented. DOE does not have as strong of an applied materials program as it needs.
- It might be useful to benchmark the rollout of infrastructure in other areas, such as compressed natural gas and liquid natural gas.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- The sincere dedication of the program managers in this area is to be commended. The only way to improve the sub-program’s effectiveness is to increase outreach to external scientific communities. Awareness of the challenges will increase research (funded by other sources) that will benefit the sub-program and the nation over the long term.
- The sub-program should routinely compare the delivered cost of hydrogen by various avenues (e.g., on-site production, trucked-in gaseous hydrogen, and trucked-in liquid hydrogen), review what the IGCs are using at warehouses and at fueling stations, and focus projects to minimize the costs of those pathways.

Hydrogen Storage Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- As usual, the description of the sub-program was quite clear, as evidenced by the lack of any substantive questions following the presentation. The goals, current status, and future trajectories and strategies of the sub-program were described in the level of detail appropriate for the overview.
- The sub-program was described well, and the long- and short-term strategies were presented.
- The overall strategy was clearly communicated.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- With the completion of the three materials centers and the engineering center wrapping up, it would appear that the balance in the sub-program is shifting to more near-term development projects at the expense of performing on the more difficult, riskier, but potentially higher-payoff mid- to long-term research projects where DOE is expected to excel. Shortening up the time horizon to two-year projects that are largely development projects begs the question of how near is near for the Hydrogen Storage sub-program, and where the low-risk, low or moderate benefit line crosses over. The newer projects directed at improving high-pressure tanks are, albeit interesting, perhaps too close to where industry is playing now that 700 bar tanks are on the horizon for commercial deployment. It is unclear if industry really needs DOE assistance and motivation to make incremental changes in resins, or resin additives, for example, to make incremental improvements in performance and/or cost. Perhaps the pendulum has swung too far. Perhaps this is also somewhat driven by DOE's new rules on procurements, which has substantially slowed funding opportunity announcements (FOAs) over the last couple of years. Perhaps this trend will be reversed with more FOA opportunities.
- Unfortunately the sub-program has too much emphasis on short-term R&D; for example, on high-pressure gas tanks and engineering material-based systems, as opposed to long-term innovative research approaches to overcome existing limitations with the gas tanks.
- This was difficult to assess without seeing the budget breakdown between these areas.

3. Were important issues and challenges identified?

- Issues that remain were nicely identified by the now common practice of placing progress on easy-to-understand spider charts, where the "white space" clearly identifies the technical barriers that still remain to achieve the target(s). The sub-program has worked nicely to provide this information all on a fairly common set of parameters, so that each system approach, or material approach, may be quickly evaluated and compared as far as progress is concerned. One example where the sub-program has done a very good job in identifying future opportunities is in the chemical hydrogen storage area, where the sub-program has "reverse engineered" a basis set of minimum materials requirements that will be of substantial help in guiding future R&D in chemical hydrogen storage materials and approaches. This does not seem to be available (yet) for the metal hydrides, but does seem to be "in progress" for the sorbents. These "reverse engineered" materials properties will all hopefully be made available for the various materials approaches as part of the Engineering Center's final report, or associated publications.
- Short-term issues of cost were addressed. For the long term, it is obvious that breakthroughs are needed, but it is not clear how the materials being targeted will address the gaps.
- Issues and hurdles were identified and explained.

4. Are plans identified for addressing issues and challenges?

- Plans were explained.
- The spider charts do a nice job of describing where there are still unmet technical challenges to overcome in either the materials or systems. There was some discussion of where the remaining challenges are (e.g., carbon fiber costs, off-board inefficiencies), but plans to address the challenges were largely not made available. Clearly there are ongoing plans and actions to address the carbon fiber cost issue, but plans for addressing many of the materials or systems deficiencies were not explicit.

- Sort-term strategies to lower carbon fiber costs were clear, but it was not clear that relevant targets and metrics exist to assess the success of new materials-based storage systems.

5. Was progress clearly benchmarked against the previous year?

- The spider charts provide a very clear representation of progress as a function of time. Presentation of this information gives a readily interpretable snapshot in time of where things were over the last several years, and even a glimmer of evidence as to where they might get to in the future. They are a very nice addition to how the overall Hydrogen Storage sub-program displays a large volume of data in a readily interpretable format, and one that surely drove the Engineering Center toward.
- Progress was communicated and benchmarked.
- Short-term carbon fiber accomplishments for polyacrylonitrile (PAN) precursors were clear. For other areas, 2014 progress was not as obvious.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- Storage of hydrogen for either vehicular or niche applications is crucial for the successful deployment of fuel cell technologies, and the sub-program is providing significant strides forward in anticipating future hydrogen storage needs. Thus, the sub-program is crucial to overall FCTO success. With high-pressure tanks in commercially available fuel cell cars coming onto the market, the sub-program has the opportunity to reassess barriers for advanced hydrogen storage concepts (e.g., materials for onboard applications).
- The projects are partially addressing the challenges. It would be beneficial to have more emphasis on material-based approaches.

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- The sub-program is very well managed. The sub-program lead is a technical expert in hydrogen storage, and the rest of the team contributes substantially to maintaining the focus of each and every project within its portfolio. The project management team is obviously very valuable to FCTO in that it is highly engaged on a day-to-day basis with its entire portfolio. The team responds well to change, makes decisions, and communicates easily with all of its principal investigators (PIs) and Engineering Center members. This sub-program must be considered exemplary in addressing the nation's needs in storage to achieve the broader FCTO goals.
- It is well focused.

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- The sub-program's strong technoeconomic analysis is a strength.
- One weakness is that there is some overlap in the Argonne National Laborotory system analysis activities and the Engineering Center activities, and likewise between the cost analysis portion of the sub-program and some of the Engineering Center activities. Perhaps some overlap or repetition is good as a crosscheck, but one must not allow it to go too far for too long. An additional weakness is that the portfolio has become quite near-term in focus. Perhaps that will be remedied in the next several rounds of potential FOAs. Also, another weakness is that there is a sorption project (ST-103) that is still operating a lot like a BES project, and it has not really responded to input on what it takes to succeed in an applied research program. A strength of the sub-program is the excellent communication among the stakeholders, the PIs, and DOE to drive the projects forward more effectively. The Engineering Center has become a very strong example of this.
- A weakness is the focus on short-term activities.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- As the Centers ended, and the Engineering Center winds down, the number of novel approaches or degree of innovation in the overall sub-program has declined. Perhaps this is to be expected because these previous projects have provided many more technical constraints and boundaries as to how to meet all of the targets simultaneously. The "phase space" for successfully meeting all the targets simultaneously with one material and one system has certainly been dramatically shrunk, which may be wringing much of the potential for a high frequency of innovation out of the field. It is a very hard problem to address in innovative ways within the constraints of dollars and time that DOE places on researchers.

10. Has the sub-program engaged appropriate partners?

- Absolutely—it is hard to imagine the sub-program doing this any better.
- It did engage appropriate partners.

11. Is the sub-program collaborating with them effectively?

- Yes. The sub-program is internationally recognized for being a collaborative, innovative focal point.
- The sub-program is collaborating effectively.

12. Are there any gaps in the portfolio for this technology area?

- The sub-program needs to lengthen out the time horizon to attract more innovative approaches that may be quite risky, but with high payoffs if successful. The sub-program has transitioned to a time horizon that is too short, which makes it difficult to propose highly innovative, albeit risky, R&D. The periodic table is not getting any bigger (for stable elements). The sub-program may want to go back and revisit "nitrogen trihydride" and update its view on onboard ammonia. There are recent advances that may indicate that some R&D in this area of nitrogen-hydrogen compounds is appropriate. While off-board efficiency has been identified as a significant barrier for some materials classes, there is no current plan to address those barriers. This may be a very-high-risk program element, but if successful, it can enable some high volumetric and gravimetric systems sometime in the future.

13. Are there topics that are not being adequately addressed?

- [*No responses provided.*]

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- [*No responses provided.*]

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- Losing the Centers could be expected to substantially reduce collaboration, creativity, and innovation, and the ability for DOE to rapidly move ahead in chosen areas where the Center concept might work. DOE might want to rethink how to structure future solicitation or opportunities around some variant of the Center concept.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- The sub-program is spread too thin in the medium- to long-term research that FCTO is good at. There needs to be a rebalancing of the portfolio, and DOE needs to take a closer look at how near term they want to be. DOE appears to currently be encroaching in areas that are best suited to industry, and where DOE teams are less effective.
- The sub-program should include more projects for long-term R&D.

Fuel Cells Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- Yes, the sub-program has been well covered. The overview presentation is clear and describes very well the expectations, the barriers to overcome, the main accomplishments, and the future activities. However, having information about the targeted and current system density (kW/l and kW/kg) would have been appreciated.
- The sub-program presented an excellent summary of strategy and goals that are measurable and correctable. The focus areas look good. The use of automotive fuel cells in grid-support-related R&D focus/needs will be helpful.
- The strategy was adequately covered. The focus is on R&D of materials, components, and the balance-of-plant (BOP) system to address the cost and durability barriers, which are clearly the key barriers for commercialization.
- The sub-program covers the necessary technical focus for the pre-competitive phase of research for automotive fuel cells. The area of advanced analytical methodology to support fuel cell R&D is an opportunity to further efforts. This area may be beyond what FCTO covers. Perhaps collaboration with other DOE offices could be pursued to develop an FOA in this area. The current projects on microscopic analysis at ORNL and neutron imaging at the National Institute of Standards and Technology (NIST) are good models for this area.
- Yes, the sub-program presented a good overview of its overall strategy, progress, and accomplishments.
- The overview presentation provided a good overview of the sub-program. However, it feels like the overall strategy is a bit weak on specifics and too broad in scope.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes. The development of advanced materials to cell, stack, and system testing, as well as validation testing, are important parts of balancing. The use of market-driven parameters in planning is excellent. The high-volume projections are good but not sufficient. A transition strategy is needed from low volume to moderate volume.
- Resources are appropriately allocated for long-term and mid-term R&D.
- The balance is appropriate.
- The near-, mid-, and long-term R&D is balanced; however, more emphasis is needed on fundamental understanding of the different stack component degradation mechanisms and their coupling, component interfaces, and changes in materials properties.
- The sub-program is balancing the near and long terms fairly well. However, the strategy for the mid term is unclear. Most fuel cell original equipment manufacturers (OEMs) are concerned about the commercial “valley of death.” It is unclear whether FCTO is responsible for and mandated to think about the mid-term commercial challenges.
- Yes.

3. Were important issues and challenges identified?

- Yes. Cost, life, and reliability are important barriers. The sub-program has a good strategy to handle them. The use of national laboratory resources and feedback from industry to guide decisions and planning is very productive.
- Cost and durability have been well identified as the main challenges to overcome. Another big challenge for commercialization will be the quality control of the component manufacturing; in particular, the membrane electrode assembly (MEA) and bipolar plates.
- The research focus area is consistent with the views of OEM partners in the U.S. Council for Automotive Research (USCAR), and important issues and challenges are identified.
- The challenges/issues were highlighted and appropriately addressed as much as possible in the limited number of slides.
- Yes. Cost targets are well outlined and current cost components for fuel cells have been identified.

- Important issues and challenges have been identified and have remained the same over the past three or so years. The remaining challenge of the cost of fuel cell systems is beginning to asymptote at around $55/kW, while the target for 2020 is $40/kW. The cost reduction trend is not encouraging; it is unclear whether the sub-program is concerned about this trend and, if so, whether there is a focus on funding the key enabling technologies.

4. Are plans identified for addressing issues and challenges?

- The sub-program features good plans to address major issues. The use of analytical tools and input from stakeholders is very well done.
- Yes, in general. Among them, one would notice the better understanding of the degradation mechanisms, the development of new materials such as catalysts or critical BOP components such as compressors, the improvement of the MEA, and system modeling.
- Yes, they are.
- Plans for cost reduction in automotive fuel cells are unclear; while the focus area has been identified, the specifics are not clear. Pt reduction has been identified as a key focus area; however, it is not clear what more could be done. The high-activity catalyst (>10x compared to Pt/C) has been reported since 2009. However, these are not being used on the MEA scale to collect data or improve the cost model. The strategy seems to be stuck in neutral (i.e., the focus area is not resulting in additional cost reduction).
- Many projects that cover important challenges are ending or ended in this fiscal year. Therefore, the research project portfolio needs to be fixed. The new FOA currently planned is imperative to fix the research project portfolio. Also, it is important to track fuel cell R&D funding by other countries' governments and keep DOE R&D portfolio updated and competitive.
- Yes.

5. Was progress clearly benchmarked against the previous year?

- Yes. Good progress is being made. There is a nice list of parametric achievements.
- The progress is clearly highlighted by key new research findings. The highlighted results are very exciting; however, in some cases, the technology is by far too immature for one to understand its true impact on fuel cell commercialization (e.g., no MEA performance results exist).
- The sub-program was benchmarked against the previous year for automotive fuel cells. There was no clear benchmarking for other areas such as combined heat and power (CHP) or materials handling applications.
- Yes; however, for the budget section, it would be nice to show bar charts for budget amounts for at least the last five years to give a perspective of whether the budget is increasing, staying same level, or decreasing.
- In general, all the presentations explained the new achievements from this year.
- Further analysis on fuel cell system cost trends might be necessary. The trend of estimated cost shows a plateau for the last four years despite significant R&D efforts. Going forward, it will be important to maintain priorities and focus for additional funding and further R&D efforts to succeed.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- Yes. There is a good portfolio of solution options and progress trackers. Additional opportunities are identified that can ensure better performance results.
- Yes. Nevertheless, there are no more projects dealing with bipolar plates even though it is not clear if the announced cost targets have been achieved or if the durability of the metallic plates will be as high as needed, in particular for stationary applications.
- The projects in this technology area are partially addressing the barriers that FCTO is trying to solve. However, they do not address the broad problems in the area.
- Generally, the projects do address the barriers of fuel cell commercialization. However, many projects did end this year, and there will be a gap until the new funding is in place.
- FCTO is trying to solve problems properly. Most of the projects that focus on addressing key challengesare ending this fiscal year. The currently planned FOA should be pursued in appropriate timing to address this issue.

- Yes.

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes. OEMs' input is seriously considered, which is a very positive thing.
- The sub-program is well managed. The focus area still needs to be further refined to address the most challenging issues.
- Yes. Coordinating withother EERE offices such as Wind and Solar on energy storage challenges would enhance the value further. The workshop on multi-fuel flexibility and gas clean-up, including biogas and shale gas applications, would lead to expanding the market base and further cost reductions.
- The sub-program seems generally focused; however, there seems to be an imbalance between catalyst and MEA/cell R&D.
- Yes. *(2 responses)*

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- Strengths of the sub-program include that almost all of the funded projects are focused on issues identified by FCTO, and that the projects have very strong teams with excellent cross-collaboration between academia, laboratories, and industry. One weakness is that, although rotating disk electrode (RDE) testing should be used as a catalyst screening tool, many projects highlight RDE results as their achievements in catalyst development and these results in most cases do not correlate with MEA results. FCTO should not encourage National Science Foundation-type activities. Non-platinum-group-metal (PGM) projects are showing type iV performances under air, which is very promising, but many ultra-low-PGM projects keep focusing on RDE, which is very disappointing.
- The sub-program's strengths include the quality of the researchers involved in the projects, and the well-structured project organization with "SMART" (specific, measurable, attainable, realistic and timely) objectives. Weaknesses include the possible gap in collaboration between the researchers and industry, leading to a lack of real system understanding from the researchers.
- Strengths include the sub-program's world-class talent pool, excellent transparency, and merit review. A weakness is the sub-program's inability or lack of emphasis to move cutting-edge technologies from the laboratory to the real world by developing an adequate supply base.
- The sub-program has made good progress toward its goals. However, it also needs to focus on multipurpose solutions; as crosscutting solutions can increase near-term cost reduction. For example, development of fuel cell use for grid-support working with the DOE Office of Electricity Delivery and Energy Reliability (OE), and fuel-flexible cleanup systems for DOE Bioenergy Technologies Office and Office of Fossil Energy (FE) applications.
- The key strength is the continual reassessment of the fuel cell system cost for the different fuel cell applications to understand progress toward the 2020 targets. Another strength is the novel approach to the development of new catalysts. A weakness is that some of the project approaches are not systematic and do not generate fundamental understanding.
- The sub-program is well organized and communicates with U.S.-based automotive OEMs. It also leverages the advantages of the technical resources of national laboratories. However, the project portfolio needs to be extended; the planned upcoming FOA will be able to fix this,

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- There are some excellent outcomes with novel and innovative approaches; for example, the nanoframe catalyst.
- Yes. The use of nanomaterials, non-Pt modeling tools, and high-temperature fuel cells is good. Higher-temperature membrane work will help both automotive and grid-applications as well.
- There is a correct balance between novel and/or innovative projects and more conventional projects.
- Many of the projects are trying novel ways to address these barriers.
- No.

10. Has the sub-program engaged appropriate partners?

- The sub-program has strong engagement with U.S.-based automotive OEMs through the U.S. DRIVE Partnership.
- Yes. There is room to leverage resources with the U.S. Department of Agriculture, the U.S. Environmental Protection Agency, and OE-grid support applications—that would be beneficial. Multipurpose demonstrations with crosscutting support from other partners will increase the value being created by the current efforts.
- Yes; however, it is unclear how FCTO is leveraging similar work from other countries such as Germany and Japan. DOE conducts the Hydrogen and Fuel Cells Program Annual Merit Review (AMR); however, there is no similar-scale review for the projects funded by the New Energy and Industrial Technology Development Organization and the Fuel Cell and Hydrogen Joint Undertaking.
- The national, international, and industry collaborations are all captured.
- Yes. *(2 responses)*

11. Is the sub-program collaborating with them effectively?

- Yes. Having the collaborating partners at future peer reviews to talk about the synergistic values will be great.
- It is unclear. A strong collaboration needs to exist between the various international codes and standards activities.
- Yes. *(3 responses)*
- No.

12. Are there any gaps in the portfolio for this technology area?

- FCTO is doing a great job in this area to address issues based on feedback from stakeholders and balancing the funding respectively.
- The main gap observed is the stack level. National laboratories and universities are working only on single cells to develop new materials or new models (even system models). Stack and systems are only developed and tested by industry. The impact of the laboratory developments might be more efficient and faster if the researchers could validate their findings in an early stage at the stack level.
- The project portfolio is an issue because most projects for the important technical focus area are ending in this fiscal year. It should be fixed. The upcoming FOA is an opportunity to fix it. The next funding opportunity should consider an updated technology focus and priority.
- The key gap is understanding how state-of-the-art materials (e.g., catalysts and ionomers) can be integrated into a robust state-of-the-art MEA. There seems to a lack of leadership in trying to move technology from Technology Readiness Level (TRL) 3 to TRLs 4 and 5.
- No. Value can be added by future multipurpose validations/demonstrations.
- There are no high-level gaps.

13. Are there topics that are not being adequately addressed?

- System control strategies are not addressed at all; the research community (also outside the current fuel cell community) has a lot of competence in that field.
- Topics for projects to be funded can follow the same breakdown as fuel cell cost breakdown; for example, more funded projects for higher-cost component (catalyst).
- Fuel and air contamination is key to long-life fuel cell operation. Thus, some research should focus on contamination-tolerant catalysts.
- The development of markets for already proved technologies and a supply base is not being adequately addressed.
- No.

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- Under this portfolio, most of the areas are already covered.
- The sub-program should continue building on the high-temperature membrane working group achievements. It should also support multi-use, crosscutting technology strategies to produce a greater value proposition. In addition, it should validate advanced, higher-reliability, lower-capital and operating cost technology options that simplify hydrogen refueling and reduce parasitic power for higher-pressure hydrogen.
- There are more opportunities to develop novel analytical methodology areas to support focused R&D. The ORNL microscopic analysis is a good example.
- Yes, FCTO needs to lay out a clear roadmap on how it is planning to improve the stack robustness and simplify system design.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- Because the stack is a key element of the fuel cell system, stack development involving parallel MEA and bipolar plate development should be promoted. At the least, stack testing by national laboratories should be performed to speed up the validation of new solutions. Durability and cost barriers may be addressed by non-material constraints such as better system control strategies. Even if it is very sensitive for industry, academic researchers may also propose breakthroughs.
- There is little-to-no established supply base for key components in the United States. The United States currently does not have any world-leading supply base for catalysts, ionomers, gas diffusion layers, or bipolar plates. These four components account for more than 80% of the projected long-term stack cost. DOE and other U.S. government organizations need to be paying attention to this lack of competency. Perhaps FCTO can leverage its size and leadership to get the other organizations to open up their reviews (e.g., post proceedings online), or perhaps it can fund a project to monitor and convert these proceedings into English for a U.S. audience.
- The sub-program should pursue multipurpose technology development and validation. It should consider an R&D model similar to the one used by the Solid State Energy Conversion Alliance (SECA) program's DOE- industrial team.
- There should be more feedback from stakeholders and more focus on MEA results. MEA results should be required in the second year of the project.
- The upcoming FOA is key to fixing the project portfolio.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- The sub-program should continue using the same strategy—workshops are a great way to get guidance from stakeholders!
- Webinars have been great so far to share the results, but they should be advertised well, not only on the website. Maybe an email subscription would work better. Publications and patents filed under funded projects can be included as accomplishments in every presentation.
- The projects are annually reviewed by the U.S. DRIVE partnership's Fuel Cell Technical Team, which provides recommendations entirely focused on automotive applications. Including industry with a focus on stationary fuel cell applications would yield a balanced view.

Manufacturing R&D Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- Yes, the sub-program's needs, intent, and strategy to generate a solution were well developed with industry collaboration and thoughtful consideration.
- Yes, it was covered in sufficient detail.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- The appropriate balance between mid- and short-term development is difficult to identify at this time due to some potential overlap between evolving research and vetted products that are ready for increased efforts for lower-cost manufacturing and production. Nonetheless, the strategy to move from research to production with scale up of laboratory processes, quality control (QC) diagnostics, and quantification of defects is appropriate.
- It would be helpful to classify initiatives into immediate through long-term categories.

3. Were important issues and challenges identified?

- Yes, but determining the defining line on what products and components are ready for increased efforts for lower-cost manufacturing and production may be difficult. The approach to collaborate with industry is appropriate, but the task to vet which efforts are ready for advanced manufacturing and production should not be underestimated.
- Yes.

4. Are plans identified for addressing issues and challenges?

- Yes, identification of cost drivers, elimination of steps, use of process control tools, increased automation, reduction of scrap, and increase of yields are all appropriate to reduce production costs. Controls including scale up of laboratory processes, QC diagnostics, and quantification of defects are appropriate to maintain proper attention on the technologies and components that are ripe for commercialization and manufacturing.
- Yes.

5. Was progress clearly benchmarked against the previous year?

- This appears to be a relatively new area, but in the previous year collaboration with industry and open workshops provided a reasonable benchmark of industry concerns from which to move forward.
- The comparison with last year was a bit vague.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- Yes, they are very much addressing the problems and barriers with DOE's Clean Energy Manufacturing Initiative to increase production and manufacturing with reduced costs, consistent with the overall U.S. strategy.
- The projects discussed are consistent with FCTO goals. Rather than focusing on only one aspect, several components are addressed.

7. Does the sub-programappear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes. While it is just beginning, the sub-program appears to be well justified by industry, thoughtful to avoid waste of resources, well managed in an open and transparent manner, and consistent with U.S. policy.

- More emphasis could be placed on real-world manufacturing and technical component requirements.

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- The strategy to identify barriers and solutions is a key strength. One weakness may be the inability to project the potential for evolution of technology through advanced research, rendering the product unready for large-scale manufacturing. Nonetheless, controls have been taken to largely avoid this weakness with QC diagnostics and quantification of defects.
- One project detects online component defects, but required detection limits are not clearly quantified.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes, the approach appears well justified as an effort to move R&D to a commercial level. This may be one of the first innovative attempts to standardize the manufacturing processes for the hydrogen fuel cell industry to promote appropriate commercialization as an effort to increase competitive production.
- Yes, they represent novel and innovative ways, and some have been down-selected.

10. Has the sub-program engaged appropriate partners?

- Key industry partners were approached and engaged at the previous workshop. Perhaps an annual commercialization workshop would be appropriate to help gauge progress with the technology targets. This approach might be considered on a regional basis to directly address industry clusters.
- Yes.

11. Is the sub-program collaborating with them effectively?

- Yes, but continued collaboration with regions and clusters would be helpful. Continued collaboration with other sub-programs (Systems Analysis; Safety, Codes and Standards; Technology Validation; and Market Transformation) would also be of long-term value.
- Yes.

12. Are there any gaps in the portfolio for this technology area?

- The sub-program should make sure technologies are consistent with fuel cell requirements.
- Continued collaboration with industry and stakeholders would be helpful.

13. Are there topics that are not being adequately addressed?

- Stationary fuel cells with other MEA stack configurations would be appropriate next steps.
- No.

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- Other MEA technologies, continued collaboration, and QC processes should be considered. Also, tracking of performance metrics would be helpful to justify sub-program funding.
- No.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- There has been much work on increasing manufacturing efficiency and competitive production within the manufacturing industry. Much of this work has been adopted by large OEMs, including in the automotive industry. Coordination with these non-fuel-cell industries to identify generic manufacturing, advanced production models, "lessons learned," and efficiency solutions would be of value.

- No.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- Coordination with the U.S. Department of Commerce and the U.S. Small Business Administration may be helpful and of value to better understand their efforts for increased manufacturing efficiency and competitive production.
- The sub-program should make sure defect detection can see defects of importance to component performance and durability.

Technology Validation Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- The validation strategy is quite straightforward and is described appropriately. No further discussion is required.
- Yes. (*2 responses*)

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- For the technology validation topic, all of the areas are going to be near term or at most mid term. It seems to be a waste of effort to validate a very immature, long-term R&D concept. Those long-term projects may need some form of investigation, but it would not be under the Technology Validation sub-program. The areas validated (e.g., buses, fuel cell electric vehicles [FCEVs], compressors, and stations) are all appropriate.
- Not applicable; this is technology validation, which, by definition is evaluating existing technology in the field, so these are all near-term projects.
- Yes.

3. Were important issues and challenges identified?

- Yes, they were identified, but not explicitly. In the future, the presentation could explicitly list the top challenges in each validation area.
- Yes.

4. Are plans identified for addressing issues and challenges?

- In some cases they are identified. In others, the goal seems to be merely testing the systems to show that they meet goals. That is a worthy effort, but it would be better to more clearly state exactly what key parameter is being tested (and why).
- Yes. (*2 responses*)

5. Was progress clearly benchmarked against the previous year?

- No, there was no clear benchmarking against the previous year.
- Yes.
- No.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- In a sense they are addressing the problems and barriers, because providing operational data helps to validate the technology and pave the way for increased market acceptance.
- Yes. (*2 responses*)

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes; it seems to be well organized and well run.
- Yes. (*2 responses*)

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- The projects examine a breadth of technology validation approaches. There are (relatively) large-scale automotive demonstrations involving the major FCEV manufacturers, specific device/system developments such as high-pressure tube trailers, testing of small-scale equipment such as compressors, and a consortium approach to catch all intangible aspects of the “fueling station experience” (H2FIRST).
- The project to test the PDC Machines compressor (TV-019) is a key strength because compressors are the weak link in hydrogen fueling stations. One weakness might be the lack of a similar project to test high-pressure liquid hydrogen pumps, because trucked-in liquid hydrogen is the primary method of delivering hydrogen to MHE sites, and trucked-in liquid hydrogen will become more prevalent for FCEV fueling stations, too.
- The newer projects—those closer to their start—did not seem like they had much to report, and maybe they should have been kept out of the review.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- H2FIRST is the most novel and innovative.
- Some projects did not represent novel and/or innovative ways.

10. Has the sub-program engaged appropriate partners?

- Some did not engage appropriate partners; some listed partners because they thought they should.
- Yes. (*2 responses*)

11. Is the sub-program collaborating with them effectively?

- It appears to be.
- It is unclear.

12. Are there any gaps in the portfolio for this technology area?

- It is unclear.
- Testing and evaluation of liquid hydrogen high-pressure pumps is a gap.
- No.

13. Are there topics that are not being adequately addressed?

- No.

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- No.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- Presenters from the same organizations should benchmark against each other; for example, at least one NREL presentation was phenomenal, other NREL presentations, with similar aims, were not.
- No.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- The sub-program should coach the laboratories as the difference between the quality of the presentations was great.

Safety, Codes and Standards Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- The sub-program's objectives and strategy are well defined. There is a very strong focus on key areas around safety, codes, and standards (SCS), which will enable the early deployment of hydrogen and fuel cell technologies.
- The sub-program's scope and activities, including its strategy, are definitely adequately covered. The presentation could have benefited from some more information on the necessary (and intended) interaction with H_2USA, both in content and in process.
- Yes. The sub-program has done good work considering its time constraints and breadth.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- There are appropriate topics, given the working area and challenges for the commercialization of fuel cell technology.
- There is good balance in the overall portfolio of the sub-program.
- Emphasis is currently concentrated on tackling outstanding issues related to the deployment of hydrogen refueling systems. This is understandable, but a longer-term view on other applications did not emerge from the presentation.

3. Were important issues and challenges identified?

- Yes, the main issues are clearly stated and the sub-program is really focused on addressing these.
- Yes. (*2 responses*)

4. Are plans identified for addressing issues and challenges?

- The plan that the sub-program has established looks very well thought out and well managed. The strong integration with the domestic and international communities is the key for the success of this sub-program.
- Such plans are mostly provided in the presentations of the individual projects covered by the Safety, Codes and Standards sub-program.
- While a comprehensive plan was put forth, it would have been good to see the foreseen challenges and the path to avoid or address them.

5. Was progress clearly benchmarked against the previous year?

- New accomplishments and their importance were clearly defined.
- This was not directly apparent from the presentation.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- Absolutely, yes! The projects in this group are generally important for the adoption of the technology related to meeting existing codes and standards or appropriately modifying codes and standards.
- Yes they are, especially the recent developments in the materials compatibility area, the in-line analyzer work at LANL, the Quantiative Risk Assessment toolkit, and the strong focus on safety education and training.
- Without any doubt, they are addressing the problems and barriers.

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- The sub-program is well managed and clearly focused on the main objectives and challenges.

- Generally, yes. The budget should be reevaluated. While the overall budget is fine, if not on the small side, the $625,000 dedicated to the hydrogen safety panel relative to the other topics is questionable.
- Yes.

8. What are the key strengths and weaknesses of the projects in this sub-program area? Do any of the projects stand out on either end of the spectrum?

- One of the key strengths of the sub-program is the very close collaboration with the key domestic and international stakeholders and organizations.
- Each project is addressing a critical need for fuel cell technology adoption. The only project that has questionable execution and adoption is the “hands-on safety training.”
- This reviewer has reviewed six projects in the SCS sub-program. Among these, the weakest score has been obtained by SCS-015, and the highest by SCS-007. All reviewed projects (not the total number of the SCS sub-program) hence fall in a rather narrow score band, indicating the generally relevant and high-quality work in the projects.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Very innovative approaches are taken, such as the work at LANL on fuel quality analysis and the hydrogen safety sensors projects.
- In this year’s review, the most novel/innovative items are the integrated HyRAM toolbox and the progress in the in-line fuel quality analyzer. Other projects that this reviewer has reviewed mostly build further on their already well-established past achievements.
- Yes.

10. Has the sub-program engaged appropriate partners?

- This is the key and the strongest aspect of this sub-program.
- For the SCS sub-program, U.S. standard development organizations and code development organizations are logical and indeed necessary partners. However, with the global deployment of hydrogen and fuel cell technologies, even more active involvement of the sub-program in international standardization and regulatory activities would be useful. It is not clear how findings from the Technology Validation sub-program are fed into (updating) the SCS sub-program.
- In general, all relevant working partners were identified.

11. Is the sub-program collaborating with them effectively?

- Collaboration is very effective and very well managed.
- Yes. (*2 responses*)

12. Are there any gaps in the portfolio for this technology area?

- Two identifiable gaps across most of the technology areas are outreach and adoption. Many of the tools and programs have high value but could be hidden within industry.
- Given the current focus on enabling the deployment of hydrogen refueling stations, SCS for stationary fuel cell applications and non-road transport could fall behind.
- No.

13. Are there topics that are not being adequately addressed?

- The topics included in the sub-program are adequately addressed.
- No.

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- There are no other technical areas to consider for funding, but periodic international workshops to take stock of global SCS activities and share SCS-relevant experiences should possibly be funded.
- The proposed upcoming activities should provide additional value to the overall success of this sub-program.
- No.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- While difficult, deepening the working relationships with the collaboration partners is recommended. This is a time issue with all parties.
- No.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- A periodic international workshop to take stock of global SCS activities (United States, Canada, Japan, China, Korea, European Union [EU]) and to share SCS-relevant experiences from ongoing and imminent deployments of hydrogen and fuel cell technologies can definitely add value. Such an activity has recently been launched for tackling issues related to the implementation of hydrogen infrastructure (Berlin and Los Angeles workshops sponsored by DOE, Germany's National Organization for Hydrogen , Japan Automobile Research Institute, Scandinavia, and the EU) with the active involvement of appropriate industries. A similar effort is ongoing on identifying R&D priorities for hydrogen safety within Hysafe, with active involvement from DOE, the European Commission – Joint Research Centre, and relevant industries. In view of the absence of industrial partners, the International Partnership for Hydrogen and Fuel Cells in the Economy's Regulations, Codes and Standards Working Group does not seem to be the appropriate forum for housing such a workshop.
- The sub-program should ensure pathways exist to market any products of this sub-program, whether they are physical (hydrogen quality) or related to training (hydrogen safety).
- The sub-program should keep the close collaboration with the international community because this is a key area, especially with the upcoming international developments in the hydrogen and fuel cells space.

Market Transformation Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- Yes. The objective of the Market Transformation sub-program was evident and focused.
- Yes, the general area of the Market Transformation sub-program was adequately addressed, with a presentation on demonstration work for stationary, mobile, refueling, and hybrid applications.
- The sub-program and strategy were adequately and thoroughly covered.
- The sub-program was covered during the overview presentation. The slides could be improved by providing a framework or context that explains why the specific niche markets were chosen (and others were not). Otherwise, the slides give the impression that the applications were chosen opportunistically as opposed to being driven by a DOE market transformation strategy that focused on "low-hanging fruit" to make the most efficient use of taxpayer money.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes, the work appears to be tied to near-term demonstration activities; however, this approach is appropriately balanced because it is coordinated with other DOE initiatives for systems analysis, codes and standards, and technology validation. With such coordination, there is an appropriate balance for near-, mid-and long-term development.
- With the limited funding, concentration on near-term opportunities that can be leveraged for longer-term benefits seems appropriate.
- The balance does not come out from the presentation, but perhaps it is difficult to show without an overall market transformation strategy that clearly maps niche markets versus their maturity/attractiveness.
- Yes.

3. Were important issues and challenges identified?

- Yes, important issues and challenges were clearly identified for the given target applications that are being funded.
- Yes, the sub-program manager painted a complete picture.
- Yes, they were identified—principally, as technology and market adoption due to price.
- Cost and budget were identified as critical issues; however, market drivers, technology validation, and compliance with codes and standards were also addressed.

4. Are plans identified for addressing issues and challenges?

- Yes, coordination with private companies, state and federal government, and other stakeholders was presented. This coordination was shown to be critical in identifying appropriate projects for demonstration and validation with the best probability for long-term development.
- Yes, for each issue/challenge, an action plan is identified on the individual project level.
- Yes. (*2 responses*)

5. Was progress clearly benchmarked against the previous year?

- Yes, progress was clearly benchmarked against the previous year, but once again it was mainly on the individual project level.
- Yes, past progress and future work were addressed and benchmarked, but future plans may be constrained by budgets and funding.
- This is unclear. Although good information was presented about ongoing initiatives and projects, one could not determine how the "needle" had been moved from 2013 to the 2014 review.
- Yes.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- Yes, the presentation addressed demonstration work for stationary, mobile, refueling, and hybrid applications; all represented important breakthroughs in market transformation.
- Once again, given its very limited resources, FCTO is doing a very credible job of trying to advance the technology through partnering and leveraging investments from multiple parties.
- Each individual project is contributing. A gap analysis is missing that might identify a key application market that is not being currently funded, and that should be solicited. The upcoming request for information (RFI) on fuel cell range extenders is a step in the right direction, but it seems like this has been chosen opportunistically.
- Yes.

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes, the sub-program was well focused and justified on a sub-program basis and a project-by-project basis. Management was excellent, and the project results will be helpful for increased market acceptance in opportunistic market areas.
- Overall, yes, the sub-program appears to be addressing FCTO's needs; it would be useful to look at market transformation more strategically to see if there are niche markets that should be more aggressively pursued by DOE.
- Yes, this is a complete effort.
- Yes.

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- The breadth of projects being funded is a strength of this sub-program. The projects that appear to be the best use of taxpayer funding are those that are on track to show results, such as: (a) small commercial building fuel cell CHP, and (b) ground system equipment.
- The effort to focus on Hawaii presents the best opportunity to create a situation that helps move the market forward on several of these project fronts. The sub-program should put a "full court press" on getting this as the springboard for the technology.
- The sub-program is consistently high performing.
- The project demonstration with performance tracking for key emerging technologies in emerging markets was clearly a strength. Weaknesses are related to the decision-making process of which projects get selected for demonstration and which ones are not selected, consistent with budget limitations. Projects that are selected clearly stand out as superior, but there was not presentation of information on rejected projects and the reasons why such projects were not selected for demonstration. Such a vetting process may exist but may be tedious to present, and thus it was not presented. Perhaps some type of a decision matrix with criteria and topics could identify the general decision-making process with accepted and rejected topics.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes, all the projects appeared to be well selected as innovative to address goals, market drivers, and removal of barriers.
- The novel aspect of all these projects is that they are very business-case focused; this is unusual for government-funded projects and is to be applauded.
- These are tried and true pathways to address and solve some of the challenges—they are not necessarily novel or innovative.
- Yes.

10. Has the sub-program engaged appropriate partners?

- Yes, the Market Transformation sub-program's coordination with agencies is commendable and allows the sub-program and the other agencies to leverage funding to achieve mutual and individual goals.
- The list of participants and partners is very robust; it is perhaps one of the strengths of the program.
- Yes; for example, having BMW in the landfill-to-gas project is an excellent idea.
- Yes; such collaboration was well justified, but there may be opportunities for increased collaboration and identification of additional partners. Such partnerships may be difficult with competitive private companies, but they may help to increase awareness among additional stakeholders and market participants.

11. Is the sub-program collaborating with them effectively?

- Yes. There is good communication and sharing, and collaboration is evident.
- Yes, this seems to be the case.
- Yes. Collaboration was well justified and effective, but there may be opportunities for increased collaboration and identification of additional partners. Such partnerships may be awkward and difficult with competitive private companies and a larger number of project participants, but such expanded partnerships may help to increase awareness among additional stakeholders and market participants.
- Yes.

12. Are there any gaps in the portfolio for this technology area?

- There are no significant gaps in the sub-program, but some areas that could be addressed include the following:
  - Continue collaboration with other DOE teams (e.g., Systems Analysis; Safety, Codes and Standards; and Technology Validation).
  - Continue efforts to increase partnership building for project demonstration.
  - Provide a summary presentation and justification of all demonstration projects selected and rejected.
- The only thing that is crosscutting and needs to be attacked aggressively is the cost of the technology and helping to bring that down while improving the value proposition in the market.
- The upcoming RFI on fuel cell range extenders fills in an existing gap.
- Yes, there are gaps that additional funding could help.

13. Are there topics that are not being adequately addressed?

- All topics appear to be relevant and well justified. Another topic that appears ripe for demonstration is fuel cell microgrid applications. This topic is receiving national and international attention with the need for standardized, ultra-clean, quiet, and efficient stationary generation resources for use with mission-critical end users, and for nodal grid reliability.
- Lowering costs will bring stronger market adoption. It is unclear what exactly could address that, but that might be a topic for consideration.
- The upcoming RFI on fuel cell range extenders addresses a topic that was not covered.
- No.

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- The Market Transformation sub-program is already leveraging other agencies.
- One particular area that may be ripe for consideration is the diversification of zero emission vehicle (ZEV) refueling infrastructure. This topic is receiving attention as a result of potential overreliance on electric grid resources for plug-in battery electric vehicles (EVs). This overreliance is tied to local grid interconnection with home and commercial recharging stations that may threaten grid capacity, and it is further complicated with the tie between centralized electricity systems and the transportation sector. Solutions include the use

of islandable stationary fuel cell generation facilities at EV recharging stations and diversification of the ZEV refueling market with increased efforts to deploy hydrogen refueling at mission-critical fleets.

- A gap analysis is missing that might identify a key application market that is not being currently funded, and that should be solicited.
- Cost-cutting or other measures to improve the value proposition should be considered.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- Recommendations include the following: (a) continue collaboration with other DOE sub-programs (Systems Analysis; Safety, Codes and Standards; and Technology Validation), (b) increase partnership-building for project demonstration, (c) summarize reasons to select and reject all demonstration projects considered, and (d) expand the scope to consider fuel cells and hydrogen refueling as a reliability asset on the electric grid.
- No. (*2 responses*)

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- The sub-program should (a) investigate fuel cells for microgrid applications at mission-critical facilities, (b) investigate stationary fuel cells at EV recharging stations as a reliability asset on the electric grid, and (c) investigate hydrogen refueling for FCEVs at mission-critical fleets as a reliability asset on the electric grid.
- A gap analysis is missing that might identify a key application market that is not being currently funded, and that should be solicited.
- Increased funding might help to identify a stronger role in the value proposition.
- No.

Systems Analysis Sub-Program Comments

1. Was the sub-program, including overall strategy, adequately covered?

- The sub-program features a very broad and diverse analysis portfolio that really focuses on the main goal of supporting the infrastructure development. It is a very-well-managed sub-program. It features an excellent combination of industry, academia, and laboratory work.
- Yes, the strategy was adequately covered. Fred Joseck presented a comprehensive description of how the strategy will help address the challenges and meet the objectives of the Systems Analysis sub-program area. Models and tools were well described and seemed adequate to address some of the barriers outlined in the FCTO Multi-Year Research, Development, and Demonstration Plan.
- The sub-program was described well and all areas were covered.
- Yes. All major areas were mentioned.
- Yes.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes; however, greater emphasis is now needed on issues related to the early deployment of hydrogen fuel cell vehicles and infrastructure. Greater emphasis is also needed on the dynamics and costs of the transition. Great work has been done on the current situation and on the long-run, post-transition costs and benefits; however, the transition itself is critical and needs to be better understood. Good work is being done on non-automotive fuel cell applications and on hydrogen delivery and station economics. Life cycle analysis and market penetration models are generally addressing long-run conditions. The workshop on infrastructure financing was a step in the right direction, but much more is needed in this area given that Hyundai is leasing vehicles now and Toyota and Honda will soon follow.
- The Systems Analysis sub-program seems to be tackling near-, mid-, and long-term challenges. The models and tools, as well as the financial analyses, are helping stakeholders understand the current status of the technology and its near-term challenges. Further, the investor workshop is creating awareness among the investing community; this is paving the way for the future while keeping stakeholders engaged now. For the mid and long terms, the projects sponsored by this sub-program are helping to do the following:
    - Identify potential investment gaps.
    - Analyze cost implications for different refueling pressures from the points of view of the consumer and the station owner.
    - Understand the environmental implications of different transportation modes, including greenhouse gas emissions and water.
    - Model the future impact on employment.
    - Understand which components need additional R&D to get the technology to compete in future markets.
- Yes, but as presented, the difference between near term and long term was not always clearly marked. Thus, a practice of explicitly noting the term might benefit the sub-program balance and illuminate asymmetries.
- The sub-program has a well-balanced portfolio in terms of the different time frames.
- The analysis portfolio is broad based and attempts to cover the most pressing analysis needs.

3. Were important issues and challenges identified?

- Yes, slide 3 describes in detail the challenges being tackled by the sub-program: future market behavior; data availability, accuracy, and consistency; and coordination of analytical capability.
- The main challenges were clearly stated by the sub-program, and the sub-program is clearly focused on addressing these issues.
- Yes, with one exception that was noted by the sub-program manager. That is, models and planning tools are needed that incorporate the interdependence of hydrogen supply and demand. There is a need to have transferable planning models that municipal, state, and federal decision makers can use in collaboration with industry to plan the deployment of early hydrogen infrastructure and test policies for supporting that deployment. This applies not only to the first few stations (such as the STREETS model), but also to the

next 100 and so and beyond. These models must represent the interdependence of vehicle demand and infrastructure supply. They must deal with transition costs such as majority risk aversion, value of fuel availability, etc. These are very difficult topics and no one has yet produced a model or models that adequately represent all the issues. Indeed, basic research is needed to understand many of the issues. Given the magnitude and difficulty of the challenge, the Systems Analysis sub-program budget is probably not adequate at the present time.

- The challenge of describing future markets stands out as a major challenge.
- Generally, yes, they were identified.

4. Are plans identified for addressing issues and challenges?

- The various methodologies employed by the overall Systems Analysis portfolio provide great value to DOE's efforts in achieving its technical targets.
- Yes, with one exception: models and planning tools are needed that incorporate the interdependence of hydrogen supply and demand. Outstanding work has been done on life cycle analysis, hydrogen station financial analysis, employment impacts, the economics of hydrogen delivery pressure, trigeneration, interaction with the grid, and more. Data development and validation efforts are very strong. A powerful suite of models and analytical tools has been developed. Unfortunately, more is needed to address the challenges of the transition. A large-scale energy transition to address social concerns (e.g., climate change, energy security, and energy sustainability) is a new problem for DOE and public policy in general. A well-focused and well-funded effort is needed to adequately address the research needs and model development to support such a transition.
- Yes, the projects described tackle all the challenges described in slide 3.

5. Was progress clearly benchmarked against the previous year?

- The cash flow and financing work is a valuable addition to the sub-program.
- Yes, this was clearly shown in a slide.
- No, the presenter did not mention how much progress was made compared to last year. However, it seemed that many of the projects described were initiated within the last year.
- Progress was generally not clearly benchmarked.

6. Are the projects in this technology area addressing the broad problems and barriers that the Fuel Cell Technologies Office (FCTO) is trying to solve?

- Yes, the analyses supported by this sub-program are painting a very clear picture of the pathways that can be taken to help accelerate the introduction of hydrogen FCEVs and their supporting refueling infrastructure.
- The analytical work in this sub-program definitely provides a lot of value in addressing the main challenges of FCTO.
- Analysis efforts are well suited to addressing the pressing issues. The cradle-to-grave study has been a valuable addition to the Systems Analysis portfolio.
- Yes, with one exception: models and planning tools are needed that incorporate the interdependence of hydrogen supply and demand.
- Yes.

7. Does the sub-program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes, none of the projects seem to overlap, the data is accurate, models are validated, the right stakeholders have been engaged, and future activities are in alignment with FCTO's goals.
- This is a very focused and excellently managed sub-program.
- The sub-program appears comprehensive. A large number of models have been developed for system analysis and are available for future study.

- The analysis efforts are focused on key areas and the available resources seem to be allocated appropriately.
- Yes, with one exception: models and planning tools are needed that incorporate the interdependence of hydrogen supply and demand.

8. What are the key strengths and weaknesses of the projects in this sub-program? Do any of the projects stand out on either end of the spectrum?

- The projects are generally rigorous and state-of-the-art. For example, the hydrogen fueling station operation and cost analyses, the life cycle impact analyses, and the grid integration analysis stand out as especially strong examples. More needs to be done on transition analysis. The *Transitions to Alternative Vehicles and Fuel*[1]*s* study is a good resource for illustrating the kind of knowledge that needs to be developed and the kind of modeling that will be needed. However, even that study does not go far enough with respect to the need for planning tools at a high level of spatial resolution.
- Key strengths include the following: (a) the strong support from national laboratories and U.S. DRIVE partners, (b) the PIs have strong analytical capability and experience in advanced fuels and vehicle technologies, and (c) the portfolio of activities is diverse and targeted. Weaknesses are that (a) the industry data is not always available and (b) the budget will remain flat for 2015.
- The sub-program is doing a great job of modeling the technoeconomics associated with FCEVs. There may be a need to involve stakeholders more to look at financial and investment strategies.
- The main strengths are the very strong collaboration with some of the top experts in these areas and that the analysis work is based on very-well-established modeling tools. Weaknesses include the limited resources and budget, especially for a key area such as this one.
- Some of the assumptions used in the individual analyses are optimistic. For example, DOE Program Record #14003[2] is shown as documenting 7–9 cents/kW for stationary fuel cell systems, yet it is predicated on $1,500/kW installed capital cost. Thus, the levelized cost of energy may be true, but the capital cost is not achievable in the near term. The analysis is useful, but critical assumptions (and the time frame in which performance levels are likely to be achieved) must be (repeatedly) stated.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Some of the projects are innovative, while others have not been around for a long time. The latter is not necessarily a bad thing; useful models such as the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET) model and the Hydrogen Analysis (H2A) model need to be continuously updated and refreshed.
- The projects reflect many innovative ideas for finding new markets or new ways to use hydrogen and fuel cells. However, the economics remain difficult.
- Yes.

10. Has the sub-program engaged appropriate partners?

- Yes, there is strong stakeholder support on the technical side; the models are built by PIs with very strong technical skills (e.g., ANL and NREL) and vetted by the appropriate industry partners. Further, the AMR provides a great venue for other stakeholders to comment and help direct the sub-program.
- The very strong and broad collaboration is probably one of the key aspects for the high success and value of this sub-program.
- The collaboration with industry is strong. The appropriate partners are involved. The collaboration among national laboratories is strong. The sponsorship of university research is not so strong.

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[1] National Research Council. *Transitions to Alternative Vehicles and Fuels*. Washington, DC: The National Academies Press, 2013.

[2] U.S. Department of Energy, Offices of Solar Energy Technologies and Fuel CellTechnologies. Program Record 14003: Levelized Costs of Electricity from CHP and PV. http://www.hydrogen.energy.gov/pdfs/14003_lcoe_from_chp_and_pv.pdf. March 14, 2014.

- Industrial secrecy and business concerns often make it difficult to vet models and supporting data, but the sub-program is making a good effort to work with industry and other stakeholders to develop and validate models.
- Yes.

11. Is the sub-program collaborating with them effectively?

- Yes—through U.S. DRIVE meetings and presentations, informal discussions with industry and government laboratories, and the AMR, the sub-program has received invaluable feedback and information.
- The collaboration among the laboratories is good. The sub-program needs to continue to work with H_2USA and international groups. More direct involvement with universities would be good, but the budget would probably need to be increased.
- Yes.

12. Are there any gaps in the portfolio for this technology area?

- Many of the models assume a certain level of market penetration to determine cost. It would be good to see those models focus on current and near-term (i.e., 5–10 years into the future) scenarios as well to understand how the transition may happen.
- Demand modeling is perhaps the greatest gap and is one area where the required expertise may not reside within DOE or national laboratories.
- Yes—transition modeling, research, and planning tools.
- Limited resources and budget are gaps for the sub-program.

13. Are there topics that are not being adequately addressed?

- The previous Secretary of Energy overemphasized nonautomotive fuel cells and hydrogen use relative to automotive use. That imbalance has not yet been fully corrected. At the same time, the challenges of achieving a transition to hydrogen FCEVs are great. There are many important aspects of market behavior, for example, that are poorly understood at present, but that are very important to the transition. Although great progress has been made in modeling, there is still no model that adequately represents the interdependence of infrastructure supply and vehicle demand. This is an important area that needs to be addressed.
- The following topics are not adequately being addressed: (a) market transition; (b) integration of natural gas systems with renewable energy technologies; (c) impact of international introduction of advanced vehicle technologies on the U.S. market; (d) large-scale electrolyzers >1 MW; and (e) comparing the cost, emissions, and performance of tri-generation systems with other advanced technologies (e.g., cogeneration and integrated gasification combined cycle [IGCC]).

14. Are there other areas that this sub-program should consider funding to meet overall programmatic goals?

- The current portfolio is very complete.
- Regarding market transition, it is unclear how current technology needs will evolve in terms of cost, efficiency, and rate of deployment to achieve the penetration levels in H2A. Regarding the integration of natural gas systems with renewable energy technologies, natural gas is a low-cost, low-carbon fuel that can be combined with renewable energy technologies, including wind, photovoltaics (PV), fuel cells, batteries, and others, to provide stable power and heat in the most environmental manner. In terms of the impact of the international introduction of advanced vehicle technologies, it is unclear how the introduction of hydrogen production, delivery, and dispensing infrastructure abroad will impact the market in the United States. Regarding large-scale electrolyzers (>1 MW), no data is available, even though the central-production scenarios of DOE models assume that the technology will be available. The sub-program should compare the cost, emissions, and performance of tri-generation systems with other advanced technologies (e.g., cogeneration and IGCC). The sub-program should also pursue financing options for fueling infrastructure, such as leasing, purchasing, and hydrogen production and delivery agreements.

- In order to predict consumer behavior, the sub-program should seek to partner with OEMs (possibly through third parties to make OEM data anonymous).
- Yes.

15. Can you recommend new ways to approach the barriers addressed by this sub-program?

- An interesting approach would be to help developing countries that are in the process of developing their vehicle and fuel infrastructure. Because greenhouse gas emissions are global and affect everyone equally, a reduction in emissions abroad has the same effect as a reduction in emissions domestically. Technology deployment abroad could be cheaper and help prove technologies. Further, domestic production and exports of alternative vehicles, components, and fuels could be boosted. It would be interesting to model and analyze this approach. There should be more engagement with policy makers to explain the results of the models. In addition, this reviewer recommends projects to explore the following issues:
    - The dynamics of the market transition, and how current technology will evolve in terms of cost, efficiency, and rate of deployment to achieve the penetration levels in H2A.
    - The integration of natural gas systems with renewable energy technologies.
    - The impact of the international introduction of advanced vehicle technologies and hydrogen production, delivery, and dispensing infrastructure abroad will impact the market in the United States.
    - Data gathering onlarge-scale electrolyzers (>1 MW), which the central-production scenarios of DOE's models assume will be available.
    - Comparison of the cost, emissions, and performance of tri-generation systems with other advanced technologies (e.g., cogen and IGCC).
    - Alternative financing options for fueling infrastructure, such as leasing, purchasing, and hydrogen production and delivery agreements.
- It is not likely that the markets for fuel cells and hydrogen could develop without public policy support. Policies are considered in the Systems Analysis sub-program, but there could be a stronger connection with DOE policy and a stronger emphasis on modeling and analyzing how policies affect business models and what policies are needed for market development.

16. Are there any other suggestions to improve the effectiveness of this sub-program?

- The sub-program should take a step back and rethink the modeling portfolio. The Systems Analysis sub-program has an impressive array of sophisticated models (shown on the models slide). In many areas, the models work together well to address needs for analysis. However, it is not clear how they all fit together to address the modeling needs. Perhaps a workshop on this subject, supported by an initial white paper, could lead to an improved understanding of how the models relate to the range of analytical needs and possibly to more effective use of the modeling resources.
- The sub-program should involve more automotive manufacturers.

Research and Development Project Evaluation Form

This evaluation form was used for the following Hydrogen and Fuel Cells Program sub-program panels: Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Safety, Codes and Standards; and Systems Analysis.

Evaluation Criteria: U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program Annual Merit Review

Please provide specific, concise comments to support your evaluation. It is important that you write in full sentences and clearly convey your meaning to prevent incorrect interpretation.

1. Approach

To performing the work – the degree to which barriers are addressed, the project is well-designed, feasible, and integrated with other efforts. (Weight = 20%)

4.0 - Outstanding. Sharply focused on critical barriers; difficult to improve significantly.
3.5 - Excellent. Effective; contributes to overcoming most barriers.
3.0 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2.5 - Satisfactory. Has some weaknesses; contributes to overcoming some barriers.
2.0 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1.5 - Poor. Minimally responsive to project objectives; unlikely to contribute to overcoming the barriers.
1.0 - Unsatisfactory. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Approach to performing the work:

2. Accomplishments and Progress

Toward overall project and DOE goals – the degree to which progress has been made and measured against performance indicators, and the degree to which the project has demonstrated progress toward DOE goals. (Weight = 45%)

4.0 - Outstanding. Sharply focused on critical barriers; difficult to improve significantly.
3.5 - Excellent. Effective; contributes to overcoming most barriers.
3.0 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2.5 - Satisfactory. Has some weaknesses; contributes to overcoming some barriers.
2.0 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.

1.5 - Poor. Minimally responsive to project objectives; unlikely to contribute to overcoming the barriers.
1.0 - Unsatisfactory. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Accomplishments and Progress toward overall project and DOE goals:

3. Collaboration and Coordination with Other Institutions

The degree to which the project interacts with other entities and projects. (Weight = 10%)

4.0 - Outstanding. Close, appropriate collaboration with other institutions; partners are full participants and well coordinated.
3.5 - Excellent. Good collaboration; partners participate and are well coordinated.
3.0 - Good. Collaboration exists; partners are fairly well coordinated.
2.5 - Satisfactory. Some collaboration exists; coordination between partners could be significantly improved.
2.0 - Fair. A little collaboration exists; coordination between partners could be significantly improved.
1.5 - Poor. Most work is done at the sponsoring organization with little outside collaboration; little or no apparent coordination with partners.
1.0 - Unsatisfactory. No apparent coordination with partners.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Collaboration and Coordination with other institutions:

4. Relevance/Potential Impact

The degree to which the project supports and advances progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. (Weight = 15%)

4.0 - Outstanding. Project is critical to the Hydrogen and Fuel Cells Program and has potential to significantly advance progress toward DOE RD&D goals and objectives.
3.5 - Excellent. The project aligns well with the Hydrogen and Fuel Cells Program and DOE RD&D objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
3.0 - Good. Most project aspects align with the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
2.5 - Satisfactory. Project aspects align with some of the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
2.0 - Fair. Project partially supports the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
1.5 - Poor. Project has little potential impact on advancing progress toward the Hydrogen and Fuel Cells Program and DOE RD&D goals and objectives.
1.0 - Unsatisfactory. Project has little to no potential impact on advancing progress toward the Hydrogen and Fuel Cells Program and DOE RD&D goals and objectives.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Relevance/Potential Impact:

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 its goals and, when sensible, mitigating risk by providing alternate pathways. Note: if a project has ended, please leave blank. (Weight = 10%)

4.0 - Outstanding. Sharply focused on critical barriers; difficult to improve significantly.
3.5 - Excellent. Effective; contributes to overcoming most barriers.
3.0 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2.5 - Satisfactory. Has some weaknesses; contributes to overcoming some barriers.
2.0 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1.5 - Poor. Minimally responsive to project objectives; unlikely to contribute to overcoming the barriers.
1.0 - Unsatisfactory. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4.0 - Outstanding

- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Proposed Future Work:

Project Strengths:

Project Weaknesses:

Recommendations for Additions/Deletions to Project Scope:

Technology-to-Market Project Evaluation Form

This evaluation form was used for the following Hydrogen and Fuel Cells Program sub-program panels: Market Transformation and Technology Validation.

Evaluation Criteria: U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program Annual Merit Review

Please provide specific, concise comments to support your evaluation. It is important that you write in full sentences and clearly convey your meaning to prevent incorrect interpretation.

1. Relevance/Potential Impact

The degree to which the project supports and advances progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. (Weight = 15%)

4.0 - Outstanding. Project is critical to the Hydrogen and Fuel Cells Program and has potential to significantly advance progress toward DOE RD&D goals and objectives.
3.5 - Excellent. The project aligns well with the Hydrogen and Fuel Cells Program and DOE RD&D objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
3.0 - Good. Most project aspects align with the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
2.5 - Satisfactory. Project aspects align with some of the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
2.0 - Fair. Project partially supports the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
1.5 - Poor. Project has little potential impact on advancing progress toward the Hydrogen and Fuel Cells Program and DOE RD&D goals and objectives.
1.0 – Unsatisfactory. Project has little to no potential impact on advancing progress toward the Hydrogen and Fuel Cells Program and DOE RD&D goals and objectives.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Relevance/Potential Impact:

2. Strategy for Technical Validation and/or Deployment

Rate the degree to which barriers are addressed, how well the project is designed, its feasibility, and integration with other efforts. (Weight = 20%)

4.0 - Outstanding. Sharply focused on critical barriers; difficult to improve significantly.
3.5 - Excellent. Effective; contributes to overcoming most barriers.
3.0 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2.5 - Satisfactory. Has some weaknesses; contributes to overcoming some barriers.
2.0 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1.5 - Poor. Minimally responsive to project objectives; unlikely to contribute to overcoming the barriers.
1.0 - Unsatisfactory. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on the Strategy for Technology Validation and Deployment:

3. Accomplishments and Progress

Toward overall project and DOE goals – the degree to which progress has been made and measured against performance indicators, and the degree to which the project has demonstrated progress toward DOE goals. (Weight = 45%)

4.0 - Outstanding. Sharply focused on critical barriers; difficult to improve significantly.
3.5 - Excellent. Effective; contributes to overcoming most barriers.
3.0 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2.5 - Satisfactory. Has some weaknesses; contributes to overcoming some barriers.
2.0 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1.5 - Poor. Minimally responsive to project objectives; unlikely to contribute to overcoming the barriers.
1.0 - Unsatisfactory. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair

- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Accomplishments and Progress toward overall project and DOE goals:

4. Collaboration and Coordination with Other Institutions

The degree to which the project interacts with other entities and projects. (Weight = 10%)

4.0 - Outstanding. Close, appropriate collaboration with other institutions; partners are full participants and well coordinated.
3.5 - Excellent. Good collaboration; partners participate and are well coordinated.
3.0 - Good. Collaboration exists; partners are fairly well coordinated.
2.5 - Satisfactory. Some collaboration exists; coordination between partners could be significantly improved.
2.0 - Fair. A little collaboration exists; coordination between partners could be significantly improved.
1.5 - Poor. Most work is done at the sponsoring organization with little outside collaboration; little or no apparent coordination with partners.
1.0 - Unsatisfactory. No apparent coordination with partners.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Collaboration and Coordination with other institutions:

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 its goals and, when sensible, mitigating risk by providing alternate pathways. Note: if a project has ended, please leave blank. (Weight = 10%)

4.0 - Outstanding. Sharply focused on critical barriers; difficult to improve significantly.
3.5 - Excellent. Effective; contributes to overcoming most barriers.
3.0 - Good. Generally effective but could be improved; contributes to overcoming some barriers.
2.5 - Satisfactory. Has some weaknesses; contributes to overcoming some barriers.
2.0 - Fair. Has significant weaknesses; may have some impact on overcoming barriers.
1.5 - Poor. Minimally responsive to project objectives; unlikely to contribute to overcoming the barriers.
1.0 - Unsatisfactory. Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

- ☐ 4.0 - Outstanding
- ☐ 3.5 - Excellent
- ☐ 3.0 - Good
- ☐ 2.5 - Satisfactory
- ☐ 2.0 - Fair
- ☐ 1.5 - Poor
- ☐ 1.0 - Unsatisfactory

Comments on Proposed Future Work:

Project Strengths:

Project Weaknesses:

Recommendations for Additions/Deletions to Project Scope:

List of Projects Presented but Not Reviewed

Project ID	Project Title	Principal Investigator Name	Organization
PD-016	Oil-Free Centrifugal Hydrogen Compression Technology Demonstration	Hooshang Heshmat	Mohawk Innovative Technology
PD-017	Development of a Centrifugal Hydrogen Pipeline Gas Compressor	Frank Di Bella	Concepts NREC
PD-021	Development of High-Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery	Don Baldwin	Hexagon Lincoln
PD-031	Renewable Electrolysis Integrated System Development and Testing	Kevin Harrison	National Renewable Energy Laboratory
PD-036	Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures	Tasios Melis	University of California, Berkeley
PD-056	Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen	Liwei Xu	Midwest Optoelectronics, LLC
PD-091	Bio-Fueled Solid Oxide Fuel Cells	Gokhan Alptekin	TDA Research, Inc.
PD-092	Rapid, High-Pressure Liquid Hydrogen Refueling for Maximum Range and Dormancy	Aceves Salvador	Lawrence Livermore National Laboratory
PD-101	Cryogenically Flexible, Low-Permeability Hydrogen 700 Bar Delivery Hose	Jennifer Lalli	Nanosonic
PD-104	Hydrogen Generation for Refineries	Girish Srinivas	TDA Research, Inc.
ST-009	Testing and Modeling of a Cryogenic Hydrogen Storage System with a Helical Coil Electric Heater	Mei Cai	General Motors
ST-014	Hydrogen Sorbent Measurement Qualification and Characterization	Phil Parilla	National Renewable Energy Laboratory
ST-028	Design of Novel Multicomponent Metal-Hydride-Based Mixtures for Hydrogen Storage	Christopher Wolverton	Northwestern University
ST-034	Aluminum Hydride: the Organometallic Approach	Jim Wegrzyn	Brookhaven National Laboratory
ST-048	Hydrogen Storage Materials for Fuel-Cell-Powered Vehicles	Andrew Goudy	Delaware State University

Project ID	Project Title	Principal Investigator Name	Organization
ST-067	Neutron Characterization in Support of the U.S. Department of Energy Hydrogen Storage Sub-Program	Terry Udovic	National Institute for Standards and Technology
ST-095	Low-Cost, Metal-Hydride-Based Hydrogen Storage System for Forklift Applications (Phase II)	Adrian Narvaez	Hawaii Hydrogen Carriers, LLC
ST-105	Ultra-Lightweight, High-Pressure Hydrogen Fuel Tanks Reinforced with Carbon Nanotubes	Dongsheng Mao	Applied Nanotech, Inc.
ST-110	Optimizing the Cost and Performance of Composite Cylinders for Hydrogen Storage Using a Graded Construction	Andrea Haight	Composite Technology Development
ST-111	Thermomechanical Cycling of Thin-Liner High-Fiber-Fraction Cryogenic Pressure Vessels Rapidly Refueled by Liquid Hydrogen Pump to 700 bar	Salvador Aceves	Lawrence Livermore National Laboratory
ST-112	Load-Sharing Polymeric Liner for Hydrogen Storage Composite Tanks	Scott McWhorter	Savannah River National Laboratory
FC-006	Durable Catalysts for Fuel Cell Protection during Transient Conditions	Radoslav Atanasoski	The 3M Company
FC-010	The Science and Engineering of Durable Ultralow Platinum Group Metal Catalysts	Fernando Garzon	Los Alamos National Laboratory
FC-036	Dimensionally Stable High-Performance Membranes	Cortney Mittelsteadt	Giner Electrochemical Systems, LLC
FC-040	High-Temperature Membrane with Humidification-Independent Cluster Structure	Ludwig Lipp	FuelCell Energy, Inc.
FC-048	Effect of System Contaminants on Polymer Electrolyte Membrane Fuel Cell Performance and Durability	Huyen Dinh	National Renewable Energy Laboratory
FC-049	Open-Source FCPEM-Performance and Durability Model (FC-APOLLO): Consideration of Membrane Properties on Cathode Degradation	Silvia Wessel	Ballard Power Systems
FC-052	Technical Assistance to Developers	Tommy Rockward	Los Alamos National Laboratory
FC-054	Transport in Polymer Electrolyte Membrane Fuel Cells	Cortney Mittelsteadt	Giner Electrochemical Systems, LLC
FC-081	Fuel Cell Technology Status Cost and Price Status	Jennifer Kurtz	National Renewable Energy Laboratory

Project ID	Project Title	Principal Investigator Name	Organization
FC-084	WO_3 and Heteropoly-Acid-Based Systems for Durable Pt Catalysts in PEM Fuel Cell Cathodes	John Turner	National Renewable Energy Laboratory
FC-092	Investigation of Micro- and Macro-Scale Transport Processes for Improved Fuel Cell Performance	Wenbin Gu	General Motors
FC-102	New High-Performance Water Vapor Membranes to Improve Fuel Cell Balance of Plant Efficiency and Lower Costs	Earl Wagener	Tetramer Technologies, LLC
FC-105	Low-Cost PEM Fuel Cell Metal Bipolar Plates	C.H. Wang	TreadStone Technologies, Inc.
FC-111	Best Practices and Benchmark Activities for Oxygen Reduction Reaction Measurements by the Rotating Disk Electrode Technique	Shyam Kocha	National Renewable Energy Laboratory
FC-112	Resonance-Stabilized Anion Exchange Polymer Electrolytes	Yu Seung Kim	Los Alamos National Laboratory
FC-113	Non-Platinum-Group-Metal Cathode Catalysts Using Zeolitic-Imidazolate-Framework-Based Precursors with Nanonetwork Architecture	Di-Jia Liu	Argonne National Laboratory
MN-004	Manufacturing of Low-Cost, Durable Membrane Electrode Assemblies Engineered for Rapid Conditioning	Colin Busby	W.L. Gore
MN-008	Development of Advanced Manufacturing Technologies for Low-Cost Hydrogen Storage Vessels	Mark Leavitt	Quantum Fuel Systems Technologies Worldwide, Inc.
TV-001	Fuel Cell Electric Vehicle Evaluation	Jennifer Kurtz	National Renewable Energy Laboratory
TV-017	Hydrogen Station Data Collection and Analysis	Sam Sprik	National Renewable Energy Laboratory
TV-018	Hydrogen Recycling System Evaluation and Data Collection	Rhonda Staudt	H2Pump
TV-023	Newport Beach Hydrogen Station Key Performance Indicators	Michael Kashuba	California Air Resources Board
TV-027	Hydrogen Fuel Cell Electric Vehicle Commercialization: Facilitating Collaboration, Obtaining Real-World Expertise, and Developing New Analysis Tools	Bill Elrick	California Fuel Cell Partnership

Project ID	Project Title	Principal Investigator Name	Organization
SCS-020	International Partnership for Hydrogen and Fuel Cells in the Economy—Regulations, Codes, and Standards Working Group	Jay Keller	U.S. Department of Energy Consultant
MT-015	FCTAC Web Portal Tool Development	Matthew Post	National Renewable Energy Laboratory

2014 Annual Merit Review Survey Questionnaire Results

Following the 2014 U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program) Annual Merit Review (AMR), all participants were asked for feedback on the review process and meeting logistics. This appendix summarizes the results of that feedback, and is organized by type of respondent, as follows:

1. All Respondents
2. Responses from "Attendee, neither Reviewer nor Presenter"
3. Responses from Reviewers
4. Responses from Presenters

1. All Respondents

1.1. What is your affiliation?

	Number of Responses	Response Ratio
U.S. federal government	18	9.4%
National/government laboratory, private-sector, or university researcher whose project is under review	44	23.1%
Non-government institution that received funding from the office or sub-program under review	42	22.1%
Non-government institution that does not receive funding from the office or sub-program under review	33	17.3%
Government agency (non-federal, state, or foreign government) with interest in the work	6	3.1%
National/government laboratory, private-sector, or university researcher not being reviewed	23	12.1%
Other	20	10.5%
No Responses	4	2.1%
Total	190	100%

"Other" Responses

- Industry
- Consultant
- Supplier and distributor
- HRS equipment manufacturer
- Reviewer
- Non-U.S. government organization who does not have funding from DOE
- Think tank in Japan
- Japanese company
- National organization in a foreign country
- DOE contractor
- Intern

1.2. Purpose and scope of the Annual Merit Review were well defined by the Joint Plenary Session (answer only if you attended the Joint Plenary on Monday).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	1	4	40	51
2%	1%	4%	41%	52%

16 Comments

- All speakers provided an outstanding overview of DOE efforts and the purpose of the AMR.
- Good information about the purpose of the AMR was presented at the plenary session and reinforced during review sessions.
- The Joint Plenary Session (Joint Plenary) did a good job presenting the variety of program areas that were going to be reviewed. The General Motors vehicles shown by Dr. Taub were especially enjoyable.
- A clear description of the purpose of the AMR was provided.
- The Joint Plenary helps principal investigators (PIs) at laboratories to see the big picture, where efforts are going, and how the different DOE offices are integrating their efforts to address energy issues.
- The Joint Plenary was well organized with a very-well-defined purpose.
- The overviews were useful; however, they are so compressed that it is hard to follow the details.
- The plenary speakers did a very good job of stressing the importance of this review meeting in deciding the fate of the projects that DOE funds. It would be nice if someone would include some real-world examples of how the AMR has actually led to changes in project priorities and/or changes in DOE funding priorities (without using specific PI/project names).
- It would be good to first show how the program aims to fulfill the goals of emissions legislation, and then how it aims to reduce imports of oil (unless emission requirements are on the same level of importance as reduction of oil imports).
- Speakers attempt to cover an enormous amount of information, and covering it all distracts from the high-level purpose and scope of projects supported (and how they tie together).
- Overviews need to be higher level and shorter in duration to allow overview presentations from similar programs in the Advanced Research Projects Agency – Energy (ARPA-E), National Science Foundation, the Office of Science, and a rollup of U.S. Department of Defense activities. The opportunity for cross-fertilization and reduction of redundant investments would be valuable to the overall research program and the individual researchers. Maybe the Joint Plenary could be devoted to a federal program overview, while the AMR sessions could be focused on Office of Energy Efficiency and Renewable Energy (EERE) projects.
- Including DOE Bioenergy Technologies Office (BETO) projects in the AMR is a good idea.
- It would be good to capture the whole supply chain for sustainable transportation; bioenergy is a part of that. Adding BETO to the review in future years seems like a good idea.
- BETO should be added.
- The AMR is already 4.5 days long. It is a bad idea to add BETO to this AMR. The whole AMR effort would be diluted too much.
- Adding BETO to the AMR is not favorable.

1.3. The two plenary sessions after the Joint Plenary Session were helpful to understanding the direction of the Hydrogen and Fuel Cells and Vehicle Technologies Programs (answer only if you attended either the Hydrogen and Fuel Cells or Vehicle Technologies plenary sessions on Monday).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	0	7	46	45
2%	0%	7%	46%	45%

10 Comments

- The Vehicle Technologies Office (VTO) overview presentation was very clear and gave a good sense of directions.
- For someone without much familiarity with the entire Program, these sessions were very informative.
- It was good to see a high-level overview of all projects.
- The Fuel Cell overview presentation was very good. The presenter of the Manufacturing R&D overview presentation did a great job relating her projects to the Advanced Manufacturing Initiative. This is a good way to leverage funding and promote research and development (R&D) that can help fuel cell commercialization. It would be nice if EERE technical development managers were more involved in the DOE Basic Energy Sciences and ARPA-E projects that relate to fuel cells.
- The VTO session helped to provide an overview of the directions of the Program.
- The presentations were interesting regarding progress since the last AMR. However, there was too much information on each slide.
- The presentation of the California fuel cell activities was quite interesting, but it is not clear if it helped in understanding the direction of the DOE Programs, as the survey question asks.
- It is unfortunate that they were parallel sessions and it was impossible to attend both. The limited time to cover the material prevented the briefers from providing all of the relevant information. Also, it was very unfortunate that the plenary briefs were not available on the CD or website, because they are most useful to this respondent's agency. The respondent would have taken more copious notes, because it is also not clear when those might be available.
- DOE's slide presentations in general are filled with way too much detail. It is impossible to read most of the text because it is so small, and the slides are really overcrowded. The slides become a distraction, rather than an aid to understanding the material. The presentation on the Fuels and Lubricants program was enjoyable. The slides were not overwhelming, and the presenter did a good job of presenting what the program does.
- Most everyone who comes to the AMR already knows the purpose and scope. Hence, there is a better use for this time.

1.4. The sub-program overviews were helpful to understanding the research objectives (answer only if you attended one or more sub-program overviews).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	2	4	63	58
2%	2%	3%	49%	45%

16 Comments

- The overviews were particularly useful for showing the overall relationships among projects in each technology area; this is something that can be lost in individual sessions. In addition, they provided an opportunity to hear the major thrust in all technology areas, which was helpful because attendees cannot be in all technology area sessions at the same time.

- The sub-program overviews were extremely useful.
- Presenters provided good insight to the research being funded and how it fit into the overall VTO mission.
- In all cases, the presentations made it clear what the research objectives were, and this helped to frame the respondent's appreciation for the project presentations.
- The Hydrogen Delivery sub-program overview provided good explanations of the goals and objectives of the sub-program.
- It was very informative hearing from the different program areas in VTO.
- Good information was provided in the VTO plenary sessions.
- It was interesting to see what other program areas within VTO were doing and how it is intended to mesh.
- It would be highly beneficial to have these overviews available on the CD or on the website (although having to download them individually is a big hassle) in time for the review.
- The presentations were interesting regarding progress since the last AMR. However, there was often too much information on each slide. Connecting the sub-program overviews was really appreciated and should be repeated at the next AMR.
- Including the presentation materials on the CD would be appreciated.
- The sub-program overviews were well placed; however, there still needs to be a little time for this at the start of the project review sessions throughout the week, because the topics covered in the sessions should be aligned with DOE goals.
- The presentations were more of an overview of the sub-programs than about objectives.
- DOE should continue to evaluate sub-programs.
- It is not always clear how specific objectives tie into the larger goal of technology commercialization and what the path toward commercialization is.
- There was a range in how the respondent would rate the sub-program overviews (some would have been rated as "agree" and some as "neutral”). Some of the presentations could have been more specific with respect to describing research objectives.

1.5. 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	88	46.3%
Presenter of a project	53	27.8%
Peer Reviewer	46	24.2%
No Responses	3	1.5%
Total	190	100%

2. Responses from "Attendee, neither Reviewer nor Presenter"

2.1. The quality, breadth, and depth of the following were sufficient to contribute to a comprehensive review:

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Presentations	0	3	3	51	26
	0%	4%	4%	61%	31%
Question and answer periods	0	2	10	46	25
	0%	2%	12%	55%	30%
Answers provided to programmatic questions	0	2	17	50	14
	0%	2%	20%	60%	17%
Answers provided to technical questions	0	1	12	44	25
	0%	1%	15%	54%	30%

9 Comments

- Using the same format for each presentation made it easier for the reviewers to "check off" the topics that needed to be covered by the presenter. Many question-and-answer (Q&A) periods were cut short. Adding 2–3 more minutes to these periods could be useful.
- Questions were mostly technical in nature and primarily from the reviewers.
- Some project presenters were posed with programmatic questions that should have been directed to the DOE lead. This is to be expected, considering that the programmatic overview happened two full days before the project presentations from that sub-program occurred.
- The time for Q&A seemed to be less, and hence many times there were only few questions asked. There does not appear to be a follow-up strategy for the questions asked.
- The general public was invited to attend the review; however, the information was not presented clearly enough to be easily understood by the general public. Sometimes, a 1–2 minute introduction explaining the goals and a description of the overall general topic would enhance understanding of the scope and details of the project. It is understood that the presentation was mostly targeted at reviewers who are familiar with all aspects of a given project, but then it is not clear why the general public is invited.
- Programmatic input should be provided to reviewers electronically, and not presented during the presentation. The presenters should focus more on the technical accomplishments of the projects and their impacts.
- One of the high-profile presentations seemed to side step the hard questions.
- Some sessions did not have enough time for Q&A.
- It is difficult to cover the breadth and depth of deployment projects with a structured presentation template that is identical for every project.

2.2. Enough time was allocated for presentations.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	2	6	49	27
0%	2%	7%	58%	32%

7 Comments

- The DOE briefing format, with key information (e.g., project start/end, funding, partners, and barriers) presented first ensures that those items are covered with adequate additional time/slides for technical depth.
- The time for each presentation was very well controlled. None of the presentations appeared rushed at the end.
- Presentations were about the right length. In some cases the time provided for Q&A (10 minutes), did not allow all questions to be asked.
- It is not clear whether a standard template was provided, because there was considerable variability. Most of the presentations were quite good, but not all used the same format, which would have been helpful.
- There could be a lot more valuable, in-depth information shared if presentation time slots were longer. Although Q&A time was sufficient for most, there were definitely some presentations that evoked a lot of questions and could have used more time for Q&A.
- More time would have been nice, but this has to be balanced against the fact that the AMR is already five days long.
- For larger projects ($2 million/year or greater), a longer presentation and Q&A time would be beneficial.

2.3. The questions asked by reviewers were sufficiently rigorous and detailed.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	7	13	47	16
0%	8%	16%	57%	19%

5 Comments

- Reviewers did an excellent job challenging the presenters on various aspects of their research.
- Several sessions only had two reviewers and the bulk of the Q&A time went to the audience.
- In many presentations there were no reviewer questions.
- Sometimes reviewers did not seem too knowledgeable about the subject matter, which would then be a disadvantage in terms of the comments, either positive or negative.
- Often questions were very good, but frequently reviewers seemed to ask irrelevant questions that sort of played to their own particular interests/expertise.

2.4. The frequency (once per year) of this formal review process for this Office or Program is:

	Number of Responses	Response Ratio
About right	74	38.9%
Too frequent	3	1.5%
Not frequent enough	1	<1%
No opinion	6	3.1%
No Responses	106	55.7%
Total	190	100%

5 Comments

- The frequency of the review is very appropriate. A shorter time between reviews would cause too much repetition, and a longer time would not allow for adequate frequency of input.
- When projects only last for 12–18 months, having an annual review is the only way to capture feedback regularly.
- An annual review with this mix of participants is valuable in assessing progress, sharing knowledge, and facilitating collaboration to address challenges and keep projects on track. It is valuable to conduct this type of review meeting annually. A formal review (peer review with resumes, documented and collated

comments, subsequent PI responses, etc.) every year may not provide the best return on investment, given the significant additional logistics. A formal review every two or three years might be more effective.

- It would be more helpful to have a midterm review on-site.
- A biannual review would be sufficient.

2.5. Logistics, facilities, and amenities were satisfactory.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	2	4	32	46
0%	2%	5%	38%	55%

21 Comments

- *From two respondents:* The hotel was expensive.
- The facilities and logistics were outstanding!
- The facility was excellent in every way.
- The hotel and general area are perfect for the VTO AMR.
- The hotel was very nice, and the food was particularly good. This meant the respondent did not have to go out and get food, and thus could spend more time getting in touch with folks and listening to presentations.
- This year’s location was much better than previous years, and it was good to have it all in one hotel.
- The provided meals and refreshments were outstanding.
- The facilities were very nice.
- Having the AMR in one hotel, rather than two, is preferred. It made it much easier to connect with colleagues.
- The Wardman Park location was greatly appreciated. Not having to hop between hotels makes the meeting much smoother.
- This location is better than the Crystal Gateway.
- The location, rooms, food, and the Marriott hotel in general were excellent.
- The new venue is good.
- It was a little expensive, although it was good.
- A location with cheaper parking would be appreciated.
- Wi-Fi service is requested.
- Parking was expensive for those who did not have access to the Metro.
- The accommodations were very expensive. A bottle of water cost $4; a beer cost $8. It might be better if this were held in Baltimore.
- This was a difficult location with few alternatives.
- The Crystal City location is better.

2.6. The visual quality of the presentations was adequate. I was able to see all of the presentations I attended.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	6	16	38	23
2%	7%	19%	45%	27%

19 Comments

- *From five respondents:* It was a little difficult for the people sitting in the back of the giant, long rooms to see the presentations.
- *From two respondents:* The majority of the presentations were easy to see, and only a few needed to have the projection screen a little higher.
- *From two respondents:* Some rooms were long and narrow. The screen must be on a platform in all of the rooms for those more than halfway back to read.
- Some of the conference rooms were long, making the presentations hard to see unless sitting very close. Also, some rooms had seating along the walls, which then made it impossible to see the screen at all.
- Some presenters used fonts that were too small to read, but most presenters used good visual aids.
- This respondent had some trouble seeing some of the presentations, despite sitting fairly close to the front for most. The reviewers' computers got in the way. It might work to have the screen set off to one side, with the reviewers on the other side.
- The fonts on slides were too small to be seen clearly. This respondent was looking at his laptop monitor all the time. However, it was wonderful and really helpful that all the presentation slides were provided to attendees.
- It was sometimes very difficult to see the details of the slides, especially when they were too busy. For people with some hearing difficulties, it was very hard to understand what was being presented and discussed.
- Some presenters try to put too much information on a single slide, making it difficult to read and comprehend all of the information in the time it is on the screen. This is even more difficult for those sitting in the back of the room.
- The screens in some of the meeting rooms were set up in a corner and were difficult to see from some angles.
- Too many speakers apologized because a particular slide was difficult to read. In addition, the room geometry did not always work well for speakers.
- Screens were too small to see any details, or too much information was presented on each slide to be useful.
- The bigger rooms made it hard to see.

2.7. The audio quality of the presentations was adequate. I was able to hear all the presentations I attended.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	4	3	40	37
1%	5%	4%	47%	44%

7 Comments

- *From two respondents:* The sound quality was excellent.
- As long as presenters used the microphones, the audio was fine. Some presenters elected to walk around and not use the microphones from time to time.
- The audio was acceptable—it could have been louder.
- Some presenters would have benefitted from lavaliere microphones because they had a difficult time speaking into the microphone or moved around.
- Sometimes it seemed like the microphone on the podium was not turned on. Also, many speakers did not focus on speaking into the microphone. All this led this respondent to attempt to sit near the front of the rooms.
- In some cases, the speaker could not be heard clearly. When coupled with the fact that the screens were small or too much information was on each slide, the talks became very cumbersome.

2.8. The meeting hotel accommodations (sleeping rooms) were satisfactory.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	19	23	26
0%	1%	28%	33%	38%

7 Comments

- The accommodations were excellent.
  The accommodations were a little expensive, although they were good.
- This was a great hotel for this meeting. It was a little too expensive, but the quality was outstanding.
- The room was good, although it was noisy outside.
- The hotel costs were incredible—not just the room costs, but also the parking cost and the cost of connecting to the Internet. This respondent travels quite a bit, and this is the first hotel that he has ever stayed in that wanted to charge for an Internet connection.
- This respondent was in a first-floor room in the Wardman Tower, and there was construction taking place directly above the room beginning before 7 am.
- The hotel seemed to be a bit expensive compared to the comfort and the amenities provided.
- The rooms were gone too fast.

2.9. The information about the Review and the hotel accommodations sent to me prior to the Review was adequate.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	8	34	39
0%	1%	10%	41%	48%

2 Comments

- Information on the review was very comprehensive. This respondent does not recall receiving anything on hotel accommodations.
- This respondent was informed one week before the review that he was not needed as a reviewer. This is a little late. He had already booked a couple of days to attend.

2.10. What was the most useful part of the review process?

51 Responses

- *From nine respondents:* The opportunity to meet and network with other participants.
- *From five respondents:* The Q&As after each presentation.
- *From five respondents:* The presentations.
- *From four respondents:* Getting updates on R&D projects and results.
- *From three respondents:* The Program and sub-program overview sessions are very helpful in setting the direction, providing the big picture of the current technology status, and providing background for the project presentations.
- *From two respondents:* The poster sessions.

- Bringing government, researchers, technologists, and industry together in a single platform is very useful. It gives a good orientation and vision of how the Program is progressing. In particular, the way the sessions are organized to move from broad overviews to detailed project presentations is a good approach.
- The actual presentations and Q&A, plus the time after the presentation to get to know some of the reviewers and attendees and to talk more about this respondent's project.
- The ability to get an overview of the research activities supported by both the VTO and Hydrogen and Fuel Cell Programs.
- Learning the true status of projects, not the typical story presented to the public.
- The technical descriptions in oral and poster presentations and the ability to meet and network with presenters.
- The face-to-face time with award recipients. In addition, the ability to hear questions from the attendees and reviewers helps attendees better understand the recipients' perspectives.
- Meeting up with everyone, seeing what researchers are working on in batteries and fuel cells and vehicle modeling.
- The different subjects that were discussed were varied and interesting.
- Having the same location for both the Hydrogen and Fuel Cells Program and VTO.
- The presentations and Q&A sessions, as well as hallway/break/meal discussions.
- Hearing details of the projects, and hearing how PIs speak about their work.
- The information on the projects' progress and the ability to talk to the presenters in the poster sessions.
- This is a one-stop shop for high-level overviews and deep technical talks.
- The sub-program overviews right at the beginning of the session.
- The electronic data of the presentation documents being distributed in the DOE AMR meeting place.
- The information about the market development of hydrogen technologies.
- The information exchange and the contributions (experience/knowledge) of the reviewers.
- It seems that the process is trying to get the national laboratories more engaged with industry.
- Having the recipients present the status and challenges of their projects.
- The Keynote and Joint Plenary sessions.
- The good technical discussions.
- Getting information on fuel cell and vehicle applications.
- Learning about future funding opportunities.

2.11. What could have been done better?

35 Responses

- *From two respondents:* Not much could be improved. The review seemed well planned and executed.
- *From two respondents:* More time is needed for poster sessions.
- *From two respondents:* There should be longer break times to allow more interactions among the attendees.
- The DOE AMR offered information that will be useful in determining future directions for the development of this respondent's company.
- DOE has done a great job.
- The AMR is already at a very high level. If something were to be improved, maybe the reviewer questions should be more oriented to the DOE barriers and targets.
- One negative is the industry partners having to pay for the national laboratories. National laboratories should be funded separately by DOE without industry support. The current structure makes it a disincentive for the industries to work with the national laboratories.
- Requiring presentation attendees to wait until a presentation (Overview and Q&A) is over to enter/exit the presentation room would decrease distractions. Additionally, prohibiting typing on a laptop during a presentation would also decrease distractions and would provide respectful attention to the presenter(s).
- Speakers should be encouraged to give 1–2 minute general introductions on why their project is important. This could involve providing a background of their technology and what they are trying to improve overall, rather than what they are improving based on previous years.
- The format for each presentation is still too rigid. Even though speakers have 20 minutes, several of them have commented that it feels they have only half that, given the prescribed structure of the talk.

- DOE should remove all side meetings and keep this dedicated as a review meeting. It is getting too crowded with side meetings that take people out of the reviews.
- This respondent received material on Monday, and it was really useful. Receiving it one week earlier would be more beneficial for study and preparation.
- A significant amount of work is missed from the time recipients' submit their presentations and the date of the AMR.
- Maybe there were too many topics and subjects; the attendee could not limit his or her time to a select few to analyze.
- The DOE Program/sub-program managers could have done even better jobs of providing the background information to set the stages for the project presentations.
- The quality of information provided during the presentations was difficult to ascertain because of the audio-visual concerns.
- There should be more electrochemistry presentations. Perhaps two rooms could be used simultaneously because there are many good posters that are not presented.
- There should be more focus on accomplishments and impact. DOE should provide the programmatic information on the projects to the reviewers electronically.
- Many of the projects were not new, and many of the researchers appeared to ignore much of the literature in the area of interest.
- The project objectives should be on the first slide of the presentations, rather than launching into the details of cost, etc.
- The event is a bit jammed! The suggestion of adding BETO might push it to the breaking point.
- For the lunch exercise, DOE should have displayed the topics more prominently at each table.
- There should be more information on future funding opportunities.
- There should be at least one good technical question for each presenter.
- There should be directions to find the rooms, although the people helping with that were very helpful.
- Using a USB drive instead of a CD would be better.
- There should be more critical review—"marketing" efforts should be resisted.
- Attendees need to have the plenary and overview talks available in advance of the review.
- The event should be at a lower-cost hotel.
- More information should be provided on the market effects and the infrastructure.
- The presentations could be done better.
- Breakfast could be improved.

2.12. Overall, how satisfied are you with the review process?

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	4	51	27
0%	0%	5%	62%	33%

4 Comments

- This is the first time this respondent attended this meeting. It was very well organized and efficient. The big picture and detailed research presentations are balanced. It has been a good learning experience.
- This is a good meeting. It allows for good networking, including with fuel cells stakeholders. The subject of national laboratories or universities is getting more like industrial company subject. Hope they stay more in scientific approaches.
- This meeting is well worth attending.
- It would be nice if the DOE program and sub-program managers made more effort to be available to stakeholders.

2.13. Would you recommend this review process to others, and should it be applied to other DOE programs?

	Number of Responses	Response Ratio
Yes	76	40.0%
No	3	1.5%
No Responses	111	58.4%
Total	190	100%

6 Comments

- The nature of the review process should depend on the type and scope of projects. This format is probably sufficient for small, applied projects. It is not clear whether the format allows a sufficiently thorough review of larger projects, given the time limitations of the presentations, as well as the reviewers' schedules. The format may not be appropriate for other DOE programs, such as those with basic science projects.
- DOE persons should also be involved in the review. This may be a separate group that does not talk with the reviewers during the review process.
- It would be good to see an Advanced Manufacturing Office annual merit review.
- Each program should design its own review process to meet its needs.
- Yes, but with more rigor.
- There should be more reviewers.

2.14. Please provide comments and recommendations on the overall review process.

17 Responses

- The management of the meeting was perfect. DOE deserves many thanks. This respondent is looking forward to next year.
- This is a highly unique forum not only because it involves reviewing the DOE programs, but also because of the high number and quality of scientists and engineers brought together in one place, which facilitates discussions that would not otherwise occur.
- The review process was well executed, and there was plenty of opportunity to interface with the PIs, especially those at the poster sessions.
- This is a very good event.
- The location in Washington, D.C. was better than in Arlington, Virginia.
- The review process has come a long way to become transparent. It is not perfect yet, which is quite acceptable. At the same time, the process by which the projects are selected remains opaque—in fact, more so than before. The process for selecting the reviewers and, worse yet, the quality of the feedback provided to the unsuccessful proposals is poor.
- This is a great process for everyone to see what is going on with DOE funding. One thing that is missing is cross-pollination. Maybe this does not occur because teams view each other as competitors. Cross-pollination seems like a good development, so DOE may want to think about how to achieve it.
- This looks like a transparent process. Some statistics on how reviewers' comments are taken into account (e.g., continue/discontinue/modify projects) would be interesting.
- If BETO is added to future reviews, DOE should hold one review on vehicle electrification (i.e., batteries, motors, power electronics, and fuel cells), and the other review on biofuels and engines.
- If the agenda includes industrial subjects, some reviewers with experience in large industrial settings should be selected.
- Presenters are sometimes allowed to avoid difficult questions—researchers appear more interested in presenting positive results and "winning" than true inquiry.
- Each sub-program team leader needs to specifically discuss the reviewer comments with the PI or project manager. It does not appear that this is systematically accomplished.

- The DOE AMR should distribute the electronic data of the presentation document on a website a few days earlier to deepen understanding of the audience.
- A couple of reviewers were late to the sessions this respondent attended. DOE should stress the importance of reviewers being in their assigned rooms early.
- The review is expensive and time consuming. A biannual review would be adequate.
- A strategy must be in place to follow up on the Q&A sessions.
- BETO would not be a fit at this time.

3. Responses from Reviewers

3.1. Information about the sub-program(s)/project(s) under review was provided sufficiently prior to the review session.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	3	17	25
0%	0%	7%	38%	56%

9 Comments

- There is a well-documented process for review, and the online refresher tutorial was helpful prior to the AMR.
- Reviewers were able to review project information for the reviewed projects prior to the meeting. The website had big-picture information available for those who wanted it.
- Being able to review the presentations ahead of time was very beneficial—it allowed reviewers to concentrate on the speakers and the material being presented. Reviewing the sub-program overviews beforehand helped, but they may not have been necessary to review the projects.
- Those organizing the meeting have the process well in hand.
- Oak Ridge Associated Universities made all presentations available to reviewers. It would have been nice to also have the DOE presentations in advance.
- The presentations from the previous year and the current year were provided; it may be useful to have a brief outline of the original proposal.
- Providing the information one week earlier would have been appreciated.
- Receiving the information a little earlier would have been better, but it was acceptable.
- For most of the projects this reviewer was asked to review, there was not much additional information beyond the presentation (most of them were also rather new projects and/or not reviewed in 2013).

3.2. Review instructions were provided in a timely manner.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	1	11	33
0%	0%	2%	24%	73%

9 Comments

- *From three respondents:* The instructions and Q&A in the webinar the week before the meeting were very helpful.
- *From two respondents:* The timing of the tutorial webinar allowed enough time for reviewers to experiment with the system and get questions answered.

- It was good to have someone affiliated with the review process in the reviewer room to help with logging in and answering questions about the review website. This person was also able to quickly provide paper forms for reviews, if desired.
- There were good instructions and adequate timing.
- The reviewer instructions were fine.
- An orientation for new reviewers (beyond how to use the review website) would have been helpful. This reviewer figured it out but would have preferred to have been better prepared.

3.3. The information provided in the presentations was adequate for a meaningful review of the projects.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	1	7	23	13
2%	2%	16%	51%	29%

20 Comments

- Most presentations followed the proposed outline and were easy to review.
- Past years' presentations are especially helpful for those who have not been tracking the project they are asked to review.
- Most presentations were very good, but the newer projects needed to concentrate more on the approach and plans rather than the accomplishments.
- Generally, yes, it was adequate, although there were obviously varying levels of detail provided across all the presentations. Some provided useful roadmaps to answering evaluation questions, while others left a lot to be gathered "between the lines."
- Generally yes, it was adequate, but the level was not fully consistent. Some projects had more useful information than others. The comments from previous reviewers were very helpful.
- The information was usually adequate, especially with the provided backup slides. .
- Presentations were informative and fit the time allowed. Some slides contained too much detail.
- Most presentations were very good, but some with lots of progress/data were pressed for time and had to leave some information out that had to be asked about in the Q&A time.
- Presenters probably did as well as can be done in the time available.
- Providing technical information in this presentation format limits the ability of the reviewer to gain insight that may provoke more meaningful feedback, both during and after the actual presentation by the researchers. A technical paper that accompanies the presentation would be helpful to provide additional details.
- Even though the presentations covered the objectives of the projects and how they address the barriers, it would be useful to see the evolution of the project. For this to happen, the original proposals would need to be reviewed. As the projects are carried out, the direction set in the original proposals may be varied, and it will be difficult to judge progress from only the presentations.
- Actual project activities were frequently well beyond the material provided for review and were sometimes significantly different from the material presented. Because investment decisions are meant to be the result of reviewer remarks, the disconnection between actual activities and reported activities could make those decisions inappropriate.
- It is extremely difficult to review a project based only on the presentations submitted. There should be guidelines for the PIs on what information should be on the slides. Additional text would have been valuable to understand major assumptions, calculations, methodology, and results.
- Sometimes it was difficult to determine why certain decisions were made in the project. When presenters were asked for more detail, often the answer (particularly in cooperative research and development agreement [CRADA]-related projects) was that this was proprietary information.

- It is unreasonable to expect presenters to share enough information in a 20-minute presentation to allow review of a $100 million project. That is $5 million per minute.
- The technical content provided in the American Recovery and Reinvestment Act project presentations was minimal.
- The presenters were required to squeeze too much information onto the first chart after the title slide.
- Most presenters did a good job, but it was hard on some of the longer or newer projects because they either had too much information on the charts or not enough detail. In all cases, it was possible to glean enough to evaluate progress.
- Some timing charts were hard to follow—it would be better to have a standard format such as a Gantt chart or some other standard. The location was hit or miss, but it is not necessarily overly important unless it is indicative of collaboration among team members.
- Not all of the presenters gave their milestone schedules. Milestone schedules must be required.

3.4. The evaluation criteria upon which the review was organized (see below) were clearly defined.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	0	1	2	25	17
	0%	2%	4%	56%	38%
Approach	0	0	1	28	15
	0%	0%	2%	64%	34%
Technical Accomplishments and Progress	0	1	0	24	19
	0%	2%	0%	55%	43%
Technology Transfer and Collaboration	0	1	2	24	18
	0%	2%	4%	53%	40%
Proposed Future Research	0	0	5	25	15
	0%	0%	11%	56%	33%

11 Comments

- It helped to have clearer explanations on the evaluation form of what each score would mean, and it was also good to add the half-point levels.
- The list of evaluation criteria was very clear; this reviewer has nothing to add.
- Most presentation slides had titles that helped track the content being presented to the evaluation criteria. New projects had some minor issues, but one expects them to get better—they were mostly first-time presenters.
- It was confusing to hear about the project barriers before knowing the project objectives. The objectives were also sometimes hidden in the approach slides. In addition, the titles of the projects are sometimes completely different than the objectives, approach, or results. It would also be good to add an “overall comments” section to allow more general observations, such as oral presentation effectiveness/ communication or impressions about the projects.
- This reviewer is not sure that collaboration needs to be a criterion. It might be a good thing to encourage in general, and certainly in specific cases, but other times none is needed, and the project should not be rated down because of that.
- The technical accomplishments criterion is still too vague. For a number of projects, the accomplishments are just completing the last few months of research or catching up with the milestone schedule. In addition, not all presenters provided a milestone schedule. This should be required.
- The Relevance section may not be necessary, because the proposals have been prepared to address specific targets; their usefulness to petroleum savings has been reviewed and approved, so relevance may not be useful as an evaluation criteria.

- Some presentations did not follow the required outline, and some information required for the review was missing. DOE managers must ensure that all necessary slides are included.
- For projects that at least nominally aim to address future commercial prospects, perhaps evaluation criteria to assess this aspect should be included.
- Some projects that recently started had very limited progress/accomplishments. Proposed future research is also difficult to assess when projects are just getting underway.
- There should be a "Not Applicable" option for the Future Research question for completed projects.

3.5. The evaluation criteria were adequately addressed in the presentations.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	0	1	9	25	10
	0%	2%	20%	56%	22%
Approach	0	0	4	29	11
	0%	0%	9%	66%	25%
Technical Accomplishments and Progress	0	0	4	23	18
	0%	0%	9%	51%	40%
Technology Transfer and Collaboration	0	2	5	29	9
	0%	4%	11%	64%	20%
Proposed Future Research	0	2	6	28	9
	0%	4%	13%	62%	20%

13 Comments

- On average, the PIs did an excellent job of addressing the criteria.
- Everyone had standard slides on these topics, so the information was available. Some presentations missed commenting on the real impact of their work. Yes, accomplishments and progress were reported, but no one shared excitement about how those translated into real-world impacts. It would be good to hear things like "people have been calling DOE for guidance," "industry is applying these lessons learned and seeing results," or "the results are really changing the way..."
- Many of the projects that this reviewer reviewed did not include the big-picture application—how the investigators hope to implement the work. It would have been good to have a little more time spent on the relevance of the work.
- Most were clear; however, technology transfer/collaboration information typically included identifying partners, but not how they were involved or what was going to be transferred. Even a high-level description would help.
- Some presentations did better than others. Most projects addressed the criteria toward the front of this list (relevance, approach, accomplishments) better than the back.
- It is hard to answer this question, given that each presentation had a different level of detail.
- Some did very well, while others did not, but information was presented that allowed the projects to be evaluated based on the criteria.
- There was a wide range—some presentations were excellent, and others were borderline. This reviewer's rating of "agree" is an average figure.
- This was variable among the projects; therefore, it is difficult to answer this question in a general way.
- This varied (which is to be expected, given the different styles).
- The time devoted to proposed future work often got trimmed due to lack of time in unrehearsed talks.
- Technology transfer was not always clearly addressed.
- Not all the presenters agreed in their interpretation of the criteria.

3.6. The right criteria and weightings were used to evaluate the project(s).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	0	1	4	27	13
	0%	2%	9%	60%	29%
Approach	0	0	2	26	17
	0%	0%	4%	58%	38%
Technical Accomplishments and Progress	1	0	4	24	16
	2%	0%	9%	53%	36%
Technology Transfer and Collaboration	1	4	3	24	13
	2%	9%	7%	53%	29%
Proposed Future Research	0	0	7	27	11
	0%	0%	16%	60%	24%

9 Comments

- This reviewer is not sure how DOE could improve the weightings because an "apples to apples" comparison is required. However, some projects are finishing and others are just getting underway, making it difficult to assess accomplishments and future research of projects at these different stages. If possible, variable weightings based on project progress should be considered.
- Sometimes a project is important for other reasons than relevance to the mission of DOE. Somehow, such reasons should be accommodated. Also, not all projects have aspects that are immediately amenable to technology transfer—this criterion should be revised to "future applicability of a concept or principle as well as a technology."
- Relevance may not be a proper review criterion. Of course these projects are relevant, and the question is provided as only a "yes or no" question. If DOE is trying to figure out which projects are most relevant, then it should ask that (with a scale). More specific questions (or at least a question) are needed on how the project is going and how well it is being managed. The only place to discuss that now is buried in Accomplishments and Progress. Management (schedule) is often where projects struggle.
- DOE might consider having some different criteria for science-based projects than for projects dealing with technology assessment, demonstration, or tangible (engineering) deliverables. Whether milestones for scientific projects are met is an artificial metric and often does not reflect the real value of the work.
- It did not seem like future plans had been analyzed in terms of their chance of achieving ultimate success for the project, just in terms of what would be good to do next.
- Technical accomplishments and progress should not be 45% of the weight. A PI could be on target for a project that is not meaningful. Impact should have a higher weight.
- Weighting and scoring were not too important. This reviewer made sure that projects that did not seem useful had a low score and those with a big potential/impact had higher scores.
- Technology transfer is the underlying purpose of these projects—if it does not get into industry, it is not going to help anybody.
- Getting the information/technology into the hands of consumers is the goal—it is important that this is done. Accomplishments and plans are very good, but the goal is production!

3.7. During the Annual Merit Review, reviewers had adequate access to the Principal Investigators.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	3	22	20
0%	0%	7%	49%	44%

11 Comments

- Reviewers were treated like rock stars!
- Giving the reviewers first priority for asking questions is good. The only issue was if the PI was not there, but the stand-ins did a good job.
- The process of allowing reviewers' questions to be addressed first worked well.
- The PI was in the room, so if there was a need to speak to him or her, it would have been possible.
- This is one of the benefits of the AMR.
- There were no issues.
- There was enough access during the Q&A sessions, but not enough access for any follow-up questions.
- In addition to reviewing a session, this reviewer was an audience member during a separate, earlier session, when a question was taken from the general audience before the reviewers got a chance to ask a question. Clearly, this was an exception to practice and probably little more than an honest mistake by the session chair. This reviewer does not recall ever seeing this in earlier AMRs.
- There is not enough time during one poster session to talk with 6 to 8 PIs. Reviewer assignments need to be split between poster sessions and oral presentations.
- This issue gets to the insufficient time/depth of questions; a smaller meeting with more in-depth review would be much better.
- This reviewer did not have enough time to ask all of his questions after the presentation, but he caught up with one of the PIs afterward.

3.8. Information on the location and timing of the projects was adequate and easy to find.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	4	18	22
0%	2%	9%	40%	49%

10 Comments

- This is a super-well-run event!!!!
- The location this year was so much better than last year! It was easy to get to all the sessions from the hotel.
- There was perfect organization.
- The organizers have this down.
- The timing was acceptable, but the location was not. The location should have been better advertised because it was not in the same place as the previous AMR. The Crystal City location is preferred.
- This reviewer assumes this question means "presentations" instead of "projects."

3.9. The number of projects I was expected to review was:

	Number of Responses	Response Ratio
Too many	5	2.6%
Too few	5	2.6%
About right	35	18.4%
No Responses	145	76.3%
Total	190	100%

9 Comments

- *From two respondents:* The number was acceptable for this meeting because the reviewer was primarily interested in the projects that he was reviewing. However, in future meetings it might be too many, especially if the reviewer is assigned projects in other topic areas.
- This reviewer appreciates DOE not scheduling back-to-back reviews, which allows time for evaluation.
- This reviewer was able to switch her block of presentations to ones she was more interested in reviewing; she appreciates that last-minute flexibility.
- This reviewer had three reviews, which was appropriate.
- Because this reviewer was attending locally, this was not a problem, but if he had to travel to do the reviews, he would have needed more than two projects to review to justify the trip.
- This reviewer reviewed perhaps a few more projects than desired. If the reviewer had not conflicted out on several, it would have been many more than desired.
- Reviewing 18 projects makes it hard to do an excellent and thoughtful analysis for each.
- This reviewer had six; four may be a better number.

3.10. Altogether, the preparatory materials, presentations, and question and answer period provided sufficient depth for a meaningful review.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	1	4	26	12
2%	2%	9%	59%	27%

12 Comments

- The format is appropriate; the content of the presentations can vary.
- For the most part, the process works for oral presentations. It is less clear whether it works well for the poster reviews; researchers often get locked into making their entire presentations, rather than simply responding to reviewers' questions. Yes, it is tough to present a poster; however, poster reviewers are usually trying to do multiple reviews simultaneously and are not planning to spend a half-hour at each.
- Most were very good, but it would have been good to see more information on how accomplishments are feeding into future work or impacting plans, such as changing them because of test results, etc.
- Yes, if a reviewer had reviewed the same project for multiple years; the depth was a bit lacking for new reviewers, but it was acceptable.
- There were a couple of presentations that would have benefited from a full hour, but most were able to address everything in the 20-minute session.
- There is never enough time to go into details, regardless of the time provided. Nevertheless, discussions were frequently cut off because of time limitations.
- A technical paper that accompanied presentations would be helpful to provide additional detail. This is especially true for reviewers to review in advance of the actual presentations.
- The materials were adequate, but they did not provide enough information at times.

- This was variable among presentations.
- Advance copies of the DOE presentations would have been helpful.
- Perhaps it was sufficient for small projects.
- The presentations did not provide enough information to do a fair review.

3.11. Please provide additional comments.

15 Responses

- This was a very interesting review regarding technical progress and personal exchanges. As usual, it featured excellent organization. Congratulations!
- This reviewer did not make it to the opening presentations on Monday and would have liked to have seen a summary page on the objectives of the AMR and how reviewers could contribute to the process. The reviewer attended the webinar, but that was more about the mechanics of being a reviewer. The reviewer appreciated the opportunity and will participate in 2015.
- It was great that the sessions started and ended on time or slightly early. The room setup was acceptable.
- It was a very busy week, but it was enjoyable.
- There was excellent time keeping in the sessions. The only concern is traveling between presentations if they are during the same break-to-break interval, but it is unclear how DOE would fix that problem. The facility and location were good, but some rooms were very cold.
- This reviewer was asked to review individual investigators for a project set up as a team effort. This is difficult to do and unfair to the individual investigators. DOE can still ask the investigators to present on different aspects of the project, but if significant collaboration is required, DOE should ask the reviewers to review the project as a whole.
- This may seem like a small point, but a 12-hour day of service and then having to get dinner afterward makes for a grueling week with little sleep. The snacks offered during the poster sessions were only good for people with no dietary restrictions. The AMR may be too big, and it is not clear whether adding BETO would be a good thing.
- The meeting is too big. Not only should BETO NOT be added, but VTO and the Hydrogen and Fuel Cells Program should get split back out. Having one in-depth meeting for each program area (e.g., Energy Storage) would be even better.
- It is unclear whether BETO should be added. It may result in an unmanageable event. It would also likely cause greater schedule conflicts for attendees and/or reviewers, because there may be some alternative fuel production and alternative fuel utilization (deployment) or R&D sessions up against each other. It is already difficult for some reviewers to attend all of the sessions they desire.
- It would be better to have two full weeks following the AMR to complete reviews, particularly because the review website was unavailable on the Sunday immediately following the AMR. Many reviewers are reviewing on their own time and need weekend time to complete the reviews.
- A major goal of DOE funding appears to be stimulating R&D activities. Research, by definition, also requires providing a fundamental picture. By contrast, a lot of reported findings lacked a clear explanation about the fundamentals, possibly because the PIs had never been asked to use first principles to describe their findings.
- Expanding the review by adding BETO is a bad idea. The meeting is already too large. Adding another Office's program area will limit site and scheduling options and make logistics for the meeting more difficult.
- The information should be sent a month before the AMR so reviewers have enough time to read all the presentations and prepare their questions.
- The bio-production of hydrogen is a field in its own right. The differences in technology and terminology are a reason not to include it in the currently constituted AMR.
- The presentations followed a similar format. It would be good (if not provided already) for the AMR to have a specific format. Some of the presenters put too much information on each chart. The presentation abstracts have a very nice and consistent format; it would be nice if something like this could be enforced at the AMR.

4. Responses from Presenters

4.1. The request to provide a presentation for the Annual Merit Review was provided sufficiently prior to the deadline for submission.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	3	1	22	24
2%	6%	2%	43%	47%

7 Comments

- The save the date was sent early enough to plan around, but the guidance for the presentation was not received far enough ahead to sufficiently draft the presentation in advance of the deadline. This reviewer wanted to begin drafting his presentation earlier, but he was told to wait until revised guidance was issued because of potential substantial changes.
- The request was sufficient, but the due date is still too far in advance to get the latest available information into the presentation.
- It is unclear why presenters have to submit their presentations so far in advance. By the time of the AMR, this presenter has forgotten what she wrote in March, and conclusions may have changed with the additional time and effort.
- The request might be too early. During the presentation, one had to go back in time to present based on where the project was when the presentation was created.
- Yes, it was requested in early April, and most of the presenters had to note during their presentations that the data was outdated.
- If the deadline is pushed back, more recent results can be included in the presentations.
- The presentation was due far earlier than necessary.

4.2. Instructions for preparing the presentation were sufficient.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	2	25	23
0%	2%	4%	49%	45%

8 Comments

- The instructions were helpful.
- The instructions were sometimes a little overly detailed.
- The guidance was thorough but confusing. Including a complete sample presentation as an additional file, not interspersed with other guidance, would be helpful.
- The amount of detail was confusing, but the examples were very helpful.
- Unfortunately, the amount of mandatory slides was increased for this year's AMR by making the presenters include slides addressing "Response to Previous Year's Reviewers' Comments" and "Remaining Challenges & Barriers." These are important slides and must be included; unfortunately, the time (20 minutes plus 10 minutes for Q&A) for each presentation was kept the same as in previous AMRs. This presenter was not able to include sufficient information for the Results/Accomplishments section.
- Directions changed after the submittal date, requiring revision of this presenter's presentation.
- The instructions were very lengthy, especially for a deployment project.
- Changes were made late into the process.

4.3. The audio and visual equipment worked properly and were adequate.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	1	21	28
0%	0%	2%	42%	56%

4 Comments

- It worked fine for this presenter’s presentation.
- Presenters should use lapel microphones instead of podium microphones.
- There were not any glitches except that two presenters had problems with “movies” embedded in their presentation files.
- The equipment did not work great for this presenter, but it seemed to for everyone else.

4.4. The evaluation criteria upon which the Review was organized were clearly defined and used appropriately.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	0	2	5	23	17
	0%	4%	11%	49%	36%
Approach	0	1	3	26	17
	0%	2%	6%	55%	36%
Technical Accomplishments and Progress	0	1	2	25	19
	0%	2%	4%	53%	40%
Technology Transfer and Collaboration	0	1	5	26	15
	0%	2%	11%	55%	32%
Proposed Future Research	0	2	7	23	14
	0%	4%	15%	50%	30%

5 Comments

- The criteria make perfect sense and are clearly defined; however, it does not seem like reviewers take into consideration whether the sequence and breakdown are followed or ignored in the material presented. It is more important to make sure that the presenter gets a chance to express the project’s relevance/approach/etc. in his/her own way than trying to make sure that all slides are formatted with a common template. The instructions for the template are too rigid in that regard.
- The structure is mostly good! However, PIs should also have a section where they can explicitly emphasize the innovation aspect of their project.
- The reviewers of this presenter’s project did NOT use the evaluation criteria appropriately. The questions and comments presented by the reviewers displayed a remarkable lack of qualification on the part of the reviewers and a remarkable focus on trying to discredit the project without reason.
- “Proposed Future Research” is unclear and does not apply well. “Relevance” is also unclear, as well as too broad; presenters should just be asked to state the applicable funding opportunity announcement to which the project applied.

- The guidance was generally confusing and dictated an order that was quite awkward for presenting and explaining the many facets of projects.

4.5. Explanation of the questions within the criteria was clear and sufficient.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	0	2	5	24	15
	0%	4%	11%	52%	33%
Approach	0	1	3	27	15
	0%	2%	7%	59%	33%
Technical Accomplishments and Progress	0	1	4	26	15
	0%	2%	9%	57%	33%
Technology Transfer and Collaboration	0	1	4	27	14
	0%	2%	9%	57%	30%
Proposed Future Research	0	2	5	23	15
	0%	4%	11%	51%	33%

3 Comments

- The explanation was very clear.
- The “Barriers” section is a good example of why the explanation of the criteria is invalid. The instructions clearly state that barriers should be taken from DOE’s list in published reports, but all reviewers have their own interpretation of how to pick barriers. This presenter was penalized one year for limiting barrier selection to the DOE report, so now the presenter follows the majority of other reviewers and selects more specific barriers—with better feedback from reviewers so far.
- It is unclear what “questions” this question refers to.

4.6. The right criteria and weightings were used to evaluate the project(s)/program(s).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance	0	0	10	23	12
	0%	0%	22%	51%	27%
Approach	0	0	9	25	11
	0%	0%	20%	56%	24%
Technical Accomplishments and Progress	0	1	7	23	13
	0%	2%	16%	52%	30%
Technology Transfer and Collaboration	0	1	12	23	9
	0%	2%	27%	51%	20%
Proposed Future Research	0	1	11	24	9
	0%	2%	24%	53%	20%

6 Comments

- There is always room for debate on weighting, because there are many different projects whose purposes do not align perfectly (nor should they be made to). Overall, the weighting seemed adequate.
- The criteria and weightings were acceptable.
- Reviewers this year seemed to be non-experts. Also, there was a person from Pacific Northwest National Laboratory sitting in the audience throughout the sessions whose role seemed to be to ask hardball questions to projects from other laboratories, although he was not a reviewer. This format seemed strange.
- More thought should be given to this weighting. For example, third-year projects should have less weight on future work and more on accomplishments. First-year projects should have more weight on future work and less on accomplishments. Similarly, technology transfer should not apply to all projects.
- Collaboration should have more weight in projects that provide substantial cost share. Keeping all collaboration partners happy and engaged takes a lot of effort.
- This presenter’s project contains five development approaches. It is not possible to accurately communicate all the work that has gone into the project in 20 minutes.

4.7. Please provide additional comments:

10 Responses

- The criteria for evaluation are well established. The flexibility that presenters received in creating their posters (slides or traditional poster style) was good. This presenter had no complaints.
- The meeting is good for networking and seeing what other people are doing; however, it would be more cost effective to have reviewers look over the material in a report rather than have everyone come to Washington, DC, for presentations.
- This was an excellent venue. It was overwhelming with so many people involved. There should be better food for the poster sessions. DOE should not add BETO.
- The cost of this review was much higher than it needed to be. The review should not occur in an expensive hotel in downtown Washington, DC. DOE should have used hotels perhaps 50 miles outside of Washington, DC, which would have cost much less and allowed more dollars to be used for research.
- Some of the reviewers do not understand the projects, as evidenced by their uninformed questions and assertive but unhelpful comments. Some comments are unclear, and there is no opportunity to clarify them. Some reviewers have their own agendas. However, this presenter understands that a systematic review is necessary and that DOE program managers are doing their best.
- After a talk, the most time for questions has always been given to the reviewers. The general audience has fewer opportunities to communicate with the presenters. The reviewers have multiple options to talk to the presenters before and after the presentations. DOE should leave more time for the audience to ask questions.
- Unfortunately, the amount of mandatory slides was increased for this year’s AMR by making the presenters include slides addressing “Response to Previous Year’s Reviewers’ Comments” and “Remaining Challenges & Barriers.” These are important slides and must be included; however, the time (20 minutes, plus 10 minutes for Q&A) for each presentation was kept the same as in previous AMRs.
- It would be helpful to all parties if the request for reviewer availability were made after the presenters know the date(s) and time(s) of their presentations. Doing the reverse forces potential reviewers to commit to review projects outside of their travel plans.
- It was unclear how many reviewers there were going to be or how projects were judged. This may have been included in the presentation template, but an email explaining the entire process would have been helpful.
- It was somewhat embarrassing to hear some speakers say that their slides were either old or not the latest version. It implied there were errors and/or omissions, which, in itself, gives a negative perception.